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ONE OF THE EDITORS OF THE CYCLOPÆDIA OF PRACTICAL MEDICINE.

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CONTENTS OF No. XV.

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British and Foreign Medical Review.

JULY, 1839.

PART FIRST.—Analytical and Critical Reviews.

PAGE

ART. I.—Histoire générale et particulière des Anomalies de l'Organisation chez l'Homme et les Animaux, ouvrage comprenant des Recherches sur les Caractères, la Classification, etc. des Monstruosities. Par M. ISIDORE GEOFFROY SAINT HILAIRE 1
A general and particular History of Anomalies of Organization in Man and Animals, comprising Researches into the Characters, Classification, &c. of Monstrosities. By M. I. G. ST. HILAIRE.

ART. II.—1. The Philadelphia Practice of Midwifery. By CHARLES D. MEIGS. 37
2. Mémoires de Médecine et de Chirurgie pratique sur plusieurs Maladies et Accidens graves qui peuvent compliquer la Grossesse, la Parturition, et la Couche. Précédés d'un Compte-rendu analytique de Maladies observées à l'Hospice de la Charité de Lyon pendant un Exercice de sept Années. Par le Docteur MARTIN, le jeune . . . . . ib.
Essays in Practical Medicine and Surgery, on various Diseases and important Circumstances complicating Pregnancy, Labour, and the Puerperal State, &c. By Dr. MARTIN, jun.

ART. III.—1. Recherches sur les Fièvres Intermittentes du Nord de l'Afrique. Par F. C. MAILLOT 56
Researches concerning the Intermittent Fevers of the North of Africa. By F. C. MAILLOT.
2. Traité des Fièvres ou Irritations Cérébro-spinales Intermittentes, d'après des Observations receuilles en France, en Corse, et en Afrique. Par F. C. MAILLOT . . . . ib.
Treatise on Intermittent Fevers or Cerebro-spinal Irritations, from Observations collected in France, Corsica, and Africa. By F. C. MAILLOT.
3. Beobachtungen und Untersuchungen über das Wechselfieber. Von Dr. KARL KREMERS 57
Observations and Enquiries on Intermittent Fever. By Dr. K. KREMERS.
4. Delle Malattie Periodiche e principalmente delle Periodiche Febbrili. Saggio Critico di PIETRO MANNI . . . . . . ib.
A Critical Essay on Periodical Maladies, and principally of Febrile Periodical Maladies. By PIETRO MANNI.

ART. IV.—1. Recherches Anatomiques et Physiologiques sur l'Organe de l'Ouie et sur l'Audition dans l'Homme et les Animaux Vertébrés, &c. Par G. BRESCHET 68
Anatomical and Physiological Researches on the Organ and Function of Hearing in Man and the Vertebrate Animals. To which is added the History of the Nervous Plexus of the Tympanum. By G. BRESCHET.
2. Handbuch der theoretischen und praktischen Ohrenheilkunde. Von Dr. CARL GUSTAV LINCKE . . . . . . ib.
Manual of Theoretical and Practical Ear-Medicine. By Dr. CHARLES GUSTAVUS LINCKE.
3. The Cyclopædia of Anatomy and Physiology, Parts XIV. and XV. . ib.
4. A Treatise on the Structure, Economy, and Diseases of the Ear, being the Essay for which the Fothergillian Gold Medal was awarded by the Medical Society of London. By GEORGE PILCHER . . . ib.
5. Handbuch der Physiologie des Menschen. Von Dr. JOHANNES MÜLLER . ib.
Manual of the Physiology of Man. By Dr. JOHN MÜLLER.

PAGE

6. Recherches Pratiques sur les Maladies de l'Oreille, et sur le Developpement de la Parole chez les Sourds-Muets. Par le Dr. Deleau, Jeune . 69
Practical Researches on the Diseases of the Ear, and on the Development of Hearing and Speech in the Deaf and Dumb. By Dr. Deleau, Jun.
7. Philocophus, or the Deaf and Dumb Man's Friend. By J. B. Bulwer . ib.

Art. V.—1. Some Enquiries in the Province of Kemaon, relative to Geology; including an enquiry into the causes of Goitre. By John M'Clelland . 103
2. Some Account of the Bronchocele of Nipaul and of the Cis and Trans-Himalayan Regions. By the late Mr. Bramley . . . ib.
3. Treatise on English Bronchocele, with a few Remarks on the use of Iodine and its Compounds. By Dr. James Inglis . . . ib.

Art. VI.—1. Cases and Observations, illustrative of Renal Disease, accompanied with the Secretion of Albuminous Urine. By Dr. Bright . . 121
2. De l'Albuminurie ou Hydropisie causée par Maladie des Reins, &c. Par le Dr. Martin Solon . . . . ib.
On Albuminuria, or Dropsy, caused by Diseased Kidney. By M. Solon.
3. Traité des Maladies des Reins et des Altérations de la Secrétion Urinaire, &c. Par P. Rayer . . . . ib.
Treatise on Diseases of the Kidneys and the Morbid States of the Urinary Secretion, &c. By P. Rayer.
4. On Granular Degeneration of the Kidneys, and its connexion with Dropsy, Inflammations, and other Diseases. By Dr. Robert Christison . ib.
5. Observations on Abdominal Tumours and Intumescence; illustrated by cases of Renal Disease. By R. Bright. . . . . ib.

Art. VII.—1. De penitiori Dentium Humanorum structurâ observationes. Auctore M. Fraenkel 158
Observations upon the internal Structure of the Human Teeth. By M. Fraenkel.
2. Meletemata circa Mammalium Dentium evolutionem. Auctore J. Raschkow. ib.
Researches respecting the development of the Teeth of Mammalia. By J. Raschkow.
3. Archiv für Anatomie, Physiologie, und Wissenschaftliche Medicin. Von Dr. J. Müller . . . . ib.
Archives of Anatomy, Physiology, and Medical Science. By Dr. J. Muller.
4. Microskopiska Undersökningar öfver Tändernes särdeles Tandbenets, struktur. Af A. Retzius . . . . ib.
Microscopical researches on the Structure of the Teeth, and especially of that of Tooth-bone. By A. Retzius.
5. On the Structure of the Teeth, the Vascularity of those Organs and their relation to Bone. By John Tomes . . . . . ib.
6. On the Structure of Teeth, and the resemblance of Ivory to Bone, as illustrated by microscopical examination of the teeth of man, and of various existing and extinct animals. By Prof. Owen . . . . . ib.
7. On the Origin and Development of the Pulps and Sacs of the Human Teeth. By John Goodsir, Jun. . . . . . . ib.
8. Elements of Physiology. By J. Müller, M.D. 159
9. Researches upon the Development, Structure, and Diseases of the Teeth. By Alexander Nasmyth. . . . . . . ib.

Art. VIII.—1. The Works of John Hunter, F.R.S.; with Notes by James F. Palmer. Vol. III., A Treatise on the Blood, Inflammation, &c. . 169
2. A Treatise on Inflammation. By Dr. Macartney . . . ib.
3. Illustrations of the Elementary Forms of Disease. By Dr. Carswell . ib.
4. Teoria della Flogosi, di Giovanni Rasori . . . . ib.
Theory of Inflammation. By J. Rasori.

Art. IX.—Der Schrägschnitt, eine neue Amputationsmethode, nebst Erörterungen anderer, die Amputationen betreffender Gegenstände. Von Ernest Blazius 205
The Oblique Section.—A new Method of Amputation, to which is added, an Enquiry into other Circumstances respecting Amputation in general. By Ernest Blasius.

Art. X.—1. Statistical Report on the Sickness, Mortality, and Invaliding among the Troops in the West Indies. Presented to both Houses of Parliament. 211

PAGE

2. Statistical Reports of the Sickness, Mortality, and Invaliding among the Troops in the United Kingdom, the Mediterranean, and British America. Prepared from the Records of the Army Medical Department and War-office Returns. Presented to both Houses of Parliament 211

3. A Letter to the Right Honorable the Secretary-at-War, on Sickness and Mortality in the West Indies, being a Review of Captain Tulloch's Statistical Report. By SIR ANDREW HALLIDAY, M.D. . . . ib.

ART. XI.—Illustrations of the Comparative Anatomy of the Nervous System. By JOSEPH SWAN 226

ART. XII.—Lectures on Diseases of the Eye. By JOHN MORGAN . . 229

ART. XIII.—Observations on the Oriental Plague and on Quarantine, &c. addressed to the British Association of Science. By JOHN BOWRING . . 233

PART SECOND.—Bibliographical Notices.

ART. I.—An Account of some New Instruments for Tying Polypi of the Uterus, Nose, and Ear, and enlarged Tonsils; with Cases. By WILLIAM BEAUMONT 237

ART. II.—Illustrations of Cutaneous Diseases. A series of Delineations of the Affections of the Skin, in their more interesting and frequent forms; with a Practical Summary of their Symptoms, Diagnosis, and Treatment. By ROBERT WILLIS, M.D. 239

ART. III.—Practical and Surgical Anatomy. By W. J. ERASMUS WILSON . 240

ART. IV.—Outlines of Human Physiology. By WILLIAM PULTENEY ALISON . 241

ART. V.—Over de Overeenkomst en het Verschil Tusschen de Jicht en de Scrophulosis, vooral met Betrekking tot de Longetering; eene Voorlezing door A. A. SEBASTIAN 242

On the Relations and Differences between Gout and Scrofula, with reference especially to Consumption. By A. A. SEBASTIAN.

ART. VI.—Vegetable Organography; or an Analytical Description of the Organs of Plants. By M. A. P. DE CANDOLLE. Translated by B. KINGDON . 243

ART. VII.—Diet and Regimen, Physical, Intellectual, and Moral, as Means in the Prevention and Cure of Disease. By ROBERT DICK . . . ib.

ART. VIII.—A System of Anatomy for the Use of Students of Medicine. By CASPAR WISTAR; with Notes and Additions, by W. E. HORNER . 244

ART. IX.—The Surgical Anatomy of the Perineum. By THOMAS MORTON . ib.

ART. X.—The Physiology or Mechanism of Blushing; illustrative of the Influence of mental Emotion on the capillary Circulation; with a general view of the Sympathies, and the organic relations of those Structures with which they seem to be connected. By THOMAS H. BURGESS, M.D. . . 245

ART. XI.—Elements of Medical Jurisprudence. By R. T. BECK and J. BECK . 246

PART THIRD.—Selections from the British and Foreign Journals.

I. THE FOREIGN JOURNALS.

ANATOMY AND PHYSIOLOGY.

Professor SEBASTIAN on the Disposition of the Blood-vessels in the Skin of Small-pox Patients 247

Dr. PAULI's Observations regarding the Reproduction of the Lens . . ib.

Professor BRESCHET on the Episternal Bones 248

Dr. NÜCKEL's Case of Spontaneous Rupture of the Spleen . . . ib.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Dr. MALIN on the Communication of Phthisis Pulmonalis to Domestic Animals . 249

Dr. TOULMOUCHE's Remarks on the Uncertainty of the Diagnostic Signs of Albuminuria . . . . ib.

PAGE
M. Trousseau on Chlorosis accompanied by Menorrhagia . . . 250
Drs. Penbeck and Goedechen on Ligature of the Limbs, as a means of shortening the duration of the paroxysm of Intermittent Fever . . 251
Polli on the Mechanical Action of Tartarized Antimony . . . ib.
Dr. Gerner's Cure of a Stubborn Case of Aphonia, by means of Ammoniacal Vapours . . . . ib.
M. Adome on the Use of Bland's Pills in Chlorotic Affections . . ib.

SURGERY.

Landouzy on Varicocele, and especially on the radical Cure of that Affection . 253
Dr. Steifensand's Case of Wound of the Heart 255
Dr. Ruete's Case of Development of Hair in the Posterior Chamber of the Eye . 256
Artificial Anus made in the Groin, with success . . . . ib.
Professor Otto on the Use of Conium Maculatum in Scrofulous Ophthalmia . ib.
Dr. Mandt's Singular Case of a Living Snake in the Stomach . . 257
Charriere's Ivory Bougies 258

MIDWIFERY.

Dr. Osiander on the Causes of Face Presentations, &c. . . . ib.

MEDICAL JURISPRUDENCE.

Dr. Graff's Case of alleged Poisoning by the Sulphocyanic and Hydrocyanic Acids ib.

MEDICAL STATISTICS.

Dr. Lohmeyer's Report of the Results of Revaccination in the Prussian Army in the year 1838, drawn up from Official Documents . . . 261

II. THE AMERICAN AND COLONIAL JOURNALS.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Dr. O'Shaugnessy on Narcotine, as a substitute for Quinine in Intermittent Fever 263

III. THE BRITISH JOURNALS.

ANATOMY AND PHYSIOLOGY.

Dr. Reid's Experimental Investigation into the Functions of the Eighth Pair of Nerves 264
Dr. Davy's Experiments on the Blood, in connexion with the Theory of Respiration 270
Observations on the Blood after Death . . . 272

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Dr. May's Case of Ulceration of the Brain 275
Squire, on the Bimeconate of Morphia, a new preparation of Opium . . ib.
Dr. Rumsey on Irritative or Pseudo-Fever, produced by the ingestion of warm fluids 277
Proceedings of the Pathological Society of Dublin . . . 279
Laycock on the effects of Colchicum and Lytta used externally . . 280
Maddock, on Rheumatism from Copaiba . . . . ib.
Stokes on the State of the Heart and the Use of Wine in Typhus Fever . . 281

SURGERY.

Davidson's Case of Laceration of the Perineum cured by Operation . . 284
Syme on the power of the Periosteum to form New Bone . . . 285
Dunn's Case of Dislocation of the Left Femur on the Pubes . . 286

Part Fourth.—Medical Intelligence.

Medical Education and Medical Reform 287
Provincial Medical and Surgical Association 303
Examiners of the London University 304
Farewell Dinner to Dr. Conolly on his leaving Warwickshire . . 304
Books received for Review 307

THE
BRITISH AND FOREIGN
MEDICAL REVIEW,
FOR JULY, 1839.

PART FIRST.

Analytical and Critical Reviews.

Art. I.

Histoire générale et particulière des Anomalies de l'Organisation chez l'Homme et les Animaux, ouvrage comprenant des Recherches sur les Caractères, la Classification, etc. des Monstruosités. Par M. Isidore Geoffroy Saint Hilaire, m.d., &c. &c.—*Paris,* 1832-36. Trois Tomes, avec Atlas. 8vo, pp. 746, 571, 618.

A general and particular History of Anomalies of Organization in Man and Animals, comprising Researches into the Characters, Classification, &c. of Monstrosities. By M. Isidore Geoffroy St. Hilaire, m.d., &c. &c.—*Paris,* 1832-36. 3 Vols. 8vo, with an Atlas.

Although some years have elapsed since the greater portion of the work before us was published, yet, as no sufficient account of it has hitherto appeared in our language, we think it important that our readers should no longer be deprived of the highly interesting and valuable information it contains, and shall therefore now present them with an analysis of it. In executing our task we shall confine ourselves almost exclusively to the exposition of the author's views; introducing our own opinions but sparingly, even when they are at variance with those maintained in the original treatise. On some future occasion we may take up the whole subject fundamentally; our present object being rather to supply facts than to criticise doctrines.

The object of M. St. Hilaire's work is to give a complete history of the subject of monstrosities: under which name all the various congenital irregularities of form and structure, occasionally met with in man and animals, are generally included. The author has collected together a great number of facts, relating to the different forms and degrees of anomaly, which he has systematically arranged in classes, orders, &c.; and he has afterwards endeavoured to establish the laws and general relations to which all the individual facts may be referred. He has shown how these laws and these relations are themselves only derived from the common laws of organization, and how, among the numerous theories of the formation and growth of animals which have been proposed in modern times, those which are not applicable to anomalous cases are also inapplicable to normal facts in general, and ought to be rejected; and many principles, on the contrary, but slightly established at present by the study of natural facts, find in the phenomena of monstrosities complete elucidation. M. Isidore St. Hilaire has also pointed out that this subject embraces all the conditions of organization in the various classes of organized beings, and that there is scarcely any general fact, any anatomical or physiological law, on which it does not throw light and either confirm or disprove. Thus the necessary consequence of an exact and profound knowledge of anomalies will be, that the study of normal and abnormal facts, intimately associated together, will lend to each other a mutual and powerful support.

A vast collection of most valuable materials on this subject may be found scattered through various publications on natural and medical science; but before the younger St. Hilaire (who has largely profited by his father's labours) undertook the task of collecting them, there existed no modern work which professed to give a complete and separate account of the various anomalies of organization, such as might serve as a text-book, and for the purposes of reference, in which all the varieties of monstrosity which have been met with should be recorded, as well as the opinions of different writers on their nature and causes.

Our author thinks that the consideration of the various kinds of monstrosity, with the laws and causes of their formation, should form a distinct branch of science, and should be treated of separately from pathological or general anatomy and physiology, embryology, or zoology; with all of which they have a very close connexion, and together with some of which they have mostly been described. To this particular subject which M. Isidore St. Hilaire has thus isolated from the sciences by which it is surrounded, he has proposed that the name of TERATOLOGY* should be given, which he considers preferable to the old denomination of *monstrosities*, the term which was previously given to all kinds of congenital malformation. Our author's views as to the separate place which Teratology should hold in science are supported by Meckel, who supposes that the various species of monstrous formation compose a series rising by regular gradations, from the natural shape to the most unnatural deformity: and that the intermediate steps are not constituted by single or individual cases, but that every variety of monstrous formation is accurately repeated in other individuals; so that, in fact, a separate and independent kingdom of monsters might be established.

Monstrosities have attracted the attention of philosophers as well as the vulgar in all ages. Among the ancients, Hippocrates, Aristotle, Pliny, Galen, and even Empedocles and Democritus noticed their occurrence and investigated their causes; and these early writers had almost as accurate notions of the nature, and gave as faithful descriptions of monstrous formations, as any of the authors on this subject before the commencement of the eighteenth century. Indeed, the history of monsters, till very lately, was composed of a collection of marvellous tales, inaccurate descriptions, and absurd and superstitious prejudices. This long period of ignorance, with respect to their true nature, may be called the fabulous period in the history of the science, and cannot be said to have terminated before the time of Ambrose Paré. A few authentic and interesting cases, it is true, had been already recorded; but these were only rare exceptions, which attracted little attention, except when some author tried to give a new and ridiculous explanation of them, derived from the fanciful ideas which were then exclusively prevalent. In fact, monsters were regarded by the writers of the seventeenth century as by those of preceding ages, as prodigies and sports of nature, arising from supernatural or unnatural causes.

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* Derived from τερας-ατος, a monster, and λογος.

After the fabulous, succeeded what St. Hilaire has called the positive period in the history of anomalies; it comprises about the first half of the eighteenth century. Evident progress now commenced, and facts were correctly observed, though still often explained on false principles. The most celebrated authors of this age on teratology were found among the members of the French Academy. Méry, Duverney, Winslow, Lémery, and Littre may be particularly mentioned. In the works of these great men, we not only find numerous facts accurately observed and described, but many judicious remarks and violent attacks against ancient prejudices. In place of those explanations of the phenomena of monstrosity, which were admitted by the superstition of the preceding period, they endeavoured to substitute scientific and reasonable theories. The causes of monstrosity particularly excited attention; and though many errors were fallen into, for want of the support of a sufficient number of facts, yet it was discovered that one of the greatest difficulties was involved in the question, whether monsters were formed so originally, or whether the monstrosity was accidentally acquired. A very long and able controversy was carried on concerning this point between Lémery and Winslow, the former of whom contended that monstrosities were formed or arose during the growth of the embryo; and modern discoveries in embryology have shown that he was correct, though his rival, who held that the germs were originally monstrous, was considered to have triumphed at the time.

The labours of these celebrated academicians conducted the science to its last epoch, which may be denominated the scientific, and which extends from the middle of the eighteenth century to the present time. It may be divided into many periods; and it will be seen that a vast difference exists between the state of teratology at its commencement and end, owing to the rapid progress which science has made. Haller may be said to have commenced this era, though Morgagni had previously corrected several erroneous opinions respecting the nature and causes of monstrous formations. Haller, in his treatise "*de monstris*," collected all the facts relating to this subject which were recorded by his contemporaries and predecessors, submitted them to a judicious analysis, and deduced from them several conclusions eminently calculated to promote the advancement of this study. Haller, however, fell into some fundamental errors, the most important of which was that respecting the mode of the development of the embryo. He supposed (and his theory prevailed till very lately, and is now partly entertained by some physiologists,) that the development of the organs of the fœtus was centrifugal, or that the heart, brain, spinal cord, &c. were formed before the vessels and nerves which were gradually developed from them. This theory, as we shall presently show, is contrary to those laws of formation by which the greater number of anomalies are explained by our author and other teratologists.

The rapid advances which have led to the present state of this department of science are owing to the indefatigable researches of modern anatomists. The study of general and comparative anatomy led the way to the true method of investigation, viz. that of comparing adult man to the embryo, and various animals to man, both in the adult and fœtal states. This comparison has given rise to two new methods of investigation, which are now almost universally recognized in the science of anatomy. One discloses the true laws of organic formations; the other embraces the general facts of the structure of animal bodies, considered in all ages and all species. Both of these methods reveal to us important knowledge concerning the composition of organs: the one plan assists us in learning the mode of their formation; and the other decomposes them by a learned analysis, and shows us the elements, everywhere identical, disposed according to invariable rules. Embryology is thus placed upon its true basis, and philosophical anatomy created. Among those naturalists to whom we are indebted for these researches, we may particularly enumerate Geoffroy St. Hilaire (the father of our author) and M. Serres in France, and Frederic Meckel and Tiedemann in Germany.

The various species of monstrous formations have been referred to three classes, viz.

1. Anomalies which arise from arrest of formation or development, in which various parts are found either imperfectly formed or altogether deficient.

2. Anomalies from excess of formation or development, in which some organs exceed their natural limits either in size or number.

3. Anomalies which result neither from arrest nor excess of formation and development, but in which the formative process seems to have been simply perverted, thus producing various modifications in the direction and situation of organs. In this class M. Isidore St. Hilaire includes the entire group of compound monstrosities, which result from the junction or fusion of two or more separate individuals. These have generally been referred to one of the previous classes.

The explanation of these different varieties of organization, or the laws of anomalies, must be derived from the general laws or principles of organization, which the study of philosophical anatomy and embryology have revealed; and, before proceeding to the consideration of the different varieties of monsters, we shall briefly mention the most interesting of these laws, and explain the manner in which they elucidate the different classes of monstrosities.

1. The first and most important is the law of *unity of organic composition.*

One great principle reigns over the whole of zoological science, that there is a unity of plan in the animal kingdom. Philosophical anatomy has shown us that the organs of animals are composed of materials which are always essentially the same, and which are combined according to definite rules; and that curious and unexpected analogies often exist between beings placed at the opposite extremities of the scale. If we admit the existence of a distinct and peculiar plan of organization for each species, or even in different families, we only obtain partial views, and the science will be reduced to the sterile observation of facts, without reciprocal connexion, rational analogies, or possible consequences. If, on the contrary, we elevate our ideas to the conception of a unity of plan pervading the whole animal kingdom, we shall only see in the multitude of beings which compose the animal series, the innumerable parts of one immense whole, the infinite varieties of one and the same type.

If we apply to the solution of the difficulties which this subject presents, the theory of inequalities of formation and development, we shall find it equally applicable to zoology as to teratology; and the fundamental truth will be apparent, that one or more metamorphoses, to a greater or less extent, sometimes consisting merely in a simple change in the mode of evolution of an organ, will explain all those varieties of form and structure which at the first aspect seem to arise from essential differences in the formative process.

The series of species in the animal kingdom seems to be parallel with the series or stages of formation or development in any individual being, or, in fact, with the series of ages in that being; and the facts of one are reciprocally connected with and explain those of the other. The connexion between teratology and zoology is now seen. The theory of inequality of formation and development relates both to the series of ages in the embryo and the series of zoological species, as well as to the series of monstrosities: it shows the parallel relation between the first and second as well as between the first and third; and by the same laws the series of zoological species and monstrous cases are necessarily analogous and parallel to each other. Thus, in an abstract point of view, all the differences between beings either normal or abnormal may be embraced in the same considerations and referred to the same formulæ: as, for instance,—the inferior beings are, as it were, the permanent embryos of animals higher in the scale; and, reciprocally, the superior beings, before they arrived at the definite forms which characterize them, have transitorily offered those of the lower animals. This must not be taken, however, quite literally; for the resemblance or analogy is only seen between individual organs, not entire beings.

By this law of unity of type in the formation of animals (which has been so fully exposed in the works of the elder St. Hilaire, Meckel, and Serres,) may be explained the resemblances which have so often been observed between the anomalous states of one species and the natural form of another. Every animal in whom there has been arrest of development should realize in some of its organs the conditions met with among the inferior classes. Excess of development, on the contrary, should cause a resemblance between the animal which is the subject of it and some of the beings higher in the scale. Many examples of monstrosity have been brought forward in support of this theory, and we may briefly state a few of them. The most numerous cases are those in which the higher animals by arrest of development present the characters which are natural to some inferior species. Thus man, when affected with monstrosity, often has a marked resemblance in some characters with different mammalia, as by the persistence of the tail,* and by many anomalies in form either of the limbs, body, or head. Thus, by the existence of a cloaca, labial fissure, duplicity of the uterus, smallness of the brain, and absence or imperfect state of the convolutions, the malformed human fœtus presents characters which are all found existing naturally in various species of *rodentia*, as the beaver, &c.

In some monsters there has been found bifurcation of the glans penis or clitoris, and two vaginæ, a disposition of parts existing normally among marsupial animals. By imperforation of the vulva, and a separate termination by distinct orifices, of the sexual and urinary organs, with imperfect development of the eyes, the genus called *aspalasomus* and other monsters realize in man those organic conditions, which in the normal state distinguish the mole and some other *insectivora* from all other mammalia. In the genus of monsters, *phocomeles*, the limbs are shortened, the hands and feet appearing to exist alone, and to be inserted immediately on the trunk, as in the seals and the herbivorous cetacea. In the rare monster, *ectromeles*, the limbs are nearly or altogether deficient, as in the ordinary cetacea.†

We may also often observe some of the conditions of animals still lower in the scale, realized in human monsters; thus, there may be a rudimentary state of the palatine arch, as in fishes; imperfect development of the diaphragm, as in all oviparous animals; a communication between the different cavities of the heart, as in reptiles; an absence of the brain and spinal marrow; and a nervous system composed only of ganglions and nervous filaments, as in the articulated animals.‡

Although the cases are much more rare in which the inferior animals resemble the higher, from excess of development,§ yet many instances of this kind have been met with. St. Hilaire has seen several individuals among the carnivora in which the tail has disappeared, and the spinal marrow has ascended in the vertebral canal, as it does in man and the most highly-organized quadrumana. This anomaly also realizes the conditions met with in some animals much lower in the series, as the anourous batrachians or frogs, where there is a continuance or excess of the process of development in the change from the tadpole to the perfect animal.

The possibility of referring the various species of monsters to a common type is a necessary and easy deduction,—in fact, an indispensable conclusion to be drawn from the theory of the unity of organic composition. When we admit that the entire classes of the animal kingdom are established upon one and the same plan, it becomes absurd to allow the existence of many types in one family. From the natural relation which exists also between the different degrees of monstrosity and the links in the animal chain results a complete demonstration that monstrosity is not a blind disorder springing from freaks of nature, but a particular class governed by constant and precise rules, and capable of being systematically divided into definite tribes and genera. The elder St. Hilaire, however, is disposed to consider each individual monster as constituting in itself a distinct species; and he does not agree with Meckel, that every variety of monstrous formation is accurately repeated in other individuals.

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* In the early stages of formation of the human fœtus, there naturally exists a prolongation of the coccyx, which is removed by the progress of development.

† It is proper to observe, however, that some of these cases were probably instances of "spontaneous amputation," and cannot, therefore, be properly called monsters at all. REV.

‡ In the last of these cases, no real analogy can be said to exist.—REV.

§ These anomalies appear to be more rare, perhaps, than they really are, on account of the much greater number of monstrosities that are observed and examined in man than among animals.

2. The second law which we shall mention as being closely connected with teratology is one of the fundamental principles of embryology: the basis, in fact, upon which that science rests, viz., that no organs originally preexist in the ovum, but are all formed at various periods of its growth. Necessarily very minute and simple at the time of their early origin, the different organs afterwards pass through a series of changes in the process of development. These changes are far from being equal either in number or importance, whether we compare together the same organ in different beings, or different organs in the same being; so that, when arrived at their definitive or permanent state, some have passed through a greater number of phases, and have departed much more from their primitive conditions than others. Such is the normal but not the invariable mode of development: an organ may stop beneath its ordinary degree of perfection, or even be entirely abortive; it may, on the contrary, exceed the natural term of its evolution, and thus will arise the two groups of anomalies, opposite in their conditions of existence, and also in their causes, to which so many of the species of monsters have been referred, viz. arrest and excess of development.

The admission of the law of non-preexistence of organs in the germ is fatal to the doctrine of original monstrosity existing before fecundation: a doctrine conceived by Licetus and the older writers on this subject, but which owed its celebrity to its adoption by Winslow and Haller. It would now have been almost forgotten had not Meckel lately attempted to revive it for the purpose of explaining the occurrence of certain monstrosities, the origin of which cannot be understood in the present state of teratology, such as the retroversion of the abdominal limbs and some other peculiarities of organization, which are constantly associated with the junction of the legs in the monsters named *symeles*. M. St. Hilaire says that the only argument brought forward by Meckel in support of his hypothesis is the impossibility of finding a satisfactory explanation of these anomalies by the theory of accidental production of monstrosities: this is true in the present state of science; but there is no reason why the obscurity of this case should not one day be cleared up, like many other facts in teratology, which were formerly thought inexplicable, and cited as certain proofs of the original production of monstrosities, but which the ulterior progress of science has discovered to be in support of the inverse theory.

According to the law which admits the formation and not the evolution of organs, monsters from arrest of development may be considered in some respects as permanent embryos: they show us at the termination of intra-uterine life some of their organs in the simple state in which they were first formed; as if nature had stopped in her course for the purpose of allowing us the opportunity of observing her processes.

3. A third law is that of eccentric development. We have already remarked that Haller (and he was followed by all the anatomists of the eighteenth century) considered that the heart was formed before any other organ, and was itself the origin of all the others; that it furnished the principal vascular trunks, which afterwards subdivided into branches more and more minute. In the same manner the nervous trunks were considered to derive their origin from the cerebro-spinal axis, which was said to be first developed, and the larger nerves were afterwards thought to ramify into the minute branches; in other words, all the vessels and nerves, subdividing more and more, proceeded from the central parts of the nervous and vascular systems towards the organs placed on the surface of the body, to which they gave nourishment and life. This theory is denominated that of *centrifugal development*, and has still many supporters.

The inverse doctrine, that of *eccentric* or *centripetal development*, was proposed by M. Serres, and is warmly supported by Geoffroy St. Hilaire and his son: all the laws of teratology proposed by the father and followed by his son in the present work are founded upon it, and by this theory a great number of anomalies are explained. These anatomists say that the vessels and nerves are formed before the heart and nervous centres; they first originate in the superficial organs on the surface of the body, and are gradually developed towards the centre; in support of this opinion it is said that the heart, brain, and spinal cord have all of them been found wanting in different monsters, while the vessels and nerves have never been seen wholly deficient. The large trunks are also found more frequently irregular in their course and distribution than the superficial branches of an artery or nerve, and the contrary should be the case if the development was centrifugal, as it has been observed that those organs which are latest formed are the least constant.

According to the observations of M. Serres, the development of the body commences on the surface of the two lateral halves, each central and single organ being originally double, its right and left portions are at first distinct and separate, and become afterwards united. If by any causes, as arrest of development, the union of these two half-organs is prevented from taking place, if this primitive state of formation becomes permanent, two lateral organs are formed, which may be either entirely distinct or only partially separated, according to the period of formation at which the arrest of development took place. The median labial fissure (often confounded with the lateral fissure or true hare-lip) has been thus explained, as well as fissure of the palate, scrotum, urethra, and spinal fissure or spina bifida, &c. M. Serres also states that the hollow organs situated in the median line are composed originally of two halves, as well as the solid organs; and his observations have been, to a certain extent, confirmed by Dr. Allen Thomson, and others. Thus, according to our author, there are at one period two hearts (this organ is placed in the first instance in the median line), two aortæ, two vaginæ, uteri, bladders, &c. These organs are considered to pass through three successive stages in the process of development: in the first they are completely double, and the two portions quite separate;

in the next stage they approach and unite in the median line, the two inner walls being applied against each other, and at the third period they become definitely fused, the inner walls being removed, and all traces of separation lost. If by arrest of development the second stage of formation becomes permanent, the inner walls of the primitive organs which unite together and form naturally a temporary septum are not removed, and the organ is intersected by a longitudinal partition. Such an anomaly is sometimes met with in the human subject, affecting the vagina and uterus, and realizing the natural conditions of the sexual organs in some marsupial animals.

Another fact which is dependent upon the law of centripetal development is the greater constancy of form in those organs which are of early formation than in those later developed. When any cause comes into action at any period of uterine life, by which the process of growth may be disturbed, those organs which are already nearly or fully evolved will necessarily be little or not at all altered; but a very marked change, on the contrary, may be effected in those parts which are very imperfectly developed, or whose formation has not even commenced. In the latter case complete atrophy may be effected.

If we add, that in most of the systems of organs the different parts are subordinate in their formation one to another, the second being produced by the first, the third by the second, and so on, we shall see that the suppression of any one of them, without having any influence on those which preceded it, will necessarily cause the complete absence of all those which ought to have followed it in the order of development. The results of observation perfectly confirm these remarks; it has been found that the umbilicus and small intestines are the parts most constant in monsters, and also the organs first formed in the embryo; the spinal cord also is less often wanting than the brain which it precedes, the aorta than the heart, &c. The superficial and lateral parts of the body are also much more constant than the central or medial organs, they often exist when the latter are wanting, and they frequently present a regular conformation when the latter are seriously modified or very incomplete. Many cases may be met with where the different parts or organs have been reduced to their external covering or integument; thus in the monsters named *cyclocephali* and *otocephali*, in which the two eyes or ears are in contact, or united in one, the nose is entirely rudimentary, the bones, &c. being deficient, and only the skin remaining, which is sometimes prolonged in the form of a snout or trunk: sometimes one of the abdominal limbs has been found in this rudimentary state, and in some very imperfect monsters the whole being seems to be reduced to the tegumentary covering, inclosing a few unconnected parts, as bones, vessels, &c.

We are far from agreeing with M. Serres and our author in all the points to which this law has been applied. The centripetal mode of development no doubt obtains to a great extent; for instance, it is now generally considered that the formation of the body goes on from the lateral halves towards the centre, and that the median single organs are originally double;* but how can the mode of growth of the limbs be explained on this principle? They bud out from the trunk, and are first formed beneath the skin which they reach, pushing out like little globular shoots. Again, it is far from proved that the nerves and vessels are formed before the heart and brain;* it is, perhaps, more probable that they are naturally evolved together; the existence of arteries and nerves in a monster who is deprived of heart, brain, and spinal cord, only shows that these parts are independent of each other, and that the formation of one has been arrested without affecting the development of the others.

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* Velpeau and some others still affirm, however, that the median line is first formed.

Burdach says that the centripetal and centrifugal modes of development both obtain in distinct parts; thus, in the bones of the head and trunk, ossification proceeds from the circumference towards the median line; but in most of the bones of the extremities this process extends from the centre to the surface. The vesicula umbilicalis again enters from the surface, and forms the digestive tube, the development of which is therefore eccentric, but afterwards the salivary glands, the liver, lungs, &c. proceed from the digestive canal which they surround.†

The theory of arrest of development does not include all the phenomena of monstrosity; it throws much light upon the origin of monsters by default, but hardly any upon those by excess. Further researches upon embryology, and especially the study of the mode of formation of the vascular system, seemed to have revealed an important law by means of which many of the monstrosities by excess might be explained. It has been found that when an organ is double, the vascular trunk which nourishes it is so also; likewise the absence of a part is necessarily connected with that of its artery. This was explained by the theory that the vascular system presided over the formation and evolution of all the other organs, so that deficiency or atrophy, duplication or hypertrophy, of any organ or region was owing to a similar state of the blood-vessels, and especially the arteries.

This ingenious theory was brought forward by M. Serres, and was supported by the elder St. Hilaire, but has never been generally adopted by anatomists, for, as it has been remarked by Béclard (*Lectures on Monsters*), "it is extremely difficult to decide, in this connexion of phenomena, which is the cause and which the effect; for there is no proof whatever in support of the opinion that the development of the organs depends on that of the arteries, which is not equally applicable to the supposition that the size of the arteries depends on the volume of the organs; and that when the arteries are wanting altogether, it is because the organs which they supply are not evolved." These objections are now admitted by M. Serres himself, who has been convinced by further researches in embryology, that he must considerably restrict his ideas on this subject; and the relation, which he now admits, between the anomalies of organs and the formation of their vessels is rather that of simple coexistence than of cause and effect. In fact, according to the law of centripetal development, the organs are formed before the vessels which are distributed to them, which cannot therefore be the agents of their formation, though they react upon them afterwards, and perform an important part in their ulterior progress.

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* The first rudiments of the sanguiferous system certainly consists in the vascular area, but the simple tubular heart is the next part which appears. The spinal cord also is the first portion of the nervous system that can clearly be discerned.

† This is not correctly expressed. The vesicula umbilicalis does not enter the embryo. The yolk-bag, its analogue in birds, does enter the alimentary canal.—REV.

4. Another law by which the production of some anomalies has been explained is that of compensation or the *balancement* of organs. By this principle, as maintained by Geoffroy St. Hilaire, exuberance of nutrition in one organ is supposed to involve, to a greater or less extent, the total or partial atrophy of some other organ, and vice versâ.* It is said to be a consequence of this law that, while in the higher orders of animals, and particularly in man, some organs reach the *maximum* of development, others remain in a *medium* or *minimum* state. Many examples of this might be brought forward; thus, while man is remarkable for the size of his brain, he is equally so by the smallness of his face. Many animals occupying a low situation in the general scale of organization are furnished with some organs in a higher state of development than is met with among the superior beings: thus some of the double and lateral parts which always remain separate in the human subject, as the kidneys, approach and unite by a continuance of development in fishes and many aquatic birds. The two eyes naturally coalesce into one in some of the lower animals, as the *monoculi* and other entomostraca; and these states of organization, which are normal in these creatures, are sometimes imitated in human monsters, and must then be considered to arise from a true excess of development; though this excess, as in the case of junction of the two eyes, cannot take place without a corresponding atrophy or suppression of some parts, and a marked arrest in the development of many others. This compensation or balancing between the different organs in the monster only renders the analogy between the malformed human fœtus and some of the inferior animals more complete, for the defects and excess of formation coincide in the normal cases as well as in the abnormal.

Innumerable applications of the law of compensation may be made to the study of monstrosities; thus, in an individual who has several supernumerary fingers or toes on one hand or foot, we often find the opposite limb with less than their natural number. Andral says that supernumerary fingers are often found in those monsters in which other parts of greater or less importance are incompletely formed or altogether deficient, as in cases of *cyclocephalus.* Two distinct anomalies of size, the one by diminution and the other by increase, may occur together in the same subject, in consequence of this antagonism of development; thus, one kidney has been found extremely small or almost atrophied, and the other in a state of hypertrophy, or the kidney has been seen very diminutive, while its capsule was very voluminous. Cases which might be cited in support of this law are met with every day.

5. The next law of organization which we shall mention, is that of similar position or affinity of similar parts for each other ("de soi pour soi.")

When two or more organs perfectly resemble each other, they seem to have a tendency to approach and unite. This has been observed between similar organs and similar portions of organs in one individual, as well as between two distinct individuals. Many anatomists at different times, when examining cases of double monstrosity, have been struck with the remarkable relation of situation and connexion which the two subjects offered towards each other; but it is only within the last few years that this circumstance has received much attention, and been studied in a philosophical point of view. The regularity of disposition which two beings present when united together is not a rare circumstance, a peculiar characteristic of certain monsters, but is constant and common to all, and must be considered as a fact of high importance, influencing all the other facts of double monstrosity. The two subjects which compose a monster completely or partially double, are always united by corresponding aspects of their body; that is to say, side to side, face to face, or back to back. Each part and each organ in the one corresponds to the same part or organ in the other; every vessel, nerve, or muscle, situated in the line of union, joins itself to the same vessel, nerve, or muscle, in the other subject, in the same manner as the two primitive halves of any single organ, which are originally separate, unite by the progress of development.

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* The *fact* of the compensation of organs was long ago recognized by Paley, who pointed out that exuberant nutrition of one organ is generally accompanied by diminished size of another. We do not feel inclined to accord with M. St. H. in regarding the former condition as the *cause* of the latter.—Rev.

These general facts, very important in themselves, are equally so from the numerous consequences which may be deduced from them; thus they serve to confirm the proposition, that the organization of monsters is governed by very constant and precise laws, and that all the irregularities of monstrosity never break through certain limits. By a knowledge of this law it becomes possible, when reading the descriptions and looking at the figures of monsters in old works, to distinguish those monstrous combinations which might really have existed, from those which are only the fanciful and absurd productions of imposture or of a fertile imagination.

The important law of affinity of similar parts for each other—established by the elder St. Hilaire, from researches made on anomalies of the most complex description, and verified, as we have shown, in all cases of double monstrosity—is also fully confirmed by the study of the simplest deviations of form and structure. We shall see this great principle account with equal facility for the union of two organs, two systems of organs, or two entire individuals. It is by this principle that we explain the different anomalies by junction or fusion; or, as they are denominated by M. Breschet, *symphyses*. Thus the two kidneys, the two eyes, the two ears, may join, and form a double kidney, eye, or ear, or the two organs may unite and form a simple or single organ, in which no traces of duplicity remain; in the same manner as in the embryo the two primitive uteri join to form a single symmetrical uterus, which union was observed to take place by M. Serres, when making those researches upon which he founded the theory of eccentric development. These anomalies, by the junction of two lateral organs in the median line, are the consequence, as we have previously remarked, of excess of development. The theory of eccentric growth shows us that single symmetrical organs, when divided, are in their primary state of development, and, when united, in their secondary degree. The eyes, ears, kidneys, &c. naturally stop at the first stage, and only accidentally arrive at the second, when they present anomalies by fusion.

If we consider that the anomalous states, by junction of double organs, arise from the same laws which regulate the natural union of parts, and are only particular effects of one universal cause, we can easily conceive that similar parts ought to be exceedingly subject to be joined and confounded together; and deviations of this kind are, in fact, very common, for there are scarcely any double organs which do not furnish us with examples. Many of these instances, it is true, are met with in cases of great monstrosity, because most of the lateral organs are separated from each other by many important parts, as in the eyes and ears, and cannot unite in the median line without the suppression of these parts, but union may take place between the kidneys without an serious alteration even of connexion. Junction of the testicles, ovaries, ribs, teeth, fingers, &c. must all be explained by this law.

We must here mention another fact, which, on account of its general application, and the light which it throws on the study of anomalies, might even be elevated to the rank of a separate law. Those parts which are several times repeated or multiplied in the body, which, in other words, are homologous, or have many congeners, as the fingers, toes, teeth, &c. taken together, form organs very important, and very constant in their development, but each part individually is of little relative consequence, and subject to vary, the frequency of which variation is proportionate to the number of analogous parts. Almost all the incontestible examples of the addition of really supernumerary parts have been met with among those organs which have the greatest number of similar parts placed in series, as teeth, &c.; and that this multiplication of organs sometimes really takes place must be admitted, though this species of excess of formation is much less common than has generally been believed. In fact, the greater number of anomalies which have been referred to this head, arise from causes altogether opposite, as in the entire group of compound monsters, which are caused by the junction or fusion of two distinct beings, and not the addition of supernumerary parts to one individual. Many simple anomalies also, in which the number of organs is apparently increased, only arise from the division or separation of one part into two, the primary state of formation having remained persistent, and thus these malformations must be referred to arrest instead of excess of development. We may here remark that the organs which are most subject to numerical variation, are precisely those which are found to be least constant in number in the different genera of animals, such as the teeth, vertebræ (except cervical), ribs, fingers, &c.

In the foregoing view of the different laws of organization which have been created, we have simply stated the opinions of the writers by whom they have been formed, and we are far from admitting all the conclusions which have been drawn by them, (in many instances too hastily, and from too small a number of facts.) In the first place, it has been lately said,* that one of the fundamental principles by which the formation of a great number of anomalies has been explained is in many cases quite unestablished. We allude to the theory of arrest of development, which, it is said, will only satisfactorily account for a very limited number of malformations; for before a congenital lesion can be stated to result from a stoppage in the process of growth, it is necessary to ascertain whether any of the transitory states of fœtal life correspond with this lesion.

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* Cruveilhier, in the Bulletin de l'Académie Royale de Médecine. Tom. iii. Nos. 4 and 5, Dec. 1838.

The researches of embryology have shown that the product of conception, between the first period of its formation and the time of its complete development, passes through a series of changes as wonderful as the metamorphosis of insects; and the comparative study of these temporary states of the human fœtus with the permanent forms of the inferior animals teaches us that the former often represent the latter; and since in a few cases certain vices of conformation have been found to represent, both some of the temporary fœtal stages and some of the permanent states of the lower animals, generalizations have been hastily formed, and conclusions frequently drawn, which are supposed to apply to all cases, without any further examination being made. The great error has thus been fallen into, of not requiring direct proof, but of allowing *a priori* conclusions to take the place of reasonings *a posteriori*, upon which alone theories should be founded in science. We should never be contented with any presumptive evidence derived from induction or analogy, however ingenious, but we must seek for direct proof; and before we explain the origin of any malformation by arrest of development, it is necessary to show, by anatomical investigation, that the embryo presents a similar disposition at some epoch of its life.

Many anomalies included in the great class of congenital adhesions (*symphyses* of M. Breschet), as the occlusion of some of the natural orifices of the body, the anus or vagina, for instance, have been referred to arrest; but, except in the case of the pupillary opening, Cruveilhier says that it has never been shown that any of the natural apertures are closed by a membrane at any period of uterine life. We will take another class of malformations, viz., congenital solutions of continuity, which have been considered to arise from the persistence of certain conditions peculiar to an early state of fœtal existence, and not as any new or accidental state. M. Cruveilhier says,* that if this is the case, this stage of formation must exist previously to the fifth or sixth week of gestation; after which period, he adds, "I can certify that it will not be met with." He also says, "I avow my incompetence to detect the structure of the human fœtus before the fifth week, and I have a great distrust of any observations made on an embryo, the formation of which has scarcely commenced; for since it is even difficult to see parts which are considerably developed, it must be far less easy to study the rudimentary states of microscopic organs, so delicate, that not merely the least touch, but even the movements of the liquid in which they are placed for the sake of examination, is capable of tearing and destroying them."

As in the case of obliteration of the pupil, there are some instances of separation of parts naturally united, which must be referred to true arrest of development, as persistence of the arterial and venous ducts and foramen ovale, permeability of the umbilical artery or vein, or of the urachus; also, persistence of the communication between the peritoneum and tunica vaginalis; but, with the exception of a few cases of this kind, it has been said* that none of the other anomalies belonging to this class can be referred to corresponding states in the life of the fœtus; thus can hypospadias, hare-lip, spina bifida, &c. ever be observed as natural conditions of the embryo, and have they not been considered as effects of arrest of development upon insufficient evidence? The remarks which we have made concerning this theory also apply in many respects to some of the laws of development; and particularly, as we have before stated, to that of centripetal formation, which cannot be verified in all parts by an examination of the mode of growth of the organs of the embryo.

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* Ibid.

Before we proceed to consider the different groups into which anomalies have been divided by our author, we shall say a few words on the very obscure subject of the causes of monstrosity, the original agent which produces a modification in the formative process. The difficulties which here surround us, are common to this, with all the other departments of natural science. Nature only presents effects to our observation, or at most their proximate causes, and the primary or directing influences can only be deduced from these effects. Teratology is so closely connected with embryology, the laws of one being derived from those of the other, that, while the causes influencing the original phenomena of normal development are unknown, those presiding over irregular formation must necessarily be involved in darkness; and direct observation is far from having furnished us with the results which might have been expected concerning the early stages of gestation, the conditions of the very young embryo, and the first steps of its development. The ancient writers took much interest in this mysterious subject, and mixed up many ingenious but fanciful ideas with the most unnatural absurdities. They almost all agreed in one point, that monstrosities arose from some disturbance or imperfection in the act of fecundation. In the opinion of some, however, malformation or disease (as tumours) of the uterus of the mother was capable of producing deformity of the child; and, lastly, unnatural connexions were admitted by all the older physiologists to be incontestible causes of monstrosity.

Most of these notions were founded on the hypothesis of original malformation of the germ; but now that this theory is discarded, we must look to influences operating after fecundation, during the progress of development. In the first place, the birth of a monster has sometimes undoubtedly followed various accidents received by the mother during the early months of gestation, as a fall, violent blow, &c. A vivid mental emotion will occasionally have the same effect, or long-continued anxiety of mind; and in proof of the action of the last cause, it has been observed that unnatural productions are more common among unmarried than married women.

Direct evidence that the development of the embryo may be affected by external causes has been afforded by experiments performed on the eggs of birds. Geoffroy St. Hilaire allowed the process of incubation to commence in some eggs under ordinary circumstances; and after it had proceeded naturally for a short period, about three days, he performed various experiments on them, as shaking them violently, perforating them at different points, keeping them in a vertical position, upon either the large or small end, covering a part of the shell with wax, or some varnish impervious to the air, &c. The effect of these perturbations was the constant production of a large number of monstrosities. These experiments were repeated by the younger St. Hilaire, in a different manner. He altered the structure of the eggs before the process of incubation had commenced, and not as in the previous instance during its course. Entirely different results followed; for in the latter case no anomalies were ever produced, vitality was mostly destroyed, the embryo in some cases seemed retarded in its development, but was never monstrous. The opposite effects of these experiments show the truth of the proposition, that the origin of anomalies is accidental, and not primitive.

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* Ibid. Cruveilhier has been here a little too sceptical; it is, certainly, well established, that at one period of fœtal life all the natural apertures of the mucous canals are closed.

Another of the most generally admitted causes of malformation is disease of the embryo itself. Haller and Morgagni among the older writers, and Meckel, Béclard, M. Dugès, Cruveilhier, &c. in recent times, have especially adopted the theory that a great number of the anomalies of the superior regions of the body may be explained by the occurrence of hydrocephalic disease. It has been said, especially by Béclard, that dropsy is an affection to which the fœtus is more liable than any other, and that the brain and parts connected with it, as the spinal marrow, are especially prone to be attacked. If this disease comes on at an advanced period of utero-gestation, it may occasion separation of the bones of the cranium, if earlier, it may produce hydrocephalic hernia and spina bifida. The protrusion of the brain may remain till birth, or it may have disappeared, causing destruction of the cerebral masses and vertebral cord; in which case, an *anencephalous* monster will be produced, in which the cranium and spinal cord are widely open and their contents deficient. By the action of this and other diseases many malformations have been explained, and this theory is doubtless partly true, though its advocates have carried it to far too great an extent.

Geoffroy St. Hilaire has proposed another hypothesis, by which he accounts for the production of many monstrosities. His idea is that unnatural adhesions sometimes take place between the embryo and its membranes, occasioned, perhaps, by some accident which tears the membranes, and probably allows of the escape of part of the contained fluid; when the edges of the aperture coming in contact with some of the organs of the fœtus, union takes place, and bridles and false membranes form, which may mechanically obstruct the development of different organs, causing arrest of formation or distortion, as in the case of club-foot. St. Hilaire artificially produced this connexion between the embryo and the shell in birds, by unnaturally raising the heat during incubation. This morbid change may account for some forms of malformation, as unnatural connexion between the placenta and different parts of the fœtus, which has been observed in several instances. A case of this kind was lately related by Dr. R. Lee to the Med.-Chir. Society of London, in which the placenta was attached to the integuments of the forehead by a membranous hand, three quarters of an inch in breadth and one inch and a half in length; the portion of skin where the union had taken place was preternaturally vascular, and presented the structure usually observed in nævi; the development of the skull had been arrested, and all the bones forming the vault of the cranium were deficient, but the brain itself and its membranes were healthy.

None of the explanations which we have given throw any light on the causes of compound monstrosity, which consists in the union of two or more individuals. It is an important question to decide whether the germs or ova are originally double: the most general opinion seems to be that they are not; we must therefore enquire how the junction of two separate beings can be explained. Why do not twin embryos always unite? Two perfect and distinct fœtuses have, according to our author, been occasionally met with contained in the same membranes, so that mere contact or pressure cannot account for it. The law of affinity of similar parts for each other has shown that the two individuals composing a double monster are always united by similar parts of their body; do ova then always unite when they happen to be placed next each other by corresponding aspects? This proposition requires to be verified by observation and experiment. In a remarkable memoir communicated to the French Academy of Sciences by MM. Delpech and Coste in 1832, these physiologists state that there exist two electric currents, which are directed towards the same point, in two embryos, placed opposite to each other, and opposed by similar surfaces, but which run in a contrary direction in those which are placed differently. In consequence of this, they say that the first are always united, and the latter as constantly isolated.

We have mentioned that, in some cases, monstrosity of the fœtus appears to have followed external causes which have affected the mother, as a blow or fall, and in other instances a vivid or long-continued mental impression. Some congenital malformations appear also to be hereditary; this has been particularly observed among the slighter anomalies, as albinism, irregularity in the number of fingers and toes, and hare-lip. St. Hilaire says that this last imperfection is seldom or never transmitted from parents to their offspring. The writer of this article has seen, however, several well marked instances of it; in one family with which he is acquainted, where the father has hare-lip, two of the children are affected with this anomaly, one having single and the other double lateral fissure. We have heard it stated, that in Hunter's celebrated case of hypospadias, where impregnation was effected by a syringe, the paternity was assured by the existence of the same malformation in the offspring.

In concluding this part of our subject, we may remark, that though a violent shock, or even anxiety of mind, has apparently disturbed the progress of development in some cases, yet there is no reason to suppose that any slight or momentary influence can have any such effect. It is contrary to science and reason to suppose that any object merely seen, dreaded, or wished for by the mother, perhaps during the latter months of pregnancy, can in any way affect the child in her uterus, or be depicted on its body—a prejudice which is as dangerous as it is ancient.

M. Isidore St. Hilaire divides the various kinds of malformation into four groups, which are not characterized, according to the plan of most preceding authors, by any supposed alteration in the nutritive process, as excess or default of development, but are founded on simple and obvious characters, either referrible to the degree of unnatural change or to the region or system of organs affected.

1. The first group to which he confines the term of anomalies, comprises those varieties or defects of conformation which are simple and uncomplicated, often not apparent externally, and though in most instances congenital, yet not necessarily so in all, as in the cases of giants and dwarfs, which our author has included here, and which arise from some irregularity in the growth of the body, which continues to act after birth.

2. The second group consists of those congenital changes in the situation of organs, in which the *relative* position and connexions of the parts are not altered. A great number of organs may here deviate from the specific type without the performance of their functions being in any way impeded. In man, and in all the higher orders of animals which are symmetrically formed, this anomaly is confined to transposition of the viscera; but in some of the inferior beings which are unsymmetrical, all the organs of the body are transposed: thus in those testaceous mollusca which are furnished with a spiral shell, the spire, in a case of *heterotaxy* (the name given to this group), will turn in an opposite direction to the course which it naturally follows.

3. The third group includes the various forms of *hermaphrodism,* in which both sexes are either present in one individual or some of their characters.

4. The fourth and last group comprises all those congenital anomalies which are complicated, many organs being seriously altered, both in their anatomical and physiological relations, producing great external deformity. St. Hilaire restricts the title of monstrosity to this group.

In the brief analysis which we shall now give of the details of the work, we must altogether pass over many of the classes and orders into which these groups are subdivided, and confine our attention to those which are most interesting, and which throw most light on the laws and causes of their production.

The first group, that of simple anomalies (*hémitéries*), is divided into five classes, which are based upon the kind of malformation with which the part is affected: thus, either the volume, form, structure, disposition, or even the number and existence of organs may be altered.

The first class consists of those cases in which there is general or partial increase or diminution in the size of the body. It includes all the forms of giants and dwarfs, and a great number of other anomalies which do not possess much interest, as unnatural development of muscles, extreme narrowness of the vagina, abnormal increase or diminution in the volume of the mammary gland, &c. Our author has given a long and interesting history of giants and dwarfs, to which we must refer the reader, as it would take up too much of our room to dwell upon it here, and oblige us to omit much more important matter; in fact, it is very doubtful whether the greater number of these cases can be properly included at all among true monstrosities, as they evidently arise from causes altering the process of nutrition after birth. Dwarfishness is almost always the result of disease during infancy, particularly rickets, some traces of which affection are exhibited by almost all these diminutive individuals, as unnatural size of the head, with disproportionate shortness of the legs, curvatures of the limbs, &c. In some cases, however, diminution of size seems to arise from causes producing imperfect nutrition of the fœtus in utero, as malformation of the uterus and disease of the embryo itself. Dwarfs are exceedingly rare among wild animals, a fact which may partly be referred to their immunity from rickets.

The causes which lead to unnatural increase of development and the production of giants are wholly unknown, but we may remark that individuals who exceed the usual limits of their species are more rare among animals than those of diminutive stature.

Together with the description of giants and dwarfs are included an account of many curious cases which have been met with of precocious development of the physical and moral powers, and particularly of the generative system. Many instances have occurred, in which children of three and four years old have presented all the signs of puberty. This unnatural state of the organs of generation is generally accompanied with premature increase of the general volume of the body, and in the male subject with other signs of virility, as the development of hairs, muscles, alteration of the voice, &c. When the sexual apparatus has thus early arrived at its perfect state, it frequently happens that the general growth from that time becomes retarded or stops altogether, these forward individuals not becoming giants, but even remaining below the ordinary dimensions; so that, though remarkable during infancy for their great bulk and stature, they become peculiar afterwards for the smallness of their size.

As, in those cases which prematurely arrive at manhood, growth generally stops, so it has been observed that in giants, where the process of development is carried to an unnatural extent, and continued for an unusual length of time, many of the characters of infancy are preserved to adult age, and the phenomena of puberty are imperfectly or tardily shown: so that, as St. Hilaire has remarked, an intimate relation seems to exist between the evolution of the generative organs and the general growth of the body.

The second class, which includes the various anomalies of form, is closely connected to the last, for the simultaneous existence of several partial alterations of volume, some by diminution, and others by increase of size, must naturally change the shape of organs. There is, also, a great affinity between this class and the fourth, which includes the alterations of position; for the situation of parts cannot well be altered (particularly those that are external) without changing the form of some portion of the body: thus, in club-foot there is a combination of these two classes of anomalies. Varieties in the conformation of parts are more common than any other kind of organic deviation; and there is no region or organ which may not present numerous modifications of external form. Though so frequent, these anomalies present few remarkable peculiarities, and consequently are of little interest. The alteration in the shape of the heads of idiots and cretins is an example of these malformations.

Our author passes them over very quickly, and goes to the next class, which includes the various changes of structure or intimate composition of parts. The most interesting anomalies found here are the varieties in the colour of the skin, arising from deficiency or excess of the pigment or colouring matter deposited in the rete mucosum of Malpighi. Perfect *albinism*, in which there is complete absence of the pigment, is much more frequently observed among the dark inhabitants of hot climates than the fairer races of cold countries. Albinos are most common in Africa among the negroes, and after them among the inhabitants of the isthmus of Panama. These peculiar individuals are generally of a delicate constitution and badly proportioned, their heads and hands being too large; their physiognomy is without expression and disagreeable, from the eyes being intolerant of light, half shut, and having a constant twinkling or oscillatory movement, arising from the complete want of pigment or colouring matter in them, which also causes transparency of the iris, with a red appearance of that organ and the pupil. In consequence of the iris not intercepting any of the rays of light which enter the eye, vision is very imperfect, except in the dusk. Albinos generally possess a lower degree of intelligence than the rest of their race; and among the negroes they are despised and badly treated. It has been observed among the negro race that albinism is more frequent in women than men; and females possessing this defect may produce children, by black men, which are either pied, entirely white, or wholly black. With respect to the causes of this anomaly, it must be referred to simple arrest of development; our author says,

> "We know that the pigment is wanting in the fœtus up to a very advanced period of intra-uterine life, and that even in black or dark people the integument remains for some time after birth of the same colour, as in the children of fair men. We can easily conceive, therefore, how the skin can stop in the series of its stages of development, before the period when in the natural order of formation the pigment is deposited in the mucous layer, and consequently it will remain uncoloured. The colouring matter of the skin and hairs, the iris and the choroid, may thus be deficient in an individual (independently of any pathological alteration), in the same manner as any organ or part of an organ may be wanting from arrest of development." (Tom. i., p. 319.)

St. Hilaire further remarks:

> "If any doubts remain regarding this explanation, I may remark that the absence of the pigment is not the only condition of fœtal life which is preserved in albinism. We know that the child, during the second half of intra-uterine existence, has the skin covered with down; this down is frequently preserved in albinos, particularly in those of the isthmus of Panama; lastly, the persistence of the membrana pupillaris, in some of these cases, beyond the ordinary term of its existence, is another equally evident proof of arrest of development." (p. 320.)

Albinism is frequently seen among animals, particularly those in domestication: white horses, pigs, rabbits, ferrets, cats, &c. are daily met with, some of which are perfect, others only partial albinos.

The presence in the rete mucosum of an unnatural quantity of dark coloured pigment constitutes the opposite anomaly to albinism, and is named *melanism*. Perfect melanism has very rarely, if ever, been observed in the human subject; no perfectly authentic instance is recorded; but among animals and birds it is commonly seen; black deer have often been found in a wild state, as well as some of the larger carnivorous animals; black sheep, rats, mice, &c. are not uncommon. Though complete melanism is unknown in the human race, partial melanism is very frequent, for to this anomaly must be referred many of the congenital spots and marks which are denominated nævi, moles, &c., and which are often confounded with the red vascular tumours or spots which arise from malformation or disease of the cutaneous vessels. These melanotic spots or patches, which are so frequently seen, particularly on the face and back, are very variable in their form, colour, and appearance; they may be elevated above the skin, or be even with its surface, smooth or covered with hairs, &c. With respect to the causes of this anomaly, it must be referred to true excess of development, arising from some unknown influences.

The fourth class of simple anomalies includes a great many interesting malformations, as changes of position, or displacement of organs, and alterations of connexion, some parts which are commonly separated adhering together, and others naturally connected remaining separate.

Organs are liable to anomalous changes of position in proportion as they are loosely connected with the surrounding parts, particularly at the early periods of their formation. Thus the walls of the splanchnic cavities are much less subject to alterations in the position of their component parts than the viscera contained within them, which float as it were loose, and some of which naturally vary slightly in situation at different periods of development. The viscera are in some cases transposed from one part of their natural cavity to another, while in other instances they may be transported into another cavity, or even become external. In the last case the anomaly is called a congenital hernia. The brain may be variously displaced in this manner, it may even pass out of the cranium through any of the sutures. M. Serres has seen the brain protrude in the median line, between the right and left halves of the ethmoid and sphenoid bones, so that some portions of it descended with its membranes, through the base of the cranium, into the nasal fossæ, and even into the pharynx. This fact is explained, according to M. Serres, by the law of eccentric development, which supposes that each organ, or part seated in the median line, is formed by the union of two lateral halves, originally distinct and separate.

The situation of the heart may be altered in various ways; one of its most interesting anomalies is where this organ is placed in front of the neck, immediately above the chest. Although rare, this malformation has been observed in man by Vaubonnais, Walter, Breschet, and others. This position of the heart realizes one of the natural characters of fishes, and is also a normal condition of the early periods of uterine life in man. It results from this displacement, that the aorta must first descend instead of ascending, and it will form no arch, which is also the case in the embryo.

With the unnatural separation of parts are included those anomalies in which various orifices and canals, usually closed after birth, remain persistent, as the urachus, ductus arteriosus, foramen ovale, &c. Cases in which the urachus has remained pervious to adult age are not very rare. When this canal only exists in part of its extent, nothing indicates the anomaly; and should it even continue to the umbilicus its presence will be unknown so long as the normal urinary passages are free; but if they become obstructed by any disease or malformation, the total or partial excretion of the urine will take place by the navel, and the defect is apparent.

The ductus arteriosus has been found in its original pervious state in many instances. This anomaly may exist independently of any other malformation of the organs of circulation, but it is generally complicated with other defects, as an aperture between the two auricles or ventricles, or even absence of the inter-auricular and ventricular septa, so that the four cavities of the heart are converted into two. The origins of the pulmonary artery or aorta may be transposed, one vessel communicating with both sides of the heart, or both vessels with one ventricle. The commencement of the pulmonary artery has even been found entirely obliterated in some cases of persistence of the ductus arteriosus.

> "These different defects of conformation," says M. St. Hilaire, "have most of them been observed, independently of the presence of the arterial canal; but more than one of them usually exists in the same subject, which may be combined with any of the others in various manners, giving rise to different states of the circulating system, which resemble the various conditions of fœtal life, and, more or less exactly, some of the characters which are presented by the lower vertebrated animals, particularly reptiles." (Tom. i. p. 563.)

Any of these malformations of the heart will produce the disease called cyanosis, which results from the communication between the arterial and venous systems.

All the organs seated in the median line are liable to be separated into two lateral halves, in consequence (as it has been said) of arrest of development. The bones offer numerous examples of this anomaly. Fissure of the sternum has often been met with, giving rise, in some cases, to hernial displacement of the heart. Partial division of the vertebræ is a very common defect, and is known by the name of spina bifida. This term is generally restricted to those cases in which the fissure only extends through the posterior part of the rings of the vertebræ; the spinous processes are here mostly divided, but they may be entirely separated from the rest of the bone, and be removed to a distance, so that the posterior wall of the spinal canal is partially or entirely wanting. The fissure may extend completely through the bodies of the vertebræ, separating the right side of the spine from the left; this, however, is exceedingly rare. Spina bifida, or, as it is more correctly denominated, spinal fissure, has been almost always confounded by pathologists with hydro-rachis, or dropsy of the spinal canal; it is, however, necessary to distinguish them, for one often exists without the other. Spinal fissure varies greatly, according to the number of bones affected, and the region in which it occurs. The whole of the column, from the first cervical vertebra to the bottom of the sacrum may be open; and in this case the malformation is generally accompanied with division of the cranial bones and entire absence of the spinal cord. In the greater number of cases the fissure is only partial, the lumbar region being most frequently affected. This anomaly is often connected with some malformation of the ventral aspect of the body, as extroversion of the bladder,* eventration, exomphalos, &c. These severe and complicated cases, however, belong to the last group, or true monsters, as many of them are incompatible with the life of the child.

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* The front wall of this organ is here deficient, together with part of the walls of the abdomen; and the mucous membrane of the posterior wall of the bladder projects above the symphysis pubis in the form of a soft red tumour.

Most of the simple anomalies which various organs present may be referred to changes of volume, form, structure or disposition of parts; but some remarkable cases exist in which there is not only a modification in the conditions of existence, but even an alteration in the number of parts, which may be either increased or diminished.

These numerical alterations seem, *primâ facie*, to be clearly defined, and circumscribed within very precise limits; but if we accurately examine the different cases which are comprised under this head, we shall find a great number which may be confounded with, and which even properly belong to, the preceding classes; many may be referred to alterations of volume, while others arise from unnatural connexion or division of organs.

> " When an organ disappears, it sometimes happens that some obscure rudiment of it may be found, by a minute dissection, and thus it has not actually ceased to exist. On the contrary, when supernumerary parts are added to any of the natural organs, anatomical analysis is sometimes capable of showing that there is not any addition of new parts, but only an increased development of those which commonly exist in a rudimentary state." (Tom. i. p. 622.)

There are also a great many cases in which the addition or deficiency of organs may be explained by the intimate union or complete division of two or more parts; but it is very difficult, nay, almost impossible in some cases to distinguish whether there is actual absence of an organ or only the fusion of two together; and whether a part is really double or only divided.

Though many of these anomalies may be thus referred to other classes, yet some still remain which present peculiar conditions, and which seem to result either from the total suppression of some part, or, on the contrary, from the addition of others completely strange to the normal states of organization. These necessarily form a separate and remarkable order, differing essentially from all other simple anomalies, and not capable of explanation by any of the laws which are known in the present state of science: they have been considered by some as incontestible proofs in favour of the theory of original monstrosity; but, as we have shown that many of those cases which primarily seem to belong to this class must really be referred to others, the origin of which are understood, there is little doubt that by the progress of science the nature of all those which remain will be elucidated, without recurrence to the hypothesis of monstrous germs.

The bones, muscles, and other organs present many numerical anomalies. Thus the muscles of the limbs sometimes vary in number; and it is remarkable that the muscles of the arm, forearm, or hand, scarcely ever depart from their normal type by alterations in the number and disposition of their parts, without falling into the natural conditions of the thigh, leg, and foot, and *vice versâ*. An evident analogy, therefore, exists between the upper and lower extremities; and Meckel, who has made many researches on this subject, has also discovered that many relations exist between the numerical varieties of muscles in the human subject and the normal conditions of the muscular system in different animals.

In this place we may mention another curious and interesting fact, which is connected with the subject of alterations in the number of parts. It is well known that in the lower vertebrated animals, and especially the invertebrated classes, many organs will be reproduced after being removed or destroyed. The new organ sometimes perfectly resembles the one which was lost; but in many cases it differs from it in the number of its parts, which are mostly fewer than natural, though in some cases they are augmented. Thus Otto says, that when the tail of a lizard is reproduced the new vertebræ are generally destitute of processes; and St. Hilaire has observed a deficiency of the eminences and horny plates with which the tail is furnished in many of these reptiles. In the lobster and other crustacea, on the contrary, when a claw is reproduced, it is frequently furnished with some supernumerary parts. The duplication and even the multiplication of the tail of lizards, and particularly salamanders, may be artificially produced by cutting off this organ, and dividing the stump into two or more strips, each of which, if kept separate during the process of healing, will elongate and form a perfect and distinct tail.

Supernumerary vertebræ have often been observed in the dorsal and lumbar regions; but we are only acquainted with one authentic case in which the number of cervical vertebræ was altered. Leveling once met with eight in an adult man. This constancy in the number of cervical vertebræ is very remarkable, especially when compared with the zoological fact that no mammiferous animal is known to possess more than seven of these bones.

One of the most common anomalies, by increase of parts, is the addition of supernumerary mammæ. The frequency of this malformation may be referred to the circumstance that, in almost all the mammalia, several of these glands exist which are disposed in two parallel series. Man, then, being provided with but two, forms an exception to the general plan; but by the addition of more, the normal arrangement in other mammalia is represented, and the series of these organs established.

In anomalous cases, the most frequent number is three, but four and five mammæ have been met with. When four exist, they are generally arranged symmetrically, two on each side of the chest; when three or five are seen, the odd one may be placed laterally, beneath one of the others, or in the median line; in the latter case it is always small and rudimentary, which imperfect development St. Hilaire supposes to arise from the arrangement of the mammary arteries being lateral, so that the gland, when placed in the median line, will receive but a limited supply of nourishment. A very remarkable but rare anomaly is the existence of a mamma in the inguinal region: a case of this kind was observed by Dr. Robert in a woman whose mother was furnished with supernumerary breasts. The organ was here placed on the external part of the left thigh, four inches below the great trochanter. Until the time of pregnancy this mamma was taken for a simple nævus, but at this period it began to develop, along with the natural breasts. It acquired the size of half a citron, and secreted milk; and the author says that the child sometimes sucked the inguinal mamma, and sometimes the thoracic. (*Journal Gén. de Méd.*, t. c. p. 57.)

The second group into which our author has divided the various malformations of the animal body is that of ***heterotaxies***, or general transposition of organs. The peculiar characteristic of this group is that the anomaly affects at the same time a great number of organs, or, in other words, is complex, and yet does not interfere with the accomplishment of any function; the first circumstance distinguishes it from simple anomalies, and the latter from true monsters. In the splanchnic inversion (the only kind that can occur in man and other symmetrical beings), all the viscera, both thoracic and abdominal, double or single, have exactly an opposite arrangement to that which constitutes their natural state. All those commonly situated on the right side, as the liver, cœcum, &c. are found on the left, and *vice versâ*, so that the whole of the contents of the splanchnic cavities present exactly the same appearance as the organs when in their natural situation would do if reflected from a looking-glass. This peculiarity of arrangement is so far from producing any inconvenience to the person affected with it that, in almost all the cases which have been observed, the anomaly has not even been suspected during life; and to prove that it does not tend to shorten existence, we may mention the celebrated case of the invalid soldier, in whom this malformation was discovered after death by Moraud, in 1660, and communicated by Méry to the Academy of Sciences. This man lived to the age of seventy-two, and had pursued a laborious occupation.

The mode of origin of this anomaly is exceedingly obscure; and the causes which give rise to it cannot be understood, until we become acquainted with the laws which govern the normal arrangement of parts. St. Hilaire has adopted a fanciful hypothesis, which was proposed by M. Serres, who says that some one organ regulates by its development the situation of all the others. The organ selected for this purpose is the liver, which certainly seems to perform a more important function in fœtal than adult life. In the early stages of the embryo this organ is large, symmetrical, and placed like the heart in the median line. Subsequently, by the unequal development of the two lobes, it is pretended that the arrangement of all the other viscera, both thoracic and abdominal, is determined. When the left lobe contracts, as in normal cases, the aortic side of the heart, the spleen, large end of the stomach, &c. are drawn to the left side, and the small extremity of the stomach, the pulmonary side of the heart, &c. go to the right; if anything alters the mode of development of the liver, the other organs take a contrary position. We confess that we cannot allow to this hypothesis any other merit besides that of ingenuity.

We have already remarked that in those animals which have unsymmetrical bodies, as some fish and many mollusca, the inversion is general, and extends to all the organs, both external and internal; these cases, however, possess no points of peculiar interest, all the phenomena being of the same character as in the partial instances: we shall, therefore, proceed to the next group, which contains the different forms of hermaphrodism.

"An hermaphrodite," says St. Hilaire, "according to the literal meaning of the word, is a being possessed of both sexes, and either able to fecundate itself or alternately to impregnate others, and to be impregnated; two modes of generation, numerous examples of which may be observed within the limits of the animal kingdom. The term hermaphrodite was formerly employed in this sense in teratology, when applied to man. The ancients designated by this name certain individuals, to whom they attributed the marvellous faculty of fulfilling by turns the reproductive functions of both sexes, or at least who simultaneously possessed both the male and female organs fully developed." (Tom. ii. p. 30.)

In modern times, though the term hermaphrodism has been retained in compliance with ancient custom, its signification has been much extended, and it is now used to designate not only an individual in whom both sexes are united, but also one who possesses any mixture of the two characters. St. Hilaire divides this group into two classes: one of which is with, and the other without excess in the number of parts. Thus, in other words, this anomaly may result from the union (always more or less incomplete) of the organs of both sexes in the same individual, some of the parts of one being added in excess to the reproductive apparatus of the other; or the hermaphrodism may consist in the possession of only some of the characters of both sexes, the organs remaining essentially single, but having some parts which resemble the male, and others the female sex. These two classes may be again subdivided into several orders. In hermaphrodism without excess, the generative organs may be essentially male or female, a few parts only presenting the opposite sexual conditions; thus arise the two orders of *masculine* and *feminine* hermaphrodism, which were determined by Ambrose Paré. The sexual apparatus, on the contrary, may present such an association of the two genders, and their characters may be so combined, that the determination of the true sex is either difficult or entirely impossible. This may result from two kinds of modification, which have generally been confounded, and of which St. Hilaire constitutes two separate families; in one case the sexes may be so combined that the organs are really neither male nor female, but may be called *neuter;* while in others the characters are so divided that one half of the sexual organs are decidedly female, and the other male, when they are called *mixed.*

Hermaphrodism with excess in the number of parts may also be divided into several orders. St. Hilaire makes three: thus, if a few female parts are added to a perfect male being, *complex masculine* hermaphrodism is constituted; if, on the contrary, a female possesses some supernumerary male organs, we have *complex feminine* hermaphrodism; thirdly, if the organs of both sexes are combined in the same subject, the order of *bisexual* hermaphrodism is formed, which may be compared to the simple mixed order of the former class. If the characters of both sexes were complete in the last order, the formation of a perfect hermaphrodite would result; but though many such cases are recorded by the older writers, they cannot be considered as authentic, and the existence of such a being among the higher classes of animals must be denied.

The mode of production of hermaphrodism without excess has been explained by the relation existing between the male and female organs of generation; which relation or resemblance becomes very close if we compare them together in the young embryo.

"There is one period in the life of the embryo," says M. St. Hilaire, "when all the parts appear female, and another in which they seem to be all male; so that the similitude is thus complete between the sexes. This anatomical analogy between the

> male and female organs, which was suspected by Aristotle and Galen, and declared by Buffon and many other authors, is now perfectly established, both by the zootomical researches of my father and M. Blainville, and also by the embryological observations of Ferrein, Autenrieth, Home, Ackerman, Meckel, Burdach, Tiedemann, and Serres. It will be found the key to all the anomalous states, which constitute hermaphrodism without excess, and which are inexplicable by any other hypothesis. In fact, if each part of the male apparatus is essentially analogous in its elementary composition to some part of the female, if their apparent difference only results from some variation in the mode or degree of their development, nothing can be more easy to conceive than intermediate states between the two extremes, between those two opposite forms which constitute the normal states of either sex. If, for example, the clitoris be considered as a penis arrested in its development, and, on the contrary, the penis as an hypertrophied clitoris, if in fact the one is the first, and the other the last stage of evolution of organic elements, which are perfectly analogous, we see that all excess of development of the one, all default of the other tends to make them fall into those conditions which are intermediate between the natural states of both. Is it not clear that if this excess or defect of development be carried to a great extent, the clitoris may be changed into a true penis, or the penis be reduced in its composition to the form and volume of a simple clitoris? Thus in the midst of other parts essentially male, a female organ may be found, and *vice versâ*, and that mixture of the sexes may arise which was considered as a prodigy by the ancients, but which is now known to be only the natural result of an excess or arrest of evolution in certain organs."* (Tom. ii. p. 44.)

Another principle by which our author accounts for many cases of hermaphrodism is the division of the sexual organs into six principal segments, which, he says, arise from distinct centres of formation, and which are, to a certain extent, independent of each other; these six segments, three of which belong to each side, are divided into deep, middle, and external.

> "The two deep segments are formed by the ovaries or testicles, and their appendages, the middle ones by the uterus or prostate and vesiculæ seminales, &c., and the external by the clitoris and vulva, or penis and scrotum. We cannot fail to observe that these six segments correspond to six different sets of vessels; the deep organs are nourished by the spermatic arteries, the intermediate ones by the hypogastric, and the outer segments are supplied by the external pudic, receiving some branches as well from the hypogastric." (Tom. ii. p. 50.)

This independence of the different portions of the sexual apparatus, may be shown by many examples; thus it is not uncommon to see one segment undergoing modifications in form or structure, and varying even in existence without affecting the other segments. An individual, for instance, may have the deep-seated organs masculine, while the middle ones are feminine; he will then possess testicles and an uterus. The external parts in this case generally partake of the characters of both sexes, but not necessarily.

Our limits will only allow us to describe briefly the different orders of hermaphrodism.

In the variety termed *masculine*, the organs are essentially male, but undergo some modification which produces a greater or less resemblance to the female sex; thus, the penis may be small, the glans imperforate, the urethra changed into a simple furrow, or altogether obliterated, the testicles imperfectly developed, and the scrotum frequently cleft into two portions, which more or less resemble the labia. The urethra commonly opens into this cleft, which, when deep, may be taken for the vagina; the testicles, in some of these cases, remain in the abdomen till a late period, and may even not descend into the scrotum at all, which will sometimes make it very difficult to determine the true sex. Together with these local characters, the general appearance of the body is mostly altered, and has a feminine character; the breasts are developed like those of a young woman, the skin is soft, and the beard thin or wanting.

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* Dr. Robert Knox, of Edinburgh, states that the sexual organs of the embryo, instead of being at one period male, and at another female, or, as Meckel says, always originally female, are in the early stages of formation, neither one nor the other, but intermediate or hermaphrodite; and he explains the origin of monsters belonging to this group by the occurrence of simple arrest of development.—*Med. Gaz., Jan.* 1839.

Feminine hermaphrodism is marked by characters the reverse of masculine: the clitoris is mostly very much enlarged, and more or less resembles a penis; it may even be furnished with an imperfect urethra, running along its under surface; the vulva is contracted or imperforate, and in some cases the ovaries descend through the inguinal canal into the labia. Together with these alterations, some of the other characters of the opposite sex are generally present, as a deep voice, masculine form, small breasts, &c.

In these two orders the deep or formative organs, as the testicles, prostate, &c. in the one sex, and the ovaries, uterus, &c. in the other, preserve their natural structure, and thus the being is essentially male or female; yet the external parts and general aspect of the body are often so modified, that it becomes difficult, in some cases, to detect the real gender.

Complete *neuter* hermaphrodism, in which all the organs should offer such an equal degree of development between the two sexes that their real nature cannot be determined, even by an anatomical examination, is very rare; partial cases, however, are often met with, the true sex of which it is often very difficult to determine by an external examination; but if carefully dissected after death, they may mostly be referred to one of the preceding orders.

The best marked examples of neuter hermaphrodism have been observed in animals. Sir Everard Home has described a dog which possessed a well-formed vulva, and a large clitoris or imperforate penis, beneath which was an opening leading to the urethra. These external organs seemed to indicate that the animal belonged to the female sex, and so it was considered until the internal parts were examined, when it proved to be neither male nor female. The reproductive organs consisted of a portion of elongated ligamentous substance, which Home considered as an imperforate vagina, of two small solid cords, intermediate in structure and disposition between vasa deferentia and round ligaments; and lastly, of two ovaries or testicles of a very equivocal nature, to which the cords mentioned were attached. These bodies, which Home considered as testicles, were very small, imperfectly formed, and of a doubtful structure; they occupied the position of ovaries. Home has added to these details that this dog never showed any sexual inclinations or heat, and possessed neither externally nor internally any vestiges of mammæ.

Mixed hermaphrodism consists in the union of distinct parts belonging to opposite sexes in the same set of generative organs. This anomaly may be varied in different ways, as either the deep organs may belong to one sex and the superficial to another, or all the segments on one side moy be male, and on the other female. This order must be explained on the principle, which we have already mentioned, of the different parts being separately developed, and independent of each other at the first period of their formation. For the explanation of the latter variety of mixed hermaphrodism, denominated lateral, in which the organs of one side belong to one sex, and those on the other to the opposite, an ingenious hypothesis has been proposed, viz. that it is the product of the intimate union or fusion of two individuals of opposite sexes, one half of each of whose bodies has become atrophied, and in whom the division of the sexes remains as the only vestige of the original duplicity. This idea, though ingenious, is not borne out by any facts.

Hermaphrodism with excess in the number of parts is characterized by the union of two sexes, having separate sets of sexual organs, which may be more or less complete, so that this class presents many degrees; thus to the male apparatus a few only, or nearly the entire set of female parts may be added, and *vice versâ*, giving rise to the two orders of *complex* masculine and feminine hermaphrodism. The third order of *bisexual* hermaphrodism consists in the coexistence of two nearly perfect generative systems of different sexes in the same individual. A most remarkable instance of this anomaly has been recorded by Schrell, a German anatomist. (Vide *Med.-Chir. prakt. Archiv. von Baden*, &c. t. i. 1804.) In this case there existed beneath a true penis, and independently of the testicles and vasa deferentia, which were naturally formed, a small vulva, furnished with labia and nymphæ, and communicating through a true vagina with a rudimentary uterus, provided with round ligaments and imperfectly developed ovaries. Here the two sets of organs were nearly complete, but the male parts were fully developed, while the female remained in a rudimentary state. This case is exceedingly interesting, from being the only authentic instance of the kind that has been met with in man.

Perfect hermaphrodism, where both sets of organs shall be fully developed, is not only unknown among the authentic details of anomalies, but is physically impossible, without great alteration of the natural connexions of the bones and other parts of the pelvis.

We now come to the last and most important group of anomalies, the monsters properly so called, which group includes all the complicated and extreme states of malformation. We shall here find the same facts and same phenomena governed by the same laws as in the more simple anomalies which we have already passed in review.

Zoologists have long since distinguished in the animal series some beings which they call simple, and others which are formed by the aggregation of two or more of the former, which they denominate compound. In the same manner there exist among monsters some which only possess the complete or incomplete elements of a single individual, while others are composed of the parts of two or more beings, united together. Thus St. Hilaire forms the two classes of single and compound monsters. The former class is again subdivided into three orders, as follows:

1. The first comprises those beings which are capable of living, and

deriving nourishment by their own proper organs (*autosites*). These can all exist for a longer or shorter time, after leaving the uterus of the mother; and the malformation only affects one part or region of the body, the others preserving nearly their natural state; the circulating system is more or less perfect, and especially the heart; the lungs, most of the digestive organs, and some part of the head are constantly present.

2. The second order (*omphalosites*) includes those monsters which only possess an imperfect and passive kind of life, sustained by means of the communication with the mother, and ceasing when the umbilical cord is divided. These monsters want a great number of organs, and have those which remain in a very imperfect state; all the external regions of the body are deeply malformed, the symmetry of the two halves is imperfect, and often entirely lost.

3. The third and last order of simple monsters is that of parasites, which are the most imperfect of all beings, consisting of irregular shapeless masses, principally formed of bones, teeth, hairs, and fat; they even want the umbilical cord, and are fixed immediately to the generative organs of the mother, at whose expense they lead an obscure and vegetative existence.

Compound monsters are divided into double and triple, each of which subclasses contains two orders, named *autositaires* and *parasites;* the former of these includes those monsters which are composed of two or more individuals, offering the same degree of development, and contributing to the maintenance of the common life. A great many genera are contained in this order, some consisting of monsters completely double; others of those which are only half double, or even single in the greater number of parts.

The second order, that of compound parasitic monsters, contains those beings which are composed of two or more very unequal and dissimilar individuals, one being nearly or quite complete, and the other very small and imperfect, and parasitically attached to the first, of which it forms a mere appendage.

To revert to the subject of *single monsters.*

> "The recent progress of embryology," says M. St. Hilaire, "enables us to distinguish three successive stages or phases in the life of the embryo, which are of very unequal duration; the first is very short, in which the embryo, scarcely formed, is fixed directly to the walls of the uterus. In the second, the embryo is distinctly shaped, and is furnished with an umbilical cord. In the third and last stage the embryo or fœtus, as it is now mostly termed, is completely developed, and capable of leading an independent existence by means of its own organs. These three stages evidently have their representatives in the three orders of single monsters, the two most imperfect of which arise from arrest of development taking place at a more or less early period, and to a greater or less extent." (Tom. ii. p. 196.)

Those simple monsters which are capable of supporting an independent existence, may be divided into several tribes and families.

1st. The trunk may preserve its natural form while the limbs are greatly altered; thus they may be entirely suppressed, or two of them may unite into one, as in those monsters termed *symeles* or sirens.

2d. The body may be malformed, and the limbs nearly natural. In this family extensive congenital hernia, or eventration of a great number of viscera, is generally observed. The head both in this, and the former tribe, is slightly, or not at all altered, while in the subsequent divisions it is the organ principally affected.

3d. In the third tribe the face is natural, or only slightly altered, while the cranium and brain are greatly malformed. Thus the brain may exist in a more or less incomplete state, but be situated partly or wholly without the cranial cavity, the walls of which are imperfect. In some cases the brain is entirely deficient, its place being partly occupied by a bright red coloured tumour, which is composed of a number of small vessels. This body lies on the base of the cranium, the upper part or vault of which is nearly all wanting. In other cases the whole brain and cranial arch are undeveloped, and this vascular tumour is not found.

4th. In some monsters the face is more extensively deformed than the cranium. In these cases there is generally atrophy of some of the central facial organs, and approximation or union in the median line of some of the lateral organs, as the eyes and ears.

We shall make but a very few observations on the monsters belonging to this order.

In those named *celosomi*, in which the trunk is malformed, we have remarked that eventration, or displacement of the viscera, constantly occurs; and it has been observed that this malformation realizes, with some modifications, those organic conditions which naturally exist in the early periods of uterine life. In fact, it may represent any of the stages through which the embryo passes, from the primitive state, when all the viscera float loose and are contained in the sheath of the umbilical cord in front of the yet open cavity of the abdomen, to the last period in the formation of the fœtus, when the abdomen is completely formed and closed, except at the point where the umbilical cord is attached. In some of these monsters the funis itself is very short and imperfect, so that the fœtus is fixed close to the placenta; and is, consequently, placed against the walls of the uterus. St. Hilaire is of opinion, that the movements of the limbs may be thus impeded, and deformity of them occasioned, such as is frequently observed in these cases.

If we examine the red tumour which is found occupying the situation of the brain in some cases of malformation of the head, we shall find that it is composed of three parts; first, of vessels which form the constant and principal bulk of the tumour; second, of a collection of serous fluid, which is generally but not always present; third, of vestiges of nervous matter, which are only met with in a few cases. The body thus formed is doubtless the result of atrophy of the brain, and of excessive hypertrophy of the pia mater, and of the intercranial or meningeal vessels, which, instead of being dispersed on the surface, and in the interior of the cerebral organs, become agglomerated into a considerable mass, and form a sort of brain composed wholly of vessels.

Teratology throws considerable light on, and is able in some cases to clear up disputed points in physiology. It has been said that the cerebro-spinal axis is the sole seat of life in the higher animals. It is, therefore, interesting to enquire, whether those monsters which are deprived of the greater part or the whole of the brain and spinal cord, as in the *anencephali*, whose nervous system is reduced to the state met with in insects and worms,* can support an independent existence or life after being separated from the mother.

> "All doubts on this subject," says our author "have been long removed by the record of many authentic facts. The first monster of this kind which was met with by M. Vincent Portal, lived a quarter of an hour, and was violently convulsed; it would probably have existed longer had it not been dropped by the nurse. In the case of M. Fauvel, life was prolonged for two hours, with evident signs of sensibility. The monster observed by M. J. J. Sue moved in several directions, and lived for seven hours; that of Malacarne for twelve, and the one of Méry for twenty-one hours, and took some nourishment. Lastly, this is not the longest period to which life may be prolonged in these imperfect beings; for an anencephalous fœtus, born in 1812, at the Hôtel Dieu at Paris, and which was seen by M. Serres, then inspector-general of this hospital, lived three days, and was fed with milk and *eau sucrée*, no nurse being found willing to suckle it." (Tom. ii. p. 371.)
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> "In reviewing these facts we may observe, that in those monsters which are destitute of brain and spinal cord, as well as in others of the same family in which the brain only is deficient, the spinal marrow existing; during intra-uterine life the malformation does not exert any injurious influence on the development of the embryo; these monsters, up to the time of birth, are strong and healthy; but when they are transported into an external world, which is not adapted to their mode of organization, and are obliged to respire atmospheric air by lungs which are not influenced by the action of nervous centres, they languish and quickly perish. In the same manner as a healthy fish, taken out of the water, dies of asphyxia, in a medium which, though it animates us, is deadly to it: in the same manner also as an embryo dies which is born long before its natural time; so the anencephalous fœtus is necessarily doomed to a more or less speedy death, not because its organization is essentially vicious, but because being only fitted for the conditions of intra-uterine life, it is not capable of supporting a free and independent vitality." (p. 372.)

The cases related by St. Hilaire of anencephalous fœtuses (he includes under this denomination those monsters which are entirely deprived of brain and spinal marrow), which have lived and breathed, even for several days, are exceedingly interesting, if their perfect authenticity may be relied upon; if it was clearly ascertained that no portion of the brain or medulla oblongata existed. For it has always been considered by physiologists that the function of respiration depends on the presence of the medulla oblongata, from which the pneumogastric nerves arise; and it is not easy to conceive how this function can be performed, even for the shortest period, without its influence. It is possible that in these cases some portion of the cerebro-spinal axis was still present, though much displaced and altered.

The second order of simple monsters, which includes those that are altogether incapable of living out of the uterus of the mother, (*omphalosites*,) has been divided into two tribes or families. In the first, the head is nearly or entirely wanting, and the body much malformed and irregular, but yet showing some tendency to preserve a symmetrical form, and inclosing the viscera. The genera *paracephali* and

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* The analogy is here not correct: see note, p. 6.

acephali belong here, the former of which shows some vestiges of the head, while, in the latter, it is entirely deficient. The other tribe, which contains the *anidiens*, is little known; the body here is still more imperfect than in the former tribe, and no longer includes the viscera, being almost reduced to the state of a cutaneous sac.

The last order of simple or more properly speaking, single monsters, the *parasites*, contain a much greater number of cases, though it is still less understood than the former order. The objects referred to this family are generally called moles, and consist of different organic parts, as teeth and bones, united into an irregular and shapeless mass, which may be found in the uterus, ovaries, or even in other situations. The history of these singular productions is very obscure, and their origin cannot be completely understood; but it seems in some cases, at least, that these parts developed in the uterus or ovaries are the products of conception which have remained exceedingly imperfect, new beings which have commenced under, or at a very early period of their formation have been subjected to, anomalous influences which have drawn them in an unnatural direction. One objection to this mode of explanation is, that tumours containing bones, teeth, hair, &c. have been found in the ovaries of virgins, and of girls before puberty; but these cases, which are very different from the preceding, evidently belong to another class, that of monstrosity by inclusion, and must be placed among the compound monsters which we shall next consider.

We have already stated that double monsters may be divided into *autositaires* and *parasites*. The first of these divisions includes several tribes and families. Thus the two subjects which compose the monster may be both nearly perfect and distinct, only adhering together by one region of the body, and having the component parts of each almost complete even at the point of junction. As an example of this tribe, we may mention the celebrated case of double female, which was born in Hungary in 1701, and which was christened by the two names of Helen and Judith. This monster was shown about for seven years in almost all the countries of Europe, and lived to the age of twenty-two years: the best description of it is given in the Philosophical Transactions, by Torkos. The two individuals were here situated back to back, and united by the buttocks and part of the loins. The external organs of generation offered evident signs of duplicity, but there only existed a single vulva, situated inferiorly and hidden between the four thighs; the vagina was at first single, but soon divided into two distinct canals, and all the other sexual organs were distinctly double. In the same manner there existed two distinct intestinal canals, which terminated by a common extremity. The two vertebral columns were also united by the second piece of the sacrum, and they terminated in a single coccyx. The two aortæ and venæ cavæ united inferiorly by their extremities, and thus established a large and direct communication between the two hearts, which produced a community of life and functions, the source of several very interesting physiological and pathological phenomena.

The two sisters were very different in some points of their characters.

One was much larger and more healthy than the other, as well as more intelligent, though they both spoke several languages; they menstruated at different periods, though there was but a common external genital orifice. The desire of passing the fæces was felt by both of them simultaneously. Their mental and nervous systems seemed to have little communication, for one often slept while the other was awake, and one was sometimes occupied in reading or writing while her sister was in a state of repose. Whenever one was ill, the other soon felt so too, and participated in her sister's disease; it was therefore predicted, and it proved to be too true, that the death of one would necessarily destroy the other. At the age of twenty-two, Judith sunk under disease of the brain and lungs; Helen was seized some time after the commencement of her sister's illness with a slight fever, and suddenly sunk into a state of collapse, preserving, however, her intellectual faculties. After a short struggle, she fell a victim to the death of her sister, and they both expired almost at the same instant.

Another well known instance of double monstrosity, analogous to the last in many respects, but differing from it, however, in some particulars, is the case which was exhibited in London in 1829-30, and denominated the Siamese twins. The junction of the two individuals was here by the ventral aspect of the body, and the monster was furnished with a common umbilicus, whereas, in the former case, there were two distinct cords. For the description of this case we must refer the reader to the periodical medical works of 1830: we have given a brief notice of it in our Second Volume, p. 299.

In the second tribe of double monsters belonging to this class, the component individuals are distinct and separate at their pelvic extremities, but more or less intimately united by their upper halves. In some of the genera of this order the two bodies are completely distinct below the umbilicus, and are surmounted by a compound head; or, in other words, by two heads intimately united together, some of the parts of which are atrophied. St. Hilaire calls these *sycephalous* monsters. In other genera, the two bodies which are separate below are furnished with a single and simple head, in which a very few traces only of duplicity can be discovered. These have been named *monocephalous* double monsters.

Our space will not allow us to enumerate any examples of the malformations belonging to this tribe, which have been principally met with among animals.

The third and last tribe is marked by exactly inverse characters to the preceding; the upper half of the body is double, while the pelvis and inferior extremities are nearly or quite single. Many cases belonging to this family have been met with in the human subject, and we may particularly mention one which excited a great interest in Paris in 1829. This double being, which was a female, and called by the double name of Rita-Cristina, was born in Sardinia, and lived to be nearly nine months old. It was carefully examined after death, and the two vertebral columns were found quite separate in their whole length, and a rudimentary pelvis formed of a single bone separated them inferiorly. Another fully-developed pelvis was situated in its natural position, and supported two well-formed abdominal limbs; the ossa innominata were widely separated posteriorly, so as to include between them the two sacra and the rudimentary pelvis; there existed a single bladder, uterus, and rectum, which were common to the two subjects, but behind these organs were found rudimentary traces of others. There were two distinct hearts, and all the other thoracic and most of the abdominal viscera were double. Many interesting observations were made on this monster during life; the nervous systems seemed to have but little communication, except in those parts which were in the line of union, as the anus and sexual organs; for if the right leg was pinched, Rita only felt it, and if the left, Cristina; thus, of this common pair of limbs, the right seemed to belong to one individual, and the left to the other.* It may be mentioned, that an undeveloped rudiment of a third limb was found in the axis of union of the two innominata, attached to the rudimentary pelvic bone. These two beings experienced the sensation of hunger separately, though they felt the desire to expel the fæces at the same time; this may be explained by the structure of their alimentary canal, which was found to be double as far as the commencement of the ilium, and single in the rest of its course.

The class of double parasites includes those monsters in which one being more or less imperfect is attached as an appendage to the body of another complete individual. In the more highly-developed cases of this anomaly, the accessory being is attached to the external surface of its companion, but in the last tribe the parasite is inclosed and completely hidden within the body of the principal subject.

Monstrosity by inclusion is such a singular anomaly, that it has excited much interest, though at present it is but little understood. The two principal situations in which the inclosed being has been found are in a pouch beneath the skin, or in the abdominal cavity of the perfect individual. In the former of these cases the parasite, though included, is not entirely hidden; for it often forms a considerable projection or tumour in the region where it is placed. The organization of the inclosed fœtus is always very simple and imperfect, yet it may vary very much in this respect; in some cases it may resemble a paracephalous or acephalous single monster, while in others it only consists of a shapeless mass of bones, teeth, and hair, as in the simple parasites named moles. The principal being which contains the parasite is generally well formed in most of its organs, though it is seldom quite perfect in all.

"All the explanations which have been proposed for this anomaly may be referred to five principal heads, viz.: the original inclusion of one ovule within the other, and their simultaneous fecundation; the formation of one ovule from two germs; the inclusion of an ovule or very young embryo in another embryo previously conceived, and already more or less developed; the inclusion of one ovule or embryo in another, which has been conceived at the same time; and, lastly, the production of the inclosed fœtus by the other individual." (Tom. iii. p. 321.)

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* Upon dissection, this was found to be the case; the spinal cords were quite separate, and there was no communication between the nerves forming the nervous trunks going to the two abdominal limbs; the only union between the nerves of the two beings, was found in the parts in the line of junction.

Triple monsters are so rare, and so little understood, that there is little to be said about them; their extreme rarity is easily accounted for; the formation of a triple monster requiring two conditions, viz., the simultaneous presence of three embryos in the uterus, and the union of all three together into a compound fœtus. In the human species we know how rare cases of triple birth are, and in those which do occur the children may be disposed in three modes: they may be all separate, as in the normal and common instances; two of them may be united together, the third being free, a disposition which has been observed in a very small number of cases; thirdly, the union of three subjects into a single being constitutes the last of these three modes, and is necessarily the most rare of them, for it results from the coexistence of two monstrous unions, and not of one, as in the former case. Triple monsters have been so seldom seen, that their existence has been doubted by some authors, among whom we may mention Chaussier, Adelon, and Meckel; but St. Hilaire says, that he himself has seen three cases of this anomaly.

The three individuals composing a triple monster are always united in one of the two following ways: they are either all joined together, meeting at the same point, which forms a common centre, or one individual is united to a second, which, placed in the middle, is joined in its turn to a third. These two forms are evidently very distinct, and they have both been realized by authentic observations, which have most of them been met with among animals, in whom this anomaly is not quite so rare as in man.

We must here conclude our notice of M. Is. St. Hilaire's work, which is to be regarded in the light of a compilation, in which the author has most industriously collected a multitude of facts and references, which he has applied to those laws which have been founded or discovered by his father and other naturalists. We earnestly recommend its perusal to all those who take an interest in the higher branches of physiology and philosophical anatomy. The book, however, is not without numerous faults: it has been spun out to an unnecessary length, and is often deficient in clearness of arrangement; the general conclusions, as the laws and causes of anomalies, for instance, though nominally placed together in a distinct section, will be found in many instances scattered through various parts, and can only be collected with difficulty. The pages are often disfigured with those defects which are certainly more common in continental literature than in our own, viz., prosy tediousness of detail and endless repetitions; yet, with all its faults, this work must be looked upon with great satisfaction, as filling up a gap in natural and medical science.

ART. II.

1. *The Philadelphia Practice of Midwifery.* By CHARLES D. MEIGS, M.D., Lecturer on Midwifery and of Diseases of Women and Children, &c.—*Philadelphia*, 1838. 8vo. pp. 370.
2. *Mémoires de Médecine et de Chirurgie pratique sur plusieurs Maladies et Accidens graves qui peuvent compliquer la Grossesse, la Parturition, et la Couche. Précédés d'un Compte-rendu analytique de Maladies observées à l'Hospice de la Charité de Lyon pendant un Exercice de sept Années.* Par le Docteur MARTIN, le jeune, &c.—*A Paris*, 1835. 8vo. pp. 462.

Essays in Practical Medicine and Surgery, on various Diseases and important Circumstances complicating Pregnancy, Labour, and the Puerperal State, &c. By Dr. MARTIN, jun., &c.

WE have joined these two works together in one article, as well because the subjects of them are similar, as because they claim and shall receive from us the same kind of notice. In reviewing them, we shall only advert to such points of science or practice as are important, and on which right views are essential to the obstetrical student. In all our reviews of books, our main object is to be useful to the practitioner: mere criticism when divorced from its legitimate end, improvement, we hold in slight estimation; and so long as we keep this end in view, we care not how discursive we seem.

We shall treat of the two works included in the present article, in succession, beginning with that of Dr. Meigs.

I. The Philadelphia school of midwifery has for many years been looked upon with great respect by the obstetricians on this side of the Atlantic; the high name and professional standing of Dr. Dewees, his great experience, and, above all, his inestimable Compendious System of Midwifery and other valuable publications, have mainly contributed to this result. Dr. Meigs's volume is upon a much smaller scale than that of his distinguished countryman, but it contains much which is valuable and instructive to the student. The subjects are arranged in the usual order of works of this kind; beginning with the anatomy and physiology, and ending with the practical portion which comprehends the larger part of the book. Between these two main divisions, the author has introduced four chapters on Menstruation, Amenorrhœa, Dysmenorrhœa, and Leucorrhœa.

1. In enumerating the diameters of the pelvic brim and their dimensions, Dr. Meigs appears to have accidentally transferred the measurement of the transverse diameter to that of the oblique: he states that the transverse diameter of the brim measures from four and a half to five inches, and that the oblique diameters are five inches. The most correct measurements of the pelvis, show that the oblique diameters are at least half an inch shorter than the transverse in the skeleton state, and taken in the French inches, which are the standard most usually adopted on the continent, they run in the following order: antero-posterior diameter, four inches; oblique diameter, four inches and a half; transverse diameter, five inches. These are looked upon as the correct dimensions of the brim

both in France and Germany, and, bearing in mind that the French inches are rather longer than our own, we shall come very nearly to the truth by making a small addition to each admeasurement. The numbers in French inches are peculiarly easy for the student to recollect, the antero-posterior, the oblique, and transverse diameters of the brim differing from each other by a successive increase of half an inch. It is to be regretted that the reader has not been made aware of this fact; the discrepancy in the admeasurements of different authors would have thus been explained. The four French authors whom Dr. Meigs has quoted give the dimensions which we have above mentioned: we may also observe that the same dimensions are given by Naegele, Froriep, and, with the exception of the antero-posterior diameter, by Meckel. It would be curious to ascertain, on what depended the difference in the measurements of the antero-posterior diameter of the brim, by Meckel and by Dr. Burns. The former states it to be four inches four lines French measure, whereas Dr. Burns, in English inches, which are shorter, states it to be only four inches. Our own opinion is, that the correct admeasurement lies between those extremes, and that it is really four inches French measure, as stated by Baudelocque, Boivin, Capuron, Gardien, and the German authorities above alluded to.

2. In speaking of the perineum and fourchette, Dr. Meigs denies that the latter is generally torn during labour; we must confess that we cannot agree with him on this point, and that the fourchette, in spite of all possible care in supporting with the hand, has usually been torn in the first labours. On the other hand, he observes very justly, "Lacerations do not always commence at the fourchette. I have already mentioned a case in which the lower third of the right labium was broken off, and an irregular lacerated wound extended from that point towards the perineum. The accident cannot always be avoided even by the greatest care and skill." (p. 33.) "In other instances, the child has been expelled through a laceration of the perineum proper, not including the fourchette or any part of the vulva, the perforation being made between the anus and vulva." (p. 33.) This latter form of laceration is occasionally though rarely observed, but the rupture of the labium is very rare and appears to result from the unusual rigidity of the perineum, directing the head forwards with so much force against the labia as to produce laceration; we know a case of this sort where a similar injury occurred to the left labium from the firmness with which the perineum was supported, and the unusual smallness of the os externum. The author, in stating this case of ruptured labium, has allowed a little inaccuracy of printing to escape him, which prevents us ascertaining on which side it really took place. At p. 23, he says it was the left, but at p. 33, he alludes to the same case as the *right* labium. In the case to which we have referred it was the *left*, and it would be curious to ascertain whether this was also the left. Would the rupture of the left or right labium depend on whether it was the first or second position of the head; the occiput, in the one case, advancing under the left ramus of the pubic arch, and under the right ramus in the others?

3. We cannot possibly agree with the author in yielding to the long exploded and, we trust, almost forgotten notion, that the gravid uterus when it rises out of the pelvis, "rests on the brim of the pelvis," (p. 34:)

the brim of the pelvis is far enough off the uterus at this time, and has nothing to do with its support.

4. His observations respecting the functions of the vagina are equally objectionable, " it (the vagina) is surely not areolar" (misprinted, we presume, for muscular), "and possesses no other contractility than that which is called elastic, and which is common to the whole of the cellular structure. It closes speedily after the passage of a child, even one of very large size. In some instances where the child's head has lingered long in the vagina, an hour or more elapses before its caliber becomes much contracted: for some hours after the birth of a child, the introduction of the hand into the vagina may be effected with the use of very little force." (p. 35.) Has the author never observed the force with which the placenta or coagula are expelled at moments when the uterus has been perfectly empty and contracted upon itself? Has he never observed the same, even in a more striking manner, when introducing a pessary? Is not the uterus, at term, a muscular organ? and is not the proper or fibrous coat of the vagina a continuation of that of the uterus? Is not the sympathetic action between the vagina and the abdominal muscles, by which they are called into such violent activity when the vagina becomes distended during the last stages of labour, the same as that which exists between them and the rectum? How will he account for the expulsion of an aborted ovum from the vagina? and still more, how could he explain the spontaneous delivery of the head in those (fortunately rare) cases where it has been born and separated from its body and left in utero, if the vagina " possesses no other contractility than that which is called elastic?" The vagina is surely a muscular canal capable of exerting very considerable expulsive power in aid of the uterus, and this has been and is the opinion of all the first anatomists and accoucheurs of Europe.

5. Dr. M.'s remarks on the changes which take place in the ovaries after impregnation, are very incorrect, and betray a greater want of reading on these subjects than we could possibly have expected. " When fecundated, these ovules, called also Graafian vesicles, are removed from the ovary by unknown processes, and, after passing through the canal of one of the Fallopian tubes, are deposited in the cavity of the womb, in order there to undergo the changes that are described under the head of pregnancy. When a Graafian vesicle leaves the ovarium, there remains in that body a yellowish red spot, which from its colour has received the name of corpus luteum." (p. 41.) That it is the ovum which is contained in the Graafian vesicle, and not the Graafian vesicle itself which quits the ovary and passes along the Fallopian tube into the uterus, is a fact so well ascertained as to require no comment from us.

6. The author's remarks, a few pages further on, in his chapter on menstruation, are not more successful. " Writers," he observes, "speak of a greater redness and fulness of the womb and the ovaries, as occurring during the catamenial flow; to all such the question might be very properly addressed, who has seen the womb and ovaries in this predicament?" (p. 49.) We presume that the fact of the uterus and its appendages having been repeatedly seen at these periods, must be well known to most of our readers, and the above remark is the more surprising, since accurate drawings of the appearance which the uterus presents at these periods have been published, and preparations of the uterus, beautifully

injected to show the general turgor of its vessels, exist in different museums.

7. Dr. Meigs considers "that cold countries are not so favorable to the increase of population as the milder regions of the south. The reproductive faculty is not so vigorous, where, in consequence of protracted and severe frosts, the productions of the soil are not sufficiently abundant to support an immense population." (p. 54.) This opinion at the first view would appear very plausible, but the result of extensive and accurate observations by no means confirm it. In Iceland the common average of a family is from fifteen to twenty children; in Sweden from eight to ten; in the Netherlands from ten to twelve; in Germany from six to eight; in this country about the same proportion; in France from four to five; in Spain and Italy from two to three; again, however, as we advance further towards the south, especially the northern latitudes of the torrid zone, it increases.*

8. The chapters on Menstruation, Amenorrhœa, and Dysmenorrhœa, are very unsatisfactory. In treating of amenorrhœa, no regular or distinct order of arrangement is pursued; the causes, symptoms, and treatment being so mingled together that it is impossible for the student to form any clear notions upon the subject. The numerous forms under which dysmenorrhœa makes its appearance, and the different periods of the patient's life at which it may come on, are not mentioned, and exposure to "cold and dampness" are the only causes enumerated. We must admit, however, that the author shows himself well acquainted with the disease in its practical details. He very justly observes, "that, where from long-continued dysmenorrhœa the womb has been the seat of a preternatural irritation and affluxion, the organ will in many instances be found tumid and painful. Such a state as this, when once fully ascertained, will throw much light on the nature of the treatment to be adopted under such circumstances as these, which are to be met with far more frequently than is generally conceived of. Rest in a recumbent posture, continued for the space of several weeks, tends greatly to the subduction of the congestive and inflammatory disorder of the womb." (p. 66.) He lays great stress upon rest in the recumbent posture, and observes that, "under this absolute rest great good may be expected from the proper use of bleeding, whether general or local." "These abstractions of blood are followed by great relief, and should be repeated from time to time according to the state of the case, which ought to be noticed by an occasional repetition of the operation of *touching*. During the course just now recommended, the patient should use a purgative or aperient dose every third or fifth day; such as a portion of blue pill and rhubarb, or the sulphate of magnesia. Those days in which no aperient is given should be employed for the administration of antimonials in such form as the physician may select." (p. 67.) Dr. Meigs is in the habit of preferring the precipitated sulphuret of antimony with small portions of camphor, and sometimes morphia or opium. In many cases where the disease is more purely spasmodic, general or local detraction of blood is not required, the sole treatment consisting in allaying the patient's sufferings during the attack by sedatives, and carefully regulating the

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* Burdach.

state of the stomach and bowels and improving the tone of the general health during the intervals. We have not employed antimonials in dysmenorrhœa, except in those cases which had been suddenly brought on by exposure to cold, and which were accompanied by considerable febrile excitement. The form of dysmenorrhœa depending on an arthritic or rheumatic diathesis is not mentioned by the author, which surprises us the more, as it was pointed out by his talented countryman Dr. Dewees: the value of the ammoniated tincture of guiacum in this form of the disease is therefore unnoticed.

9. On the subject of leucorrhœa the author's views are simple and practical, although we cannot praise his method of treating it. There is too little attention paid to arrangement. We are apt to ridicule the scholastic and methodical manner in which these subjects are discussed by our German brethren, but it is highly important that a habit should be acquired of following a distinct arrangement; the subject is thereby rendered more complete and more intelligible. Dr. Meigs remarks that "the vast majority of cases are slight, producing even less inconvenience than an ordinary cold in the head; physicians are not told of it, and it disappears spontaneously like other slight catarrhs, leaving the patient in as good health as she enjoyed before the occurrence." (p. 72.) The observation is good; but why has he not extended this comparison to acute leucorrhœa?—a form of disease which is entirely omitted. No notice has been taken of Sir Charles Clarke's valuable observations on the causes of simple leucorrhœa. The effects of gastric derangement on the general tone of the system, more especially in the degree of corrugation which the lining membrane presents, and the connexion between this state and the quantity of mucus secreted, are points which have been so well handled by this author, that we cannot pass over such an omission without expressing our regret at it. The author advocates the use of copaiba, and recommends its being combined with the syrup of tolu, not only to correct the disagreeable taste, but also as conducing to the therapeutical result which is aimed at. His remarks on the utility of bloodletting in this affection would rather stagger a London practitioner:

> "There are few American physicians who would hesitate to employ the lancet for the cure of a rebellious menorrhagia, no matter how feeble the patient might be; and inasmuch as the excretion of puriform mucus is an action depending upon excited vital force, quite equal in intensity to that which causes pure hemorrhage, the use of the lancet appears to be as clearly indicated as a general principle in the one as in the other. It is not necessary to have a hard bounding pulse in order to warrant the use of venesection; a patient who is able to walk across the floor can almost always bear the loss of a portion of blood without great inconvenience, and that inconvenience is generally not to be put in comparison with the invaluable influence of bleeding upon all the emunctories. Bloodletting, therefore, ought to be more freely and commonly resorted to in the management of fluor albus." (p. 76.)

This practice may possibly hold good among the healthy inhabitants of Philadelphia, but among a population like that of London, where want of tone and vital energy forms the predominant character of a large portion of the diseases, it would in many cases be absolutely dangerous, and in most very injurious as tending to aggravate that loss of vigour and tone which has been more or less connected with the disease since its first commencement.

10. The ninth chapter on Pregnancy contains much interesting matter, and its value would have been not a little enhanced if the author had omitted all the pages of useless and unprofitable discussion about generation, seminal animalculæ, &c. His observations on the changes which take place in the uterus after impregnation as respects the formation of the decidua and development of the ovum, are very excellent, and it is impossible not to draw a strong comparison between them and the very inferior matter with which the chapter has commenced. Dr. Meigs appears to describe the early development of the decidua from his own observations; and, if this be the case, we cannot but compliment him highly upon his researches in this difficult branch of physiology; as also for the simple, lucid style in which his description is given.

"At the time the act of fecundation takes place, or soon afterwards, the inner surface of the womb undergoes a change which causes it to be covered with a deposit of plastic lymph or albuminous matter which adheres to every part of the inner paries. This deposit has been likened to a coat of white paint plastered over the mucous lining. It might be better compared to the inflammatory exudation of croup, or to those white crusts that are found to line the soft palate and arches in some anginas. Be this as it may, when the small Graafian vesicle reaches the uterine extremity of the Fallopian tube, it finds the orifice closed by this material, which it pushes away as it advances, and at length gets within the uterine cavity; one of its hemispheres resting in contact with the naked surface of the womb, while the other is still resisted by the new deposit which it had thrust away before it as it came into the cavity: it is thus placed betwixt the womb and the deposit. As the new deposit is a mere exudation intended to subserve only temporary purposes, and to be discharged when these purposes have been fulfilled, it is properly called the deciduous coat or caducous coat. But the ovule thrusts the caduca away before it, turns it back, reflexes it, and on that account the decidua has two appellations: 1, decidua vera, meaning all that part that remains in contact with the womb; and, 2, decidua reflexa, or all that part which is reflexed or pushed away by the advancing ovule. When the ovule arrives, it is not so big as a pea, but the cavity of the womb is much greater. The decidua reflexa which closely covers the ovule is just large enough to cover it or inclose it, but the decidua vera is as large as the whole uterine cavity. In process of time the ovule is found large enough to fill up the whole cavity of the womb, and as it always carries the reflexed part of the decidua with it, it follows that the decidua reflexa and the decidua vera come at last to be brought into close contact, and by the continued pressure of the womb are finally so united or fused together as to be indistinguishable from each other." (p. 100.)

We do not make this quotation because it contains anything new, but because we think it gives a clear and correct view of these changes in fewer words than most descriptions with which we are acquainted. With such correct remarks, we are the more surprised that neither Dr. Meigs's reading nor personal observations should have informed him that the Graafian vesicle does not quit the ovary and pass along the Fallopian tube into the uterus. The paragraph which follows the above quotation contains observations on a point of ovology which is entirely new to us. "While the decidua reflexa is thus rapidly increasing in size, pari passu, with the enlargement of the ovum, the space between the two decidua is not void; it is filled with a substance resembling white of egg, contained in a delicate hyaloid membranous network. This substance entirely disappears before the fifth month of gestation." (p. 101.) We cannot presume to offer any remark upon it, not having seen the substance in question, nor do we recollect any description of it among authors on these subjects.

The author's observations on the manner in which the placenta is formed by that portion of the chorion where the ovum is in contact with the uterus are very good, his description is equally so. "Admitting that those villi of the chorion that are in contact with the naked surface of the womb are finally converted into placenta, then the whole of the villi that are in contact with the decidua reflexa never form any union with that substance; the necessity for their existence ceases, and they slowly disappear until the chorion where they covered it becomes a smooth shining membrane." (p. 102.) This fact is well known, and may be demonstrated more or less in almost any ovum which has been expelled at about the tenth or twelfth week. That the ragged vascular prolongations of the chorion during the early periods are "in contact with the naked surface of the womb," we presume there can be no doubt; the ovum at this early period is nourished chiefly by the absorbing power which they evidently possess; they are, in fact, little else than a system of roots for the nourishment of the embryo, being, according to the admirable researches of Lobstein, the extremities of venous branches which unite to form the umbilical vein, the existence of which vessel previous to the umbilical arteries has been confirmed by most original authors on these subjects. "It seems now to be generally agreed that the spongioles of the chorion are not blood-vessels, but merely a sort of areola subserving the purposes of nutritive absorption up to the period when the forces of the embryo acquire a certain degree of development." (p. 103.) The quotation which we have made from Dr. Montgomery's valuable work on the signs of pregnancy in our Number for October 1837, p. 458, bears strongly upon this interesting point; our limits will only permit us to refer our readers to it, as also to our own observations which follow.

11. We can by no means agree with Dr. Meigs that the liquor amnii "can in no manner serve for the nutrition of the child." (p. 105.) The quantity of albuminous matter which it contains during the early periods of pregnancy, and the gradual disappearance of this principle show that this is another means which is designed for the nutrition of the embryo before the placenta has attained its full development and function. Meckel instituted a variety of experiments for this purpose, which prove, we think beyond all doubt, that the liquor amnii holds a very important share in the nourishment of the fœtus, especially during the first half of pregnancy. Neither do we coincide with some other observations in the same page: indeed, they are so unworthy of the excellent remarks to which we have just alluded so favorably, that we would hardly have supposed them to have originated from the same source. The author states that the proper situation of the child "throughout the entire pregnancy is with the head downwards, the breech being towards the fundus uteri. Some cases however occur (he adds) in which the pelvic extremity of the child advances first, and we are bound to believe, when such instances do occur, that the child has been in that attitude during all the latter months of the gestation." (p. 105.) That this is very far from being the case is proved, not only by the different portions of the abdomen in which the movements of the child are felt at different times, but by the different parts of it which are frequently felt presenting when we have the opportunity of examining two or three times before labour; and it is proved still more strikingly by auscultation. His remark in the follow-

ing page, that "the cervix certainly becomes fuller and larger at a very early period of pregnancy" is very correct; it is to this circumstance that we attribute the common but erroneous notion of the uterus sinking lower into the pelvis at this period, (usually the second month,) and it is to the late Madame La Chapelle that we are indebted for a correct explanation why we can reach the os uteri more easily at this period of pregnancy than at any other.

12. In speaking of Mr. Hunter's views of the placental circulation and giving a short summary of them, the author says "the vessels of the fœtal umbilicus either pump up this fluid (the maternal blood) and take it to the embryo, &c." We cannot allow this to be said "on the authority of Mr. Hunter," whose observations and researches on this subject tend to so opposite a conclusion. We never read his Essay on the structure of the placenta, without feeling more and more thoroughly convinced of its great value: the beautiful simplicity and accuracy of his descriptions; the well-arranged observations, proceeding step by step and leading to such important conclusions, render it almost impossible to add anything further upon this subject. The oftener we read it, or the more elaborate essay on the same subject by Dr. W. Hunter, the more surprised we feel that many authors of the present day should be inclined to doubt the accuracy of such observations.

13. Dr. Meigs's attempt to explain the source of hemorrhage when the placenta has been separated from the uterus (p. 115) is exceedingly confused and incorrect; he supposes it to proceed from what he calls "hemorrhagic irritation;" and appears to have no notion of its coming from the mouths of those arteries whose communication with the placental cells had been destroyed. His views respecting the action of cold in uterine hemorrhage of this description is equally incorrect; he supposes it to act as in epistaxis, hæmoptoe, or hæmatemesis! His treatment of the whole of this subject is very imperfect, and betrays but little acquaintance with the literature of it, although it has been enriched by the observations of Wrisberg, Meckel, Lobstein, and the Hunters,—not to mention the interesting papers in the Medical Gazette by the late Dr. Hugh Ley, and Messrs. Stanley and Mayo.

14. The remarks on the treatment of abortion are simple and practical; and we were much pleased to notice some observations on the subject of retained ovum which interested us much and with which we fully concur.

> "A good many cases of abortion in the early stage, as from the sixth to the tenth week, have fallen under my notice in which the uterus was unable to expel the remains of the ovum, and in which I could not extract it. The female in such instances has always recovered without the ovum having been visibly discharged, but there was always an excretion continued for many days of offensive dark-coloured grumes and sanies which I accounted for by supposing that the substances in the uterus had macerated and come off in a state of semi-solution. I think that there is no danger in leaving such occurrences in the hands of nature, and that it is better to do so than to reiterate attempts to extract by force that have perhaps already proved quite vain, especially considering that there is as great danger of exciting inflammation by those attempts, as could be anticipated from the gradual maceration of the ovum. I am not disposed to deny that the presence of a putrefying substance even of a small size in the womb is capable of developing violent inflammation and fever; but it has not happened so with me, and I have given the same opinion to some medical friends, by whom I have been consulted, without the least cause to regret

having given such advice. Let me be clearly understood, however, to recommend that the last remainders of the ovum should be brought off where it is practicable by means of any reasonable efforts." (p. 130.)

We have had repeated opportunities of proving this fact, although we must confess that as far as respects portions of placenta which have been retained after labour at the full term, the discharge has not been as the author describes, but to all appearances perfectly natural lochia, except that it was rather sparing in point of quantity. We quite agree with him that "the presence of a putrefying substance, even of small size, is capable of developing violent inflammation and fever;" and we have every reason to suppose that the absence of these formidable symptoms in cases of this sort has been owing to the full and firm contractions of the uterus effectually excluding all excess of external air.

15. We scarcely understand why Dr. Meigs experiences such difficulty in accounting for the symptoms produced by prolapsus uteri. Both the mechanical and constitutional symptoms are in our opinion exceedingly easy of explanation; and we should be inclined to attribute a portion of the difficulty of which he complains to the wrong principle upon which he sets out, viz. in denying that the vagina has anything to do with supporting the uterus. The excellent observations of Dr. Dewees would, we conceive, have been sufficient to show that this was very far from being the case. We are the more surprised at his observations, because, when he speaks of employing a pessary, he observes that "the pressure which it must exert upon the tissues which inclose it, could not fail to have a great tendency to condense those tissues, and thus obviate the relaxation of them which constitutes the pathological essence of the disorder." (p. 138.) No notice whatever is taken of the utero-abdominal supporter invented by the late Dr. Hull, we believe, of New York. We should not have ventured to allude to it upon the present occasion had not the author entered pretty fully upon the consideration of prolapsus, merely because a woman may be liable to it if she leaves her bed too soon after miscarriage, the chapter itself being headed "pregnancy." Dr. Hull's instrument acts chiefly by removing the weight of the intestines, and from the lower part of the abdomen and pelvis by means of a very ingeniously contrived truss, which, by pressure carefully applied in a direction upwards and backwards, effectually prevents their gravitating upon the uterus and forcing it down: we have tried it, and we must confess with a preconviction of its inutility; but it answered its object most satisfactorily—the patient was immediately and agreeably relieved from the painful sense of weight and bearing down from which she had hitherto suffered.

16. We have observed in a former Number of this Journal, that the condition of the child, as respects its state of nourishment, is in no wise proportionate to the condition of the mother; that stout hearty women frequently bear meager ill-nourished children, whereas those who are emaciated by disease or want are frequently observed to have remarkably stout children. Dr. Meigs's experience perfectly accords with our own.

"I have witnessed several accouchements in females reduced to the last degree of weakness and emaciation by pulmonary consumption. In these cases I have not found that the child partook of the debility and cachexy of the unhappy mother. In one case that fell under my care the patient laboured under laryngeal consumption, and was to the last degree emaciated and feeble, notwithstanding which her pregnancy held out until the completion of her term, which she survived only four or five days. Her child was excessively fat, weighing above ten pounds, and has continued to enjoy excellent health up to the present time, when it is between five and six years of age. These instances convince me that no methods that have yet been discovered can serve to prevent the development of the child, and thus cause the woman to suffer less in her accouchement than she would do if her child were to be small." (p. 147.)

17. Dr. Meigs follows chiefly the French authors in describing the various presentations of the child, and the manner in which they advance through the passages; he applies the term vertex to the posterior fontanelle, and therefore, until the reader has understood this peculiar meaning, it is difficult to understand him. The whole chapter is very theoretical, and bears the marks of more ingenuity than of careful and practised examination; if this had not been the case we should have seen fewer such expressions as "dip of the child's head," "pivot motion," "rotation," &c. He speaks of the child being in danger of strangulation, (p. 182,) if the cord be tight round its neck when the head is passing over the perineum. Does the face in this position swell and grow livid from strangulation? or can such a term be applied to a child which has never used its lungs? Mauriceau long ago showed correctly the cause of its death, viz. from an apoplectic state of the brain being induced by the pressure of the cord upon the neck; it has nothing to do with strangulation. In other respects his remarks are simple and practical. In the page just referred to he observes, " the young practitioner and the student should be warned against falling into a habit of beginning too early to support the perineum. If the part be too early pressed upon with a napkin it might become heated and thus lose its disposition to dilate," and in the following page he observes, that where the shoulders have passed, " it is considered bad practice to drag out the body except in very particular instances: the womb and abdominal muscles are sufficiently powerful for that object; and if it be permitted to come away slowly we shall have a more complete contraction of the womb, and a more ready detachment and extension of the placenta." (p. 183.) His remarks on the management of the placenta are equally good; he directs that it should be turned round several times at the moment of its expulsion, so as to twist the membranes into a cord and thus ensure their removal without laceration.

"The influence of position in determining the momentum of blood in the vessels is well known to the profession; but there are few cases where it is of more consequence to pay a profound regard to this influence than in parturient women. A uterus may be a good deal relaxed or atonic, and yet not bleed if the woman lie still with the head low, whereas upon sitting up suddenly such is the rush of blood down the column of the aorta, the hypogastrics, and the uterine and spermatic arteries, that the resistance afforded by a feeble contraction is instantly overthrown, the volumes of blood escape with an almost unrestrained impetuosity. The vessels of the brain under such circumstances become rapidly drained, and the patient falls back in a state of syncope which now and then proves immediately fatal." (p. 192.)

Dr. Meigs very properly insists on the danger of allowing coagula to remain in the uterus, as tending to favour internal hemorrhage. He observes that " strong uteri never permit them, weaker ones allow considerably larger ones to be formed, and very feeble wombs fill until the woman faints or dies." (p. 195.) After so much valuable and practical matter we regret that he should again theorise. Obliquity of the womb is surely no cause of face presentations: we meet with them where the uterus is perfectly straight, and we constantly see cases where the fundus is very much to one side and yet the child presents as usual. We fully agree with the author, however, in saying that there are only "two positions of the face, one with the chin to the left and one with it to the right of the pelvis," (p. 204;) but we would rather invert the order of these two positions, and say with Professor Naegele that those face presentations with the chin to the right are the most common. Dr. Meigs very properly differs from the common rule of examining during a pain: "there is no need for pressing against the bag of waters during the pain, because by waiting until the pain subsides, the bag becomes relaxed, and can then be pushed back again within the mouth of the womb so as to enable the finger to touch the head." (p. 219.) We cannot, however, agree with the author in stating that "the length of the gravid uterus at full term does not exceed twelve inches," (p. 228;) the fundus is well known to have reached the umbilicus at the sixth month, and this in a moderate-sized woman would be little short of twelve inches, whereas at the full term the fundus occupies the scrobiculus cordis.

It is satisfactory to see how clear and correct our author is on all points of practice. When speaking of breech presentations and the danger to which the child is exposed from pressure upon the cord when the head is passing through the pelvis, he adds, "It is my unfailing custom therein to order my forceps to be put in readiness as soon as I ascertain that the presentation is not one of the head, and I feel very well assured that such a precaution if generally observed would preserve many a life which is now lost, either by delay in the delivery of the head, or by pernicious attempts to extract by pulling at the neck, to which the temptation is so strong in moments of great anxiety for parent and offspring." (p. 238.) This is very excellent practice, and we have no doubt that if the forceps was at hand in every case of turning or breech presentation, many more children would be born alive under such circumstances than is the case at present. The valuable observations on this subject by Professor Busch, of Berlin, of which we have given a very brief notice in our tenth Number are well deserving of notice. By attention to this point Professor B. has succeeded in saving the child's life in forty-one turning cases out of forty-four, a degree of success in this operation which has never been even approached by any practitioner. Dr. Meigs entertains the old and long-disproved notion respecting obliquity of the uterus being a cause of the child presenting unnaturally: it will be scarcely necessary to discuss the point, the highest authorities both of former as well as modern times combining to reject it entirely. His views respecting arm presentations are very unsatisfactory: he remarks that, "as there are two shoulders, a right and left one, there must be a set of positions for each shoulder." (p. 247.) Of what use in practice is all this complication? We pass our hand along the anterior surface of the child if possible, in order to reach the feet, and if the arm be prolapsed the direction of the hand will generally guide us. If the case be at all difficult we must direct the hand wherever there is most room. His directions for introducing the hand are excellent, as indeed his observations generally are on subjects purely practical.

In enumerating the various signs indicative of a shoulder presentation, the author omits the important one of not being able to feel any presenting part at all. The os uteri has gradually dilated, a large bag of liquor amnii (not always deviating from the regular form as the author mentions) is felt, and still no presenting part has descended within reach; this is a very suspicious sign, and in a primipara is a bad one, because the head is known to be usually so low in the pelvis at this time. We are far from considering this an infallible sign, however, because the breech may present, and this is generally very high up in the pelvis at the commencement of labour; or it may be a contracted brim preventing the head from descending; still nevertheless it is a symptom which ought never to be overlooked. Dr. Merriman's admirable observations on this point are well worthy of attention.

The author's directions as to the precise moment for introducing the hand in turning are excellent: "during a pain, two fingers, and then three, of the left hand should be passed into the vagina, &c." (p. 250.) The patient is much less sensible to the introduction of the hand during a pain, and we thus in a great measure diminish what is the most painful step of the operation; the hand, moreover, is now ready to enter the uterus the moment the pain goes off. "It is always desirable to get the hand out of the uterus as soon as may be; and it is far better to turn by one foot or by a knee than to incur the risk of lacerations or contusions of the organ by a tedious search after the other foot, which, if it be not originally near its fellow, is very hard to be found by any search after it." (p. 251.) This advice is important in many respects, wherever turning is at all difficult, or has to be performed under unfavorable circumstances. We are rarely able to bring down both feet at first, except where, by some very lucky chance, they are lying close to each other. Generally speaking, by the time we have brought one leg fairly into the vagina, the position of the child is beginning to change so materially that the knee or foot of the other lower extremity will come within reach. We hold it to be highly desirable to bring down both feet, if possible, as there is no doubt that the nates of the child enter the brim of the mother's pelvis much more readily. We cannot agree with the author in a sentence which follows immediately after. "Having found the foot, if a pain comes on immediately, and becomes a severe one, the foot should be let go, and caught again after the pain is gone off, according to the discretion of the operator." (p. 251.) Not only do we run a chance of the foot getting quite out of reach when the pain has ceased, which is of no slight consequence, but we are turning the pain itself (now that we have reached the foot) to no account. Our practice, wherever the foot is high and difficult to reach, is to hold it quite still for a pain or two; feeling assured that, as soon as the pain is over, it will have come a little more within reach, and enable us to take a firmer grasp against the approach of the next pain. The pressure of the other hand upon the abdomen is very important, and, as Dr. Meigs truly observes, "ought not to be neglected." He gives us no directions as regards the membranes in turning: whether we are to rupture them or insinuate the hand between them and the uterus; a point of practice which is of considerable importance, and upon which it would have given us much pleasure to have known his opinions. We presume that he recommends the latter mode, as this is evidently his practice in cases of placenta prævia: having introduced the hand between the placenta and uterus, he observes, "the membranes may then be ruptured high up in the uterus, and the feet immediately sought for." (p. 264.)

Dr. Meigs denies that the forceps act by compression as well as by extraction; but here we must differ from him: there can be no doubt that a degree of pressure, carefully applied, does alter the dimensions of the fœtal head, and assists greatly in adapting it to the passage through which it has to come. Baudelocque's experiments on the heads of stillborn children prove nothing as to the present question: indeed, their fallacy is shown by the alteration of shape which the head presents immediately after labour in most children, especially where it has been the mother's first pregnancy. The author's directions for applying the forceps are pretty nearly those of the French school, the patient being placed on her back for the operation. They are minute in some respects, but incomplete and meager in others. His precautions in extracting the head are good; but, as we cannot agree with him in asserting that the forceps do not act by compression as well as by traction, we consider his objection to keeping up a continued pressure upon the head, either by grasping or tying the handles, as unfounded. We have ever found in difficult forceps cases, where the head was considerably impacted, that our only chance of avoiding perforation was to maintain a firm but steady pressure upon the head by the above means, until it had so elongated and adapted itself to the passages as to produce less obstruction to its advance: the pressure thus artificially applied is surely as close an imitation of nature as using extractive force is: we are, in fact, only using the same means to alter the shape of the head which she avails herself of.

18. In speaking of puerperal fever, the author observes, "the peritoneal coat of the womb is greatly expanded or stretched in the last stages of pregnancy. The broad ligaments are drawn up on the sides of the uterus to a considerable height, while the portion of the membrane that lines the front and sides of the belly is also put greatly on the stretch. This tension could not but increase its natural proneness to take on inflammatory action, if exciting causes should be applied after delivery," (p. 246;) and yet he commences the following page with a remark which appears to us rather inconsistent: "The relaxation of the peritoneal membrane that follows delivery, and the reduction of the womb to a small size, is, beyond doubt, one of the most fruitful sources of inflammation of the membrane." (p. 347.) The author's high encomium on bleeding in puerperal fever, as well as his observations under this head, are interesting, inasmuch as they show us under what form the disease has most frequently manifested itself in Philadelphia.

> "I would earnestly endeavour to impress upon the mind of the student of medicine the vital importance of great promptitude in his attention to the earliest signs of this dreadful malady. I would convince him that the principal feature in the disease called childbed fever is peritonitis; that the inflammation is so acute, and the tissue in which it is seated is so inflammatory, that the malady is capable of hurrying through its curable stages more rapidly than even the redoubtable croup; and, what is of still greater moment, that it is in the incipient stages nearly as curable as croup, and that the remedy (or I might say the cure) consists in the bold and judicious employment of venesection. Let me ask what can be the value of any remedy short of venesection in a malady like this, which presents a case of pure inflammation, occu-

pying, or making haste to occupy, not a few square inches, but many square feet, of a membrane that serves as the investment of the most important organs?" (p. 352.)

Would that puerperal fever always appeared in this manageable form! Many are the times when we would have hailed the appearance of peritonitic symptoms as a fortunate occurrence for the patient. In some of the very worst cases that have fallen under our notice, there was not one symptom of inflammation during life, or one mark of it to be discovered after death: some of these, from the first perceivable change in the patient until the fatal termination of the disease, have run their course in twenty-three hours. We must, however, refer the reader to our reviews of the valuable works of Mr. Moore and Dr. Ferguson on Puerperal Fever, (vol. II. p. 481, and vol. VII. p. 482,) for our opinions on this momentous subject.

Dr. Meigs's treatment in the form of puerperal fever which he has described is very judicious; and the manner in which he has pictured the last sad hours of an unsuccessful case are so graphic and true that we must be allowed to make one more quotation.

"To find an improvement in the patient's ability to move herself, with a corresponding improvement in the circulation, is of the most favorable augury; but, to observe the pulse increasing in frequency while it also becomes more feeble, with diminished heat of the members and augmented heat of the body,—to discover a disposition to singultus, with an eructation of fluids into the mouth, an anxious expression of countenance, high and frequent respiration, with increased ability to move the legs, and diminished pain on pressure, are all indicative of the cessation of inflammation of the peritoneum; but it has ceased not by resolution or a return to health: it has come to one of its natural terms in effusion. The inflammation is at an end, and the patient begins to die. It would seem that the forces of the living economy have exhausted themselves in the struggle with a malady, which, though they conquer it at last, yet are themselves destroyed in the moment of victory. There soon comes on a vomiting, or rather a violent eructation or gurgitation of dark-looking fluid; the patient mutters, she picks the bedclothes, she clutches at muscæ volitantes; the diaphragm labours to carry on, in vain, the work of respiration; the hands and feet acquire a livid hue and are clammy; the pulse becomes a thread, it ceases in the wrists, and she dies probably in the act of regurgitating from the stomach the last draught which the anxious hand of friendship or love has tendered as a solace or as a hope." (p. 358-9.)

The work terminates with a few observations on the Morbus cæruleus. The author thinks that permanent relief may be given by placing the child upon its right side, propped up by pillows at an angle of thirty degrees; that, by thus " maintaining the heart in such an attitude, the left auricle would be perpendicularly above the right one; and that the effect of gravity alone on the blood would gradually operate in such a manner as to allow it to flow off into the ventricle, instead of finding its way to the systemic auricle." (p. 367.)

We here close our review of Dr. Meigs's book. It decidedly contains much sound practical information, but still we cannot place it in the slightest competition with Dr. Dewees's admirable work. The literature is everywhere too defective, and, by apparently confining his reading very much to the French authors, and those (with the exception of Mauriceau) chiefly of the present day, he has been too frequently led into the vague theoretical views in which they are so apt to indulge.

II. Dr. Martin's work consists of a collection of papers upon subjects more or less connected with obstetric practice, which are evidently the result of much experience gathered from long and extensive observation. If we overlook the foolish and unworthy habit which our Gallic neighbours are so apt to fall into, of indirectly arrogating to themselves the whole merit of certain facts and modes of practice, which, however good and praiseworthy, had been known in other countries long before them, we shall find that M. Martin's work contains much that is valuable and deserving of attention. The work itself, as the preface informs us, was originally a septennial report of cases at the Charité Hospital of Lyons; the author's remarks are consequently short and practical; and we shall therefore in noticing it, confine ourselves, as far as possible, to giving short extracts here and there of whatever we may consider useful or interesting to our readers.

1. In speaking of *abortion*, the author observes that "the most frequent causes are mental emotions and inveterate chronic venereal affections; under the last-mentioned circumstances, I have almost always found the products of conception macerated and as it were decomposed, without any fætor, at least in those cases where no access of the external air had taken place from rupture of the membranes. It is so true, that atmospheric air is a necessary agent to putrefactive fermentation, that I have known a full-grown living fœtus born, holding in its left arm a macerated withered abortion, without any smell, which from its size had evidently lived to the fifth month." (p. 6.)

2. The ages at which the female system becomes susceptible of impregnation, and ceases to be so, vary exceedingly. Dr. M. has met with a case of pregnancy at the age of thirteen years and a half, and another where the patient was somewhat older, but had not as yet menstruated. On the other hand, he mentions having once attended a patient in labour at the age of fifty-four, whose appearance was as decrepid as that of most women at sixty. We have ourselves met with cases of pregnancy at this extremely early age, but do not recollect one where the menses had not already appeared. The fact, however, of impregnation taking place before the appearance of the catamenia has been noticed long ago by Levret. A case of pregnancy at so late a period of life as that described by the author, we must confess we have not met with.

3. In describing the effects of difficult labours, M. M. remarks that "when the capsules of the pelvic symphyses had been torn, a species of ligamentous callus formed behind the pubes, which rendered the succeeding labours more difficult. I have seen collections of matter follow laceration of the pubic and sacral symphyses, the result of which has been usually fatal." (p. 16.)

4. In cases where the child is coming into the world with the feet foremost, and the os uteri has contracted spasmodically round its neck and thus retards the delivery of the head, the author has found the application of cold water directly to the os uteri, to have the effect almost invariably of removing this spasmodic condition of its fibres, and thus facilitating the expulsion of the head. We have no experience in the employment of such a remedy in these cases, but from what we have observed of the effects of cold injections into the vagina in dangerous cases of hemorrhage after labour, we should have expected a very opposite effect.

5. M. Martin makes the discovery that wherever he has ventured to apply

the forceps, the small diameter of the pelvis being less than three inches, that the child was born dead! We should have been inclined to ask whether the mother did not die also? We much question also the correctness of his observation where he fixes two inches and three quarters as the limit within which the Cæsarean operation is required; as numerous cases prove that, even with a greater degree of contraction, the child can be delivered by perforation and embryotomy.

6. We fully agree with the author in the following remark: "The adhesion of the placenta to the uterus has appeared to me to arise from a morbid condition of the internal surface of this organ, for I have rarely observed it in one confinement without meeting it in the succeeding ones. I have seen it four times successively in the same individual." (p. 20.) It is difficult, nay perhaps impossible, to say what is the condition upon which this state depends; and the opportunities of examining the nature of this adhesion minutely are very few. In some cases, where the attachment is exceedingly firm, the whole substance of the placenta breaks up in the attempt to remove it, becoming a soft ragged fibrous mass which adheres with the utmost tenacity to the uterus.

7. We might have added some critical observations on the author's views of placenta prævia; but our object, in the present instance, is rather to give a brief yet comprehensive account of whatever is good and useful in a work which contains so much valuable matter, and which has evidently been the result of much observation. We therefore pass on to an excellent remark on the subject of epidemic puerperal fever. "It follows (says M. M.) the type of the prevailing epidemic, and usually takes the same character." (p. 27.) The correctness of this observation has been strikingly confirmed during the epidemic of malignant typhus which has lately prevailed so destructively in London. In several puerperal cases which we have witnessed, the livid, rubeoloid eruption was precisely similar to what was so constantly observed in patients of both sexes who were at that time suffering from the prevailing disorder: in many cases the patient seemed suddenly struck down by a deadly influence, against the overwhelming effects of which, the powers of life appeared capable of offering no resistance.

8. On the subject of stillborn children he says, "Some of them present all the appearances of death, because the lungs have not yet acted, a state which may be called asphyxia; others arise from the pressure which the brain has undergone from the gorged state of the cerebral vessels." (p. 34.) We do not quote this as an original observation, because this cause of asphyxia has been pointed out long ago by Smellie and more lately by Chaussier, but merely from a wish to call the attention of our junior readers to a fact which perhaps has not received sufficient notice. His treatment of asphyxia neonatorum is good, but presents nothing new, except that he recommends plunging the child into a bath of hot wine, which would be rather an expensive remedy in this country, and which cannot have one great advantage which hot water possesses, viz. of a large quantity being kept hot in readiness the moment that the child is born; dipping the child for an instant into hot water which is at too high a temperature for it to remain in, and then suddenly exposing it to the air and again repeating the process is a plan which has, we believe, been used with considerable success by the matron at one of the Lying-in

Hospitals of London. "Bleeding from the cord," adds M. Martin, "has always sufficed to relieve the brain and remove the apoplectic condition. I can assert from my observations that the appearances of death in new-born children are very deceptive. I have recovered by perseverance children which presented every sign of being dead. I met with one case among others, which after having exercised my patience for two whole hours, recovered at the moment I was about to desist from my attempts. I have seen many children die who had continued to cry loudly from the moment of their birth, and yet upon the most careful examination after death no cause could be detected upon which the slightest conjecture could be hazarded." (p. 35.)

9. Dr. Martin considers that he has frequently prevented infection being communicated to a child from a syphilitic mother, by throwing up oleaginous injections into the vagina both before and during labour. This, of course, applies chiefly to where the mother has been but recently infected, and not to old cases of long standing where the whole system has been pervaded with the disease.

10. In his treatment of Nævus, he never uses a cutting instrument; but where it is situated upon a part which offers a sufficient degree of resistance to admit of compression, he applies it by means of a pad which is fixed by a circular spring upon the head: by this means he informs us that he has occasionally succeeded in removing the nævus, and where he has failed in obtaining this result, he has at any rate been able to arrest the progress of its growth.

11. Dr. Martin describes a case of hydrocephalus in a child eighteen months old where he performed paracentesis capitis, and drew off a pint of serous fluid; for a short time after, the child appeared much relieved, but died on the fifth day. The date of this case would have been desirable, as it would have shown whether he had performed this operation before or after it had been tried by Dr. Conquest in London, to whose observations however he makes no reference.

12. He mentions a case of hydrophobia where the disease was prevented, by cauterizing the wounds deeply with the butter of antimony; the patient, a girl, had been bitten by a mad wolf which had attacked several people, many of whom died from hydrophobia. We remember seeing a somewhat similar mode of treatment adopted by the celebrated Rust, of Berlin: he premised, however, a very effectual excision and dressed the wound afterwards with liquor potassæ, a remedy which he considered to possess a peculiar power in decomposing animal poisons. How strikingly has this latter opinion been confirmed by the masterly researches of Dr. Stevens.

13. M. Martin has given some very interesting observations on the changes which the fœtus undergoes when it dies in utero. He divides them under two heads, firstly, where the fœtus has died in utero without rupture of the membranes; and secondly, where it passes rapidly into putrefaction, where the os uteri being open and the membranes ruptured it has come into contact with the atmospheric air. Of the first class he has given six cases of abortion at an early period, in all of which the ovum was expelled entire, but without any traces of the embryo. This disappearance of the embryo after its death in the ovum has been noticed by many authors, more especially by Smellie, who considers that this

dissolution of the embryo may take place in the course of a few hours where pregnancy has not advanced beyond the month, and even in the second month twenty-four hours will be sufficient to effect this change. What the precise nature of this process is, we cannot venture to determine; but experience shows that after such a change the ovum seldom, if ever, remains any period of time in the uterus without undergoing considerable alterations of structure, which are chiefly confined to the chorion: this becomes much thicker, grows with rapidity, and assumes the peculiar fleshy structure which gives the ovum the name of *mole*. M. Martin has limited the above-mentioned cases to the first two months of pregnancy, and as regards the disappearance of the embryo he is perhaps right; but it is very hazardous to draw such lines of demarcation; we regret that the condition of the membranes was not more precisely noticed, as we cannot but think that some of those changes in them to which we have just alluded, would have been observed. It may be, perhaps, a question whether the membranes of the ovum would undergo those changes which constitute a molar growth in cases where the embryo has perished without any further injury having been sustained by the ovum; for it would seem, according to the admirable observations of Dr. Simpson of Edinburgh, that sanguineous extravasation to a certain extent is necessary to produce the fibrous lobulated appearance so frequently observed in abortions of this character. Several interesting cases of twin-pregnancy are also given, where one fœtus had died whilst the other went on to be developed up to the full time. He also mentions an extraordinary instance where expulsion took place *during sleep* at the beginning of the seventh month, the fœtus having evidently died in the sixth month. In summing up his remarks on these subjects, the author observes as follows:

> "The fœtus which dies in utero may remain there a considerable time without passing into putrefactive decomposition, so long as the os uteri is closed and the membranes unruptured. The changes which a dead fœtus undergoes in the uterus vary according to the period of pregnancy at which it ceased to live; thus, in the early grades of its development, when its structure presents but little more than a gelatinous consistence, it is dissolved in the liquor amnii which becomes thick and mucilaginous. We can find no traces of the embryo in the cavity of the membranes which may continue to remain in the uterus for some time. At a more advanced stage, viz. from the second to the fifth month it withers and shrivels up; it resembles rather a little mummy of a yellow colour, or as if it had been kept for some time in spirits. The membranes frequently undergo a similar change, the liquor amnii disappears, and in its place we find a thick and almost earthy fluid with which the fœtus becomes incrusted. From the fifth month to the full term of utero-gestation, the fœtus macerates, increases in size, and becomes soft and friable; the epidermis separates, forming phlyctenæ of more or less extent; the skin becomes discoloured, as also the muscles, which tear with the slightest effort; although animal decomposition is evidently commencing yet there is merely a faint sickly smell, and nothing of that powerful fetor which characterizes putrid fermentation." (p. 95-6.)

14. Dr. Martin has devoted a chapter to the subject of retroversion of the uterus, and has collected an unusual number of cases of this rare displacement. Two of them are peculiarly interesting. The one (p. 156), occurred on the sixth day *after delivery* in consequence of the patient making a sudden effort to save one of her children who had tumbled down in playing at her bedside: the uterus was replaced after some diffi-

culty and the patient recovered. The other (p. 161) is interesting from having been treated by puncture. The uterus had resisted every attempt to replace it; and the practitioner having felt a distinct fluctuation through the rectum, a curved trocar was introduced about five or six lines up the intestine at the spot where the fluctuation was most evident: a quantity of limpid serous fluid escaped, which diminished the size of the tumour in the vagina and relieved the patient's sufferings. Pains came on three days after the operation, and on the following morning expelled a fœtus of about four months and a half. The patient did well.

15. In the chapter *on Inversion*, M. Martin also gives a considerable number of cases. The third is peculiarly interesting from the operator having diminished the size of the uterus by compression before attempting to return it. This mode of practice was adopted with success by the celebrated C. White, of Manchester, in a case of unusual difficulty, and described by him in his work on Lying-in Women, a point which ought to have been noticed by the author. M. M. considers that *inertia uteri* is one of the great predisposing causes of inversion. "From the moment that the fundus begins to pass through the os uteri, the patient experiences a violent impulse to bear down, with painful tenesmus, producing contraction of the abdominal muscles which gradually push down the fundus; in proportion as this approaches the vulva, a painful sensation of weight is felt in the pelvis, and the patient complains of severe pains in the kidneys and groins resulting from the ligaments of the uterus being put so forcibly on the stretch." (p. 212.) He describes an exceedingly interesting case of chronic inversion of two years standing, and where the powers were gradually breaking up under the constant discharge of bloody mucus from the tumour and profuse hemorrhage at the catamenial periods; the patient had consulted the celebrated Leroux, of Dijon, who recommended a palliative treatment; her medical advisers at Lyons proposed the ligature, which was accordingly applied; the symptoms were but slight, the uterus came away on the nineteenth day without any hemorrhage and with complete restoration of her health: the lady was alive in 1833, forty-two years after the operation. The whole chapter contains much interesting information, although not much of it is original; the literature, as elsewhere throughout the work, is extremely defective.

16. The next chapter is on imperforate os uteri, a condition which has lately been made the subject of much interesting discussion in this country. His third case is quoted from Lauverjat: the patient was a primipara; a tumour was found filling the vagina and protruding during the pains: after careful examination he felt convinced that this tumour was formed by the cervix uteri, the orifice of which was entirely obliterated; he made an incision into the tumour through which the child was safely delivered: after labour the os uteri was sought for in vain, nor was it found in its natural condition until two months after. In a similar case which occurred to the author himself, a slight depression was detected, after minute examination, in the vicinity of the left sacro-iliac symphysis; after a short interval, the patient having had some very violent pains, he again examined and found the os uteri somewhat dilated at the spot where he had felt the depression: he supported the tumour whilst he pulled the os uteri forwards into the middle of the vagina, and the labour proceeded

naturally and successfully. "The analogy," says M. M., "between this case and the one related by Lauverjat is striking, and leads us to suspect, 1st, that this celebrated accoucheur made an incision into the anterior portion of the uterus; 2d, that the os uteri, which in this case was found two months after delivery to be perfectly uninjured, had been situated very high up and far backwards." (p. 245.)

17. The chapter on deposits of pus in the appendages of the uterus in consequence of labour, is very interesting. In some cases the author has succeeded in checking puerperal inflammation by the application of leeches to the labia, a mode of practice which has recently been improved upon by a distinguished accoucheur in the Irish metropolis, and attended with great success. Where the symptoms indicate the formation of pus, Dr. Martin uses the caustic potass, for the double purpose of producing a firm adhesion between the parietes of the sac and of the abdomen, and of obtaining an exit for the confined matter, which latter object he sometimes expedites by incision where the parietes are very thick.

18. The author's other chapters, although for the most part interesting, do not contain anything new or of sufficient interest to require notice here. The last chapter, on the history and application of the forceps, is of considerable length. The history is very meager and faulty and full of misspelt proper names for which the French authors are so notorious. M.M. describes a peculiar species of forceps which appears to be generally used in Lyons, the blades of which do not cross, but, as far as we can understand from the description, they cannot be looked upon as an improvement.

We take our leave of Dr. Martin's work with feelings of great respect for its industrious and experienced author. It contains much valuable matter, and will prove a useful work of reference both to the practitioners and the author.

ART. III.

1. *Recherches sur les Fièvres Intermittentes du Nord de l'Afrique.* Par F. C. MAILLOT, D.M., Médecin des Salles Militaires de l'Hospice Civil de Douai.—*A Paris,* 1835. 8vo, pp. 47.

Researches concerning the Intermittent Fevers of the North of Africa. By F. C. MAILLOT, M.D., Physician of the Military Wards of the Civil Hospital of Douay.—*Paris,* 1835.

2. *Traité des Fièvres ou Irritations Cérébro-spinales Intermittentes, d'après des Observations receuillies en France, en Corse, et en Afrique.* Par F. C. MAILLOT, D.M., Ancien Médecin des Hôpitaux Militaires d'Ajaccio et d'Alger; ex-Médecin en chef de l'Hôpital Militaire de Bone.—*A Paris,* 1836. 8vo, pp. 420.

Treatise on Intermittent Fevers or Cerebro-spinal Irritations, from Observations collected in France, Corsica, and Africa. By F. C. MAILLOT, M.D., formerly Physician of the Military Hospitals of Ajaccio and Algiers, and ex-Physician in chief of the Military Hospital of Bona.—*Paris,* 1836.

3. *Beobachtungen und Untersuchungen über das Wechselfieber.* Von Dr. Karl Kremers, Bergarzt zu Pannesheide bei Aaachen.—*Aaachen*, 1837. 12mo, pp. 132.

Observations and Enquiries on Intermittent Fever. By Dr. K. Kremers, Physician to the Mines near Aix-la-Chapelle.—*Aachen*, 1837.

4. *Delle Malattie Periodiche e principalmente delle Periodiche Febbrili.* Saggio Critico di Pietro Manni, Professore di Medicina nell'Universita della Sapienza di Roma.—*Parigi*, 1837. 8vo. pp. 68.

A Critical Essay on Periodical Maladies, and principally of Febrile Periodical Maladies. By Pietro Manni, Professor of Medicine in the University Della Sapienza at Rome.—*Paris*, 1837.

The first of these publications consists of a memoir read to the Royal Academy of Medicine, founded upon observations made or collected by the author in the garrison of Bona, relative to the destructive epidemics of the years from 1832 to 1835. In those years, the garrison consisting of between three and four thousand men, 22,530 were admitted into the hospital, and 2513 died, or 1 in 8; or, according to the more particular statement, there were

Admitted in 1832 .	4033,	of whom 449	died;	or 1 in 7.
.. 1833 .	6704	.. 1526	..	1 in 3½.
.. 1834 and 1835 .	11593	.. 538	..	1 in 20.

M. Maillot's attention was first and principally directed to determining the analogies existing between the fevers of Bona and those which he had previously had an opportunity of observing at Algiers and in Corsica, with a view to deciding on the propriety of applying to them the treatment which he had found adapted to the latter. The establishment of the characteristic of intermittence, as common to both, seems to have been the result; the fevers of Bona, like those of Corsica and of Algiers, arising in the neighbourhood of marshes, the greater proximity of which to the troops at Bona gave a severer character to the fever; demanding, as it proved, a prompt and more energetic treatment. M. Maillot maintains the relation of the continued forms to the intermittent; and the tendency of the intermittent, if unchecked, to pass into the continued; and of the continued, if bleeding was employed, to pass into the intermittent or remittent. These circumstances, he says, convinced him that he had not to deal with true continued fevers, the gastro-enterites or gastro-cephalites of France. He concluded that the probability was that the affections before him were those spoken of by Torti, part of the character of which is "de intermittente sensim, acutam et malignam migrat;" and he resolved on giving the quinine boldly in all the continued cases, without waiting either for remissions or intermissions, which were "only instantaneous when they were obtained." The results, which are very striking, are seen in the diminished mortality exhibited in the above statement.

The intermittent fevers of the winter give place, with an increase of temperature, to severer forms, intermitting, remitting, and continued; and these circumstances mark, in M. Maillot's opinion, "the passage of the irritative congestions which accompany the attack to the gastro-enterites and gastro-cephalites which are so numerous in the hot season."

[illegible] forms, and then give place to [illegible] critical details [illegible] know what a [illegible] writer on these [illegible] fevers to [illegible] probably acts by [illegible] of the body, and giving inten- [illegible] M. Maillot's opinion of the cause [illegible] intermitting forms [illegible] was entertained by [illegible] to doubt. That [illegible] spring or autumn, [illegible] malignant character, and constitute or pass [illegible] period, and which [illegible] known to observers [illegible] relation to the pro- [illegible] described by Sydenham [illegible] summer, and declining at [illegible] to its inflam- [illegible] remarks that, when [illegible] England, as in July, they [illegible] without a very strict [illegible] intermittent later [illegible] periods, been free [illegible] malignant and destruc- [illegible] were most severely pre- [illegible] fevers so notoriously are, [illegible]

[illegible] M. Maillot says it was possible, as late as the first [illegible] symptoms. This re- [illegible] than the morning [illegible] fevers of our own [illegible] bloodletting; by which [illegible] sometimes converted into [illegible] other hand, to have run [illegible] at the end of June, the [illegible] from the intermittent. [illegible] occasionally with a few [illegible] their course even with- [illegible] The point of practice which M. Maillot [illegible] notwithstanding this appearance of [illegible] administration of bark in full [illegible] necessity for this practice, were [illegible] the epidemic pernicious fevers of [illegible] quoted by M. Maillot, who [illegible] the fevers of Bona of 1832 and [illegible] M. Maillot gave large doses of the sul- [illegible] continued fever, with the exception [illegible] in which, although he thinks he [illegible] exception, he deferred its administration. [illegible] in which he enumerates cases of gastro-cephalitis, of acute gastro-enteritis, of follicular ileo-colitis (diarrhœa), of hemorrhagic ileo-colitis (dysentery), &c. &c., the disease, except in a few instances, was relieved in a few days. In almost all these cases, the patients began to take some light food on the third or fourth day. Of ninety-eight cases of gastro-cephalitis included among them, occurring in the month of July, not one became typhoid; and only five died, of whom two sunk the day after admission into the hospital. In the other cases, the solution of the disease was speedy, and the convalescence rapid.

There is something surprising in this account, and we have allowed the reader to share our astonishment, although, at page 26 of his memoir, when M. Maillot comes to relate particular cases, we find another article of treatment generally preceding the use of the sulphate of quinine, and which, although it is no other than pretty free bleeding, general and local, had not been before alluded to as of the smallest importance.

> "A soldier of the 59th, aged twenty-five years, was admitted into the hospital on the 8th of August, on the second day of an acute and excessively intense gastro-cephalitis. I immediately prescribed bleeding from the arm to fifteen ounces, the application of forty leeches to the epigastrium, and twenty leeches in the course of the jugulars; low diet; lemonade.
>
> "On the 9th, at the morning visit, the reaction was not entirely subdued; but the condition of the pulse, that of the skin, and all the other symptoms, denoted a remission indicative of approaching remittence or intermittence; and I consider it a continued gastro-cephalitis, passing into intermittent or remittent fever.—Low diet; lemonade; twenty-four grains of sulphate of quinine to be taken in a potion at one dose, and immediately.
>
> "Complete apyrexia established itself during the day. The apyrexia continued on the morning of the 10th: nevertheless, I prescribed another potion of twenty-four grains of sulphate of quinine, fearing that the fever might be tertian, and return the next morning. But the fever did not return; and convalescence went on rapidly. On the 18th, the patient was nearly on full diet." (p. 27.)

Such was very nearly the treatment of 295 cases of gastro-cephalitis; except that the sulphate of quinine, in the subsequent cases, was given immediately after the venesection; and, in certain circumstances, before any sanguine evacuation, as many of the men had been carried off by paroxysms of pernicious fever, some hours after the opening of a vein. Of the 295 cases thus treated, only twelve died, or 1 in 24. These results were certainly satisfactory: but M. Maillot observes, that such treatment would not be suitable to cases occurring in the north of France, in which dangerous typhoid affections, and (in case of recovery) tedious convalescence, would be the consequences. There can be little doubt that such would be the serious results of similar practice in the continued fevers of England; yet we believe there is much evidence of the most respectable kind among previous writers on the fevers of the Mediterranean, and of Italy, and of Africa, in support of the practice observed by M. Maillot.

Among the cases of true intermittent fever, 1582 were quotidian, 730 tertian, and 26 quartan. Of these 2338 cases of intermittent fever, the accession took place between midnight and noon in 1652, and between noon and midnight in 686. The greater number of accessions took place between nine in the morning and noon. 658 of the cases were simple, and 1680 complicated. In 1078 instances the intestinal canal was affected; alone in 343 cases: with the brain in 686 cases; with the lungs in 31 cases; with the brain and lungs in 13 cases. In 25 cases the spleen alone was diseased; and in one case the peritoneum alone. The brain was affected alone in 466 cases; the spinal cord in one; the lungs alone in 103 cases, and the pleura alone in five. In one case, a tertian, there was angina with the formation of a false membrane, and no other lesion. The intensity of all the complications was in direct ratio to the elevation of the temperature; and they were always unfavorably affected by the wind of the desert.

From the above observations, M. Maillot thinks himself authorized to conclude, that intermittent fevers are lesions of the nervous system in general, and especially of the cerebro-spinal axis. These lesions he considers to be hyperemiæ of the nervous centres; constituting, when local and slight, and uncomplicated with visceral irritation, simple intermittent fevers; but when intense and severe, the principal forms of pernicious fever; the *comatose* form, if the congestion is chiefly in the white central substance; the *delirious* form, if the membranes and the gray substance are affected; the *algide* form (with prolonged and icy coldness), if the congestion is established in the spinal cord. Death often takes place in all these forms, without any other viscus being sympathetically affected. The functional disorders of other organs, and their material affections, are only sympathetic, and are insufficient to produce the phenomena of intermittent fever. Dissection either discloses merely affections of the cerebro-spinal centre, or, if other viscera are affected, their diseased state is always complicated with lesion of the cerebro-spinal apparatus. The gastro-enteritis, or gastro-cephalitis are, in such cases, only subordinate irritative congestions, dependent on the return of the nervous phenomena, and do not constitute the intermittent fever.

In this country, pathologists have not been very ready to admit this theory of intermittent inflammatory conditions of the intestinal canal and brain. M. Maillot presents the doctrine in the least objectionable form. The secondary *congestions*, he says, are ordinarily very feeble in the first accessions, and disperse in the interval between one accession and the next. Complete apyrexia therefore ensues, without functional disorder of the digestive or respiratory passages. But when the accessions are many times repeated, and, above all, when they assume a quotidian type, each leaves some anatomical traces of congestion in the viscera affected. The capillaries soon become unable to disembarrass themselves of the blood which each accession determines to them; the tissues soon become unable to resist a state of congestion so frequently renewed, and the irritation "fixes itself anatomically," and betrays itself by symptoms more or less continued. Hence arises a prolongation of the reaction; that is to say, of the febrile symptoms, thirst, redness of the tongue, headach, heat of the skin, and all the symptoms of a gastro-enteritis, a gastro-cephalitis, a pneumonia, &c. according to the organs which are over-irritated (*surirrités*). (p. 36.)

To these remarks M. Maillot adds the very important practical observation, that simple irritations and those not of great intensity, and which yield in the intervals of an intermittent, give rise to symptoms in this class of fevers as marked and violent as those of acute gastro-cephalitis. This circumstance, he observes, if unknown or unattended to, might lead the practitioner to see inflammations where none exist, and to be afraid of administering the sulphate of quinine, on which alone the hope of preventing the returning accession of congestion must rest. In illustration of his practice, M. Maillot inserts a case in which, after bleeding during the paroxysm to fifteen ounces, the patient presenting the symptoms of acute gastro-cephalitis, twenty-four grains of sulphate of quinine were given at once, thirty leeches were applied to the epigastrium, and there was not another paroxysm. Of 250 cases thus treated, he only lost eleven, or 1 in 22: the fatal cases were all quotidian. In the pernicious forms of fever, with coma, he gave forty grains of the sulphate of quinine at a dose; and in one such case 148 grains were given in less than twenty hours, and the patient, from being in a state of coma, almost resembling death, became speedily and completely convalescent. In cases of the *algide* form, or with extreme coldness, ether was administered with the sulphate of quinine.

The most convincing proofs of the correctness of the above practice, and perhaps of the theory also, is that M. Maillot appears to have reduced the mortality in the fearful epidemic he had to contend with from 1 in 3½ to 1 in 20; for these results cannot be ascribed to any alteration in the character of the disorder; but became sensible when he began to use the sulphate of quinine more freely than he ventured to do at first, and to bleed less copiously. Subsequent engorgements of the abdominal viscera, dropsy, diarrhœa, so often considered to arise from the use of bark, were scarcely seen in any case; and M. Maillot considers them as the results not of the medicine, but of repeated paroxysms of the disease.

The title of M. Maillot's second and larger publication shows that further observation has made him sufficiently confident of the correctness of his views to apply the name of intermittent cerebro-spinal irritation to intermittent fevers. In this volume is of course collected much matter, illustrative of his views, and tending to show, by observations collected in different climates, that a high temperature imparts a peculiar character to intermittents, masking the apyrexia and periodicity, and giving an appearance of a continued type. The condition of the disease, he maintains, is an acute irritation, an hyperæmia of the great nervous centres.

The subject of the pernicious and anomalous forms of intermittents possesses much interest; and it is well and briefly treated in this publication. Affections to which the term *pernicious* has been applied are merely, M. Maillot observes (and as Broussais had before observed), cases characterized by the greater violence of the accompanying congestions; the danger depending on the importance of the organ affected. M. Maillot refers to the elucidation of these forms by Morton, Torti, Cleghorn, and others; and, putting aside the numerous and useless divisions of them made by Alibert, judiciously confines the term to those in which death is imminent or certain at the third or fourth accession, unless the malady is arrested. These forms depend, he maintains, on lesion of the cerebro-spinal apparatus, or of the abdominal organs, or of the thoracic viscera; a view of them which scarcely seems to admit of dispute.

Of the forms referrible to lesion of the cerebro-spinal apparatus, M. Maillot describes three, which are the most important: the comatose, the delirious, and the algide, or that icy-cold form for which we have no admitted English appellation.

In the *comatose* form, the stupor may vary in degree from simple oppression to profound carus. The pulse is full, large, without hardness, sometimes quickened, occasionally retarded; the respiration is slow, noisy, stertorous. The patient lies supine, and his limbs appear paralyzed; the jaw is firmly closed, and deglutition is difficult; sometimes there are epileptic spasms. These severe symptoms commonly occur with the second paroxysm, nothing taking place before to give warning of them, except it be some slowness of speech in the apyrexia. After an uncertain continuance of the comatose stage, the sweating stage follows, and the patient slowly recovers, wearing an extraordinary air of astonishment, and seeming to recover his senses one by one. Very full information concerning fevers of this kind is to be found in the excellent work of M. Bailly of Blois, published in 1825, and founded on observations chiefly made at the hospital of Saint Esprit at Rome, in 1820-21-22, to which we cannot do better than refer the reader, who is particularly interested in the nature and treatment of these dangerous affections, rare, or almost unknown, in our climate, but highly destructive in many localities where British practitioners in the army and navy are called upon to contend with them.

The *delirious* form of pernicious intermittent is, like the comatose, very common. Its name indicates its chief peculiarity. Death often takes place suddenly, without the supervention of coma: "life is broken by a single shock." If a salutary crisis ensues, the skin becomes moist and perspirable, the pulse loses its hardness, and the delirium gradually subsides.

The *algide* form is very peculiar. It is not, M. Maillot observes, at least generally, an indefinite prolongation of the cold stage. In the first stage of an intermittent, the sense of cold experienced by the patient is out of all proportion to the actual reduction of temperature; whereas in the algide fever, although the skin is icy-cold, the patient does not complain of coldness. And this cold state supervenes after reaction has commenced, and often suddenly. The circulation becomes disturbed, lowered, and the pulse can scarcely be felt, the temperature of the body at the same time rapidly decreasing. The extremities, the face, the trunk, become cold in succession, the abdomen remaining longer warm. The skin has the coldness of marble. The tongue becomes pale, moist, and cold, the lips are without colour, and the breath is cold. There is no thirst, and attempts to drink often excite vomiting. The actions of the heart become feeble, and only appreciable by auscultation. The intellectual faculties are undisturbed, and there is a sense of repose which is agreeable to the patient. All facial expression is lost. With this state, cholera may become conjoined, and the eyes then become hollow, glassy, and surrounded by a bluish circle. The approach of the algide form is so insidious as often to be mistaken for a remission produced by bloodletting, and the practitioner is only undeceived by the suddenness of the death of the patient. This deceitful calm is very strongly pointed out by M. Bailly, in his chapter on Diagnosis: he says that the patient may be walking about a few instants before his last attack; the accession is sudden, he lies down, and dies in a few hours.

Even when the pain (of the abdomen) and the danger are both considerable, the face has an appearance of calmness, as if its expression was no longer associated with the sufferings of other parts. Whenever, says M. Maillot, a sudden retardation of the pulse succeeds to reaction, and there is paleness of the tongue and discoloration of the lips, we should not hesitate to pronounce the case algide. Temporizing measures will be followed by death in a few hours. The patient dies as by an arrest of the innervation. If death does not take place, the pulse rises, the skin reacquires its natural warmth, and sometimes irritation of the brain or intestinal canal succeeds. Even this dangerous affection sometimes yields to remedial measures. The resemblance between this condition and cholera is commented upon by M. Maillot; and as, when left to itself, the algide form of fever is perhaps as fatal as cholera, he is of opinion that death in the latter affection has been too exclusively attributed to the excessive fluid evacuations.

These are the most general forms of pernicious intermittent. But sometimes the fatal symptoms are localized in the abdomen, constituting the gastralgic, choleric, dysenteric, and other forms. The *gastralgic* form is signalized by the acute, burning, tearing pain of the stomach; and in this state the face is contracted, and expressive of the utmost anxiety: this form is, however, seldom fatal. The *choleric* form is attended with very violent symptoms, which follow the periods of the fever, according to the expression of Torti, as the shadow follows the body. The *dysenteric* form, M. Maillot remarks, cannot with propriety be classed as pernicious, as when it is fatal, there is either chronic colitis, or the comatose, or delirious, or algide form of the disease has supervened. There are cases of intermittent fever in which jaundice takes place very suddenly, and disappears very slowly; this accident generally indicates a severe disease, to which the name of *icteric* fever has been applied.

To the forms in which sudden faintings occur during the attack, or palpitations, with pain at the heart and a feeble pulse, the names of *syncopal* and *carditic* have been given. When the lungs have been affected, the fever has been called *hemoptoic*, or *pleuritic*, or *pneumonic*, according to the complication.

Of all these forms of pernicious intermittent, M. Maillot gives examples; and he concludes that pernicious fevers only differ from ordinary intermittents by the violence of the congestions. They are most frequent in the hot seasons, when the visceral irritations which accompany the attacks are both more numerous and more intense.

In the chapters of M. Maillot's Treatise, in which illustrative cases are given, and in those on the causes, the mortality, the nature, and the diagnosis of intermittent affections, the reader will find much that will repay a careful perusal. His opinions on the subject of the treatment, founded on large experience, are all that we can find room to make the subject of present observation; and the importance of these is considerable: for, although the general principles on which an intermittent fever is to be treated, in whatever clime or season, must be the same, the indications always being to suspend the repetition of the attacks and to obviate their local effects, these indications require to be more promptly pursued in one locality than in another. The treatment adapted to the endemic of low and marshy places, may be unsuitable in a country that is higher and drier; and the means found to be successful in a temperate latitude may be inefficacious in a climate of higher temperature.

The remarkable cases published in the second edition of his work by the late lamented Dr. Mackintosh testify beyond doubt that the practice of bleeding in the cold stage of an intermittent has often been productive of immediate relief of the coldness, the sense of debility, and all the other phenomena of the cold stage, and has even appeared to cut short the disease. The relief was sometimes obtained after the loss of a few ounces; but the loss of twenty-four ounces was in other instances required. Several cases quoted from the Calcutta Transactions, or communicated by practitioners in India, afford similar evidence; and if M. Maillot's general plan of treatment had not been attended with results upon the whole so favorable to his practice, we might regret that he never ventured to give this method a trial. In the hot stage he generally bled to twelve or fifteen ounces, particularly when headach was present; and local bleedings were also practised when local symptoms pointed out their propriety: thirty or forty, or sometimes sixty or eighty leeches were applied to the epigastrium, or in the course of the jugular veins, or to the head, according to the circumstances of the case. M. Maillot thinks there is good reason for the opinion that the anterior lobes of the brain are more susceptible of inflammation than the other regions, and that leeches are consequently applied with most advantage to the forehead. The persistence of pain in the head, or marked disturbance of other organs, was considered an indication for repeating these measures. Obstinate vomiting was relieved by small portions of ice taken from time to time, and the diet was rigidly regulated. Of the different kinds of bark, M. Maillot prefers the red (*oblongifolia*), not only to the gray (*lancifolia*), which contains only cinchonia and not quina, but also to the yellow (*cordifolia*), from which, on account of the quantity of quina it contains, the disulphate of the London pharmacopœia is prepared. But he prefers the sulphate of quinine (disulphate of quina) to all preparations of bark; the doses being more easily regulated, the stomach tolerating it better, and its action being surer and speedier; qualities which practitioners are not all equally inclined, we think, to accord it the possession of. When the stomach rejected it, the sulphate was given in a lavement; if purging or colic ensued, the endermic method was had recourse to; and sometimes the patients were put into a bath saturated with cinchonia. In salicine and ilicine, M. Maillot places, as might be expected, little confidence. He seems not to have given any trials to opium; but he quotes the opinions of Lind and of M. Bailly to show that its power is only sedative, and not febrifuge; and that, if it seems to shorten the duration of the attacks, it does not prevent their recurrence. We have little doubt that there are cases in which opium mitigates the sufferings incidental to intermittents; and in some cases, at least in our own climate, a large dose given in the cold stage will put an end to it, and even to the disease altogether. M. Maillot agrees with M. Bailly in condemning the combination of antimony with opium as useless. With less reason he utterly condemns arsenical preparations. He also reprobates the notion of giving an emetic or even a purgative before commencing the administration of the sulphate of quinine; being of opinion that the attention of the practitioner, being directed to the subduing of abdominal or other irritations by leeches, would be uselessly given to removing any internal sources of irritation, which he looks upon as imaginary, and but the relics of exploded doctrines. In this respect, theory certainly interferes unfavorably with M. Maillot's practice; and that there are even cases of intermittent in which, after the application of leeches to the epigastrium, and the administration of a purgative, the disorder will disappear before a grain of bark is given, every one who has seen much of ague must have found. The general propriety of M. Maillot's rule, however, not to delay giving the sulphate of quinine as soon as a complete apyrexia is established, cannot be controverted; and the old doctrine of coction and crisis led without doubt to hurtful delay, during which the constitution suffered greatly from repeated paroxysms. He is, as we have seen, decidedly an advocate for giving the sulphate in doses of twenty or more grains, which he administers in four ounces of water; and the time he prefers is three or four hours before the expected attack. His experience in Africa determined him at length not to repeat the medicine more than once or twice after the suppression of a paroxysm; and he has pursued the same plan with success since his return to France. The prolonged use of the medicine is objectionable; and he agrees with M. Hepple in believing that it does not even prevent the return of the fever after a given time so certainly as having recourse to it anew, at the expected periods of return; supposing such periods to be ascertained, as stated by M. Hepple, namely, the eleventh and twenty-first day in quotidians and tertians, and between the twentieth and thirtieth in quartans.

The unfortunate tendency to a relapse makes it desirable, M. Maillot observes, that the patients should consider themselves as convalescents for two or three months, however well they may appear to be. When relapses take place, bleeding, except by leeches, is generally less required than at first; but the sulphate of quinine should be given in increased doses. In some cases a complete change of residence is indispensable to recovery; soldiers who were sent home to France after obstinate and renewed attacks often recovered health during the voyage.

The fevers of marshy countries during the hot-seasons, and the fevers of hot climates in all seasons, demand the application of all these parts of treatment with yet more promptitude and boldness. Care to remove the complications of these destructive fevers becomes absorbed in the intense anxiety of the practitioner to remove the malady itself, of which they form an essential part in such seasons or in such regions; and the treatment, less governed by the constitution of the patient or the period of the disease, becomes hardily empirical. In these circumstances we have seen that M. Maillot soon learned to administer the quinine without delay, and even to dispense with preparatory bloodletting. By the repetition of a violently congestive state, the intermittent fever had, he found, a tendency to become continued and to assume the pernicious form. These tendencies he combated by bleeding when the patient was first seen, either in the apyrexia or in any stage of reaction. If there remained headach in the interval, at least fifteen ounces of blood were taken away; and if the intestinal canal was irritated, leeches were applied to the epigastrium. If these symptoms were violent during the paroxysm, and still severe in the interval, twenty, or twenty-five, or thirty ounces of blood were taken at once.

These bleedings, which will not be considered so large by British practitioners as they seem to be in M. Maillot's estimation, were not so well borne by the patients in the great heats of July as in the spring; and sometimes the fevers put on a pernicious form afterward, an event to which M. Maillot attributes the prejudice against bleeding in these cases which exists in many countries, and especially in Italy. After the bleeding, M. Maillot insists strongly on the propriety, whatever the circumstances, of immediately giving the sulphate of quinine, in doses of from twenty-four to forty grains in a few ounces of water; after which all the symptoms will be found to disappear in a few hours, and, to use his own words, "as if by enchantment." Certainly, there is nothing more astonishing than the results of this kind of treatment appear to be in the various and fearful, and one would almost have said hopeless, cases, not only related by M. Maillot, but by Torti and other authors of authority on the fevers of hot climates. Combinations of symptoms, from which recovery would seem almost impossible, are dissipated with a rapidity of which in our climate we have no analogous examples. Some of the cases in M. Maillot's concluding chapter, are really marvellous, but at the same time, we are assured, perfectly worthy of belief. In one of them, an intense case of the comatose pernicious form of fever, 128 grains of sulphate of quinine were administered in a few hours. On the third day from the admission of this patient into the hospital, there was complete apyrexia, and on the fourth he was convalescent. The patient was also bled twice; and the importance of bleeding, both in the cases of comatose and delirious pernicious intermittent is strongly urged. A bleeding from the arm, followed by opening the temporal artery, was found to be extremely efficacious; and most so in the comatose form.

It appears, also, that blisters and sinapisms were applied in many cases; the first often preparatory to the introduction of the sulphate of quinine by the blistered surface, when, as in the choleric form, the medicine could neither be swallowed nor given by the rectum. In these forms, also, opium was sometimes conjoined with the quinine to the extent of ten grains in the course of the day.

The *algide* form is, as may be supposed from the description which has already been given of it, that in which the danger is the most imminent, and the necessity for vigorous practice the most urgent. M. Maillot gives an example in which forty grains of sulphate of quinine and two drachms of ether were given in four ounces of water, at two doses, in the course of an hour: a starch opiate injection, with sixty grains of the sulphate and two drachms of ether, was ordered at the same time; sinapisms to the legs, and a blister to each thigh. Under this sharp practice the patient began in a few hours to recover warmth, and the heart to act more forcibly; but the next morning the amendment was so slight, that a sinapism was applied to the whole length of the spinal column, and a lavement given with sixty grains of sulphate of quinine and three drachms of ether: strong reaction then took place, and recovery commenced.

Upon the whole, M. Maillot's book must be considered as a very interesting addition to our knowledge of the forms and treatment of the intermittent fevers of countries, the diseases of which have not yet, for want of sufficient opportunities, been fully observed. Any one who has looked into the discordant evidence, the violent controversies, the conflicting practical rules laid down by practitioners assuming to themselves the credit of experience in the fatal endemics of warm countries, must receive with curiosity at least, and with a somewhat grateful feeling, such details as those afforded by M. Maillot; and although some of his observations may be a little startling, the results of his practice, we repeat, are such as very strongly support his theoretical views. The work is written in a temperate spirit, and indicates both experience and habits of careful reflection.

Very little of M. Maillot's work is taken up with mere theoretical discussions. Alluding to some of the doctrines of intermittence, and particularly to that of M. Bailly, who ascribes it to the periodical alternations of the horizontal and erect positions in human beings, he prudently abstains from much personal speculation, either on this subject or on the mode of action of quina in subduing such a state. Dr. Manni's work has the air of an academical essay on questions of this kind, eloquently and diffusely written, but of no very direct application to practice. We may, however, mention Dr. Kremer's theory, leaving it for the reflection of the reader.

He asserts, that in all the intermittent fevers which, in his own neighbourhood (where they frequently occur), he has had an opportunity of observing, there exists at the situation of the first dorsal vertebra a more or less severe pain on pressure. In order to assure himself that this was not merely a characteristic of the particular epidemic during which he made his observation, he went to Antwerp, Brussels, Ghent, and Paris, where he saw many cases of intermittent fever, and met with sufficient evidence to confirm his previous experience.

In order to perceive the symptom, the pressure must be made from behind forwards, with the fingers upon the spinous process of the individual vertebra, not upon several vertebræ together; for in this latter case one bone is less affected in its relations with the others than in the former. If the intermittent fever is considerable, or old, or masked, pressure on the first dorsal vertebra, by giving pain, will suffice to evince the existence of fever. In simple or mild cases, the pain may be but trifling, and may require, particularly in phlegmatic individuals, that the attention shall be directed to it; and in order to render it evident, pressure should be first made on the first cervical vertebra, then on the last dorsal, and then lastly on the first dorsal, when the patient may be asked if he feels any difference in the sensations produced. The pain exists during the paroxysms, as well as in the apyretic interval, is stronger in epidemic than in sporadic intermittent fever, exists in all forms, and continues during the sequelæ.

We give this statement as we find it in Dr. Kremers' book, and with the view of calling the attention of our readers to corroborate or disprove it by their own observation. We confess that we are extremely sceptical as to its truth; and we find from the German journals, that some of the author's countrymen have not found the same results in examining cases of the disease, since the publication of his work. If it were established as a general truth, it would be important in a practical as well as a pathological point of view, as is remarked by the author. In practice, for instance, it would be a valuable diagnostic sign in cases of masked intermittent, and the visceral affections springing from such a cause. The author observes that we are entirely in want of a distinct diagnostic sign of the duration of intermittent fevers. A patient appears, for example, to be entirely cured of an autumnal ague; some few morbid phenomena occur during the winter, which appear to be quite unconnected with the fever, but which do not cease until after the occurrence of two paroxysms of a spring fever; or an intermittent fever returns during many successive years at a regular period, and does not come to an end until a gastric fever has run its tedious course.

Now, as the author observes, if the pain in the spine continues during the apyretic interval, it may also continue during this tedious period of indisposition, and may thus afford us a diagnostic sign of the constant connexion of apparently dissociated phenomena.

ART. IV.

1. *Recherches Anatomiques et Physiologiques sur l'Organe de l'Ouie et sur l'Audition dans l'Homme et les Animaux Vertébrés, &c.* Par G. BRESCHET, &c.

Anatomical and Physiological Researches on the Organ and Function of Hearing in Man and the Vertebrate Animals. To which is added the History of the Nervous Plexus of the Tympanum. By G. BRESCHET, Member of the Institute of France, &c. With 13 Plates.—*Paris*, 1836. 4to, pp. 295.

2. *Handbuch der theoretischen und praktischen Ohrenheilkunde.* Von Dr. CARL GUSTAV LINCKE, ausübendem Arzte und Wundarzte, akademischem Privat-docenten zu Leipzig, &c. Erster Band, *die Anatomie, Physiologie, und pathologische Anatomie des Gehörorgans. Mit fünf lithographirten Tafeln.—Leipzig*, 1837.

Manual of Theoretical and Practical Ear-Medicine. By Dr. CHARLES GUSTAVUS LINCKE, of Leipzig. Vol. I. *containing the Anatomy, Physiology, and Morbid Anatomy of the Organ of Hearing. With five lithographic Plates. — Leipzig*, 1837. 8vo, pp. 682.

3. *The Cyclopædia of Anatomy and Physiology.* Edited by R. B. TODD, M.D. F.R.S. &c. &c. Parts XIV. and XV., *Articles*, "Hearing, Organ of," by T. WHARTON JONES; and "Hearing," by Dr. R. B. TODD.—*London, August*, 1838, *and January*, 1839.

4. *A Treatise on the Structure, Economy, and Diseases of the Ear, being the Essay for which the Fothergillian Gold Medal was awarded by the Medical Society of London.* By GEORGE PILCHER, Lecturer on Anatomy and Surgery &c. *With* 14 *Lithographic Plates.— London*, 1838. 8vo, pp. 324.

5. *Handbuch der Physiologie des Menschen.* Von Dr. JOHANNES MÜLLER, &c.

Manual of the Physiology of Man. By Dr. JOHN MÜLLER, &c. Part II. of Vol. II., *containing* "The Senses." *Section Second*, "The Sense of Hearing."—*Coblentz*, 1838.

6. *Recherches Pratiques sur les Maladies de l'Oreille, et sur le Développement de l'Ouie et de la Parole chez les Sourds-Muets.* Première Partie: *Traité du Cathétérisme de la Trompe d'Eustachi, et de l'Emploi de l'Air Atmosphérique dans les Maladies de l'Oreille moyenne.* Par le Dr. DELEAU, Jeune.

Practical Researches on the Diseases of the Ear, and on the Development of Hearing and Speech in the Deaf and Dumb. First Part: *A Treatise on Catheterism of the Eustachian Tube, and on the Employment of Atmospherical Air in the Diseases of the Middle Ear.* By Dr. DELEAU, Junior. *With two Lithographic Plates.* —*Paris,* 1838. 8vo, pp. 431.

7. *Philocophus, or the Deaf and Dumb Man's Friend.* By J. B. (BULWER,) surnamed the Chirosophus.—*London,* 1648.

THE first book in the above list contains researches, which, we are informed by M. Breschet in his Introduction, were commenced in 1815, on the occasion of a *concours,* at the Faculty of Medicine of Paris, for the situation of *prosector.*

The results have been from time to time communicated in different memoirs to the Academy of Sciences; and though published in great part some five or six years ago, in the "Annales des Sciences Naturelles," they were only formally given to the world in 1836, first in the "Mémoires de l'Académie Royale de Médecine," (Tome cinquième, 3^e. Fascicule,) and then in a separate form. We have devoted some attention to researches on the structure of the ear, and can therefore, with some confidence assert, that M. Breschet, in the work before us, has unravelled the intricacies of the labyrinth very successfully, and has considered its various parts in a manner more consonant, perhaps, with the principles of philosophical anatomy than had been done up to the time he wrote. The plates attached to the work are well executed, and in general highly illustrative of the verbal descriptions. This favorable opinion is nevertheless open to some exceptions, both of a general and specific nature. The specific we shall have occasion to notice in the course of this article. The principal of the general objections we have to make are; that there are numerous repetitions, whereby the memoir is extended over a greater space than was necessary; that M. B., though he has proposed a few sufficiently appropriate new names, tries to make too much of his nomenclature; and that, perhaps, he assumes too hastily as discoveries of his own certain points in the anatomy of the ear, which have certainly been noticed and investigated by others before him.

The work of Dr. Lincke contains a most complete body of information regarding the ear, in an anatomical, physiological, and morbid-anatomical point of view. In it the history of the subject is portrayed with candour and learning, and the descriptive part is at once minute and accurate. Dr. L. has, to be sure, contributed little if anything new of his own, but he has given us an admirable abstract of all that has been written on the subject up to the time he published. A second volume on the Diseases of the Ear was promised soon to follow this on the Anatomy and Physiology, but it has not yet appeared; nor is it likely, we are afraid, soon to appear in a separate form. The cause of this we suspect is, that the first has not been sufficiently patronised. If so, we are not altogether surprised at it; for excellent as it is, the first volume is not a production likely to be appreciated by or to come within the wants of many; and as to a second volume on the Diseases of the Ear, executed in the same spirit, though it might, together with the one before us, form a valuable *Otological Cyclopædia*, it would not, like Kramer's work, be the manual required by the practical surgeon. However, we are glad to say, that we shall not be entirely deprived of the results of Dr. Lincke's labours in *ear-medicine*,* as we find it announced that he is to supply the articles on the diseases of the ear for the Dictionary of Surgery, commenced by Professors Walther and Radius, of Leipzig, and the late Professor Jaeger, of Erlangen, and still continued by the two former. An article on hearing instruments, and another on the injection of the Eustachian tube, have already appeared.

The article on the organ of hearing, by Mr. Wharton Jones is, from the nature of the publication in which it appeared, necessarily very much condensed. The author has nevertheless contrived to give a very comprehensive account of the structure of the ear, including its development and irregular conditions. He has drawn a just physiological estimate of each of the component parts of the organ, and determined their importance, relations, and analogies, throughout the whole animal series. Though Mr. W. J.'s descriptions seem for the most part newly sketched, or verified from nature, he has not refrained from making good use of Breschet's work. He has sometimes adopted, sometimes corrected the views of the latter, and has borrowed the most expressive of his figures. As our views of the structure of the ear coincide for the most part with those of Mr. Wharton Jones, we shall avail ourselves largely of his labours in the composition of the present article.

The article "Hearing," by Dr. Todd, is a very judiciously composed summary of what is known or conjectured regarding the function of the several portions of the organ of hearing, including the principal acoustical experiments and observations bearing on the subject.

The second part of the second volume of Professor Müller's work having come to hand only as this article is going to press, we are not able to make use of it so far as we would wish. The section on the physiology of hearing manifests the same elaborate investigation which characterizes the other parts of this excellent and distinguished physiological treatise.

Mr. Pilcher's volume is the Essay for which the Fothergillian gold medal was awarded by the Medical Society of London last year. The subject of the ear is one which has often before engaged the attention of the Medical Society of London, as appears from the articles in different volumes of their "Memoirs." Among others, "Observations on Deafness from Affections of the Eustachian Tube," by James Sims, M.D., the President of the time, in volume first; and in volume third, "Case of

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* Though strongly disliking the introduction of new words, particularly words borrowed from the modern languages, we are induced to tolerate this Germanic barbarism for the sake of conciseness and convenience.—ED.

Original Deafness with the Appearances on Dissection," by J. Haighton; for this Mr. Haighton received a silver medal. The Medical Society of London therefore, in selecting the "Ear" as the subject of competition for the Fothergillian medal of last year, have worthily continued to follow up what appears to have been an object with the Society from an early period of its existence. And every one must admit that they could not have more "judiciously" (to use an expression in Mr. Pilcher's introduction) "fulfilled the benevolent intention of the enlightened and liberal physician who committed this important trust to their charge," than in the attempt to further a practical knowledge of ear-medicine. The proposing of an essay on the "Structure, Economy, and Diseases of the Ear" was certainly of all modes the one best calculated to promote a practical knowledge of the subject, provided care were taken that the essay selected as worthy of the prize, was at least on a level with the present state of knowledge on the subject. It remains to be shown, in the course of the present article, whether this has or has not been done, in awarding the prize to Mr. Pilcher's Essay.

M. Deleau's work can scarcely be considered as a new one, being principally made up of his former memoirs on the same subjects. The present volume, which contains 431 pages, is entirely devoted to the consideration of catheterism of the Eustachian tube, and the employment of injections of atmospherical air. With all due allowance for the explanations and counter-explanations of the first employer of any particular mode of treatment, and especially of such a mode of treatment, and for such affections too, as that proposed and employed by Deleau, it must be confessed that this is much greater space than was actually required. In Deleau's work, there is much to praise, but there is also much to blame. A desire to make things look more favorable than they really are too often breaks through.

To our list of recent works on diseases of the ear, we have added, more for its curiosity than anything else, the ancient small volume entitled "Philocophus, or the Deaf and Dumb Man's Friend." It is a quaint dissertation on the possibility of persons born deaf learning to speak and converse by the assistance of the eye alone. The text for the commentary is the history, related by Sir Kenelm Digby, of a young Spanish nobleman born deaf, but who yet, by the sharpness of his eye and the acuteness of his intellect, acquired the power of knowing, from the motions of the lips of the speaker, the words pronounced, and thus learned to speak.

Uninfluenced by any other desire than to convey to our readers information as to what knowledge we really possess of the ear and its diseases, we take the books before us to serve as the text for three sections on the subject. The first, on the Anatomy, the second, on the Physiology, and the third, on the Diseases of the Ear.

The analysis and comments we shall have occasion to make will, we believe, justify the opinion of each work which we have just expressed.

I. ANATOMY OF THE EAR.

Of the history of the progress of discovery in the anatomy of the ear, Dr. Lincke gives a good sketch. He commences by showing that, as was to be expected, the knowledge of the ear possessed by the ancients did not extend beyond the external parts. The works of Erasistratus and Herophilus, the celebrated practical anatomists of antiquity, have not come down to us, but some idea of the extent of their acquaintance with the structure of the ear may perhaps be conceived, if we admit, which is probable, that the nomenclature of the parts of the ear employed by Rufus of Ephesus, in his work "De appellationibus partium corporis humani," written in the first century after Christ, was derived from Herophilus. The names to which we allude, *λόβος, ἕλιξ, ἀνϑέλιξ, κόγχη, τράγος, ἀντίτραγος*, &c., indicate only a knowledge of the external parts of the ear, to designate which, indeed, they are still in use. From this it would seem that, up to the time of Rufus of Ephesus, the parts contained within the *os temporis* remained unknown, unless we can understand a passage in Celsus (lib. viii. cap. i.) as evidence to the contrary. Galen compared the internal ear to a labyrinth. He was, however, quite unacquainted with the connexions, situation, and direction of the cavities and passages in the petrous bone.

The real investigations and discoveries in the anatomy of the ear began at the end of the fifteenth century, and, as was natural, proceeded from the external to the internal parts.

> "If we review," says Dr. Lincke, "what has been done in the different epochs from Vesalius to Casserius, from the latter to Duverney, from Valsalva to Scarpa, and from Soemmerring to the present time, it must be confessed that the knowledge of the structure of the ear has been enriched by the most gifted and acute anatomists; and it is but justice to say, that it is especially to Italians that we are indebted for the greatest number of contributions: to the active spirit of investigation of the German and also of the French anatomists of the present time, however, much praise is due, as it is by means of their accurate and fundamental investigations that the subject has been developed so perfectly as it now is." (*Lincke*, p. 26.)

M. Breschet, in the work before us, does not profess to consider the ear in any other than the mammiferous animals; he, however, refers, by way of illustration, to several points of its structure in the other classes.* Mr. Wharton Jones describes the human ear, and only makes use of occasional general illustrations, drawn from comparative anatomy. Dr. Lincke confines himself exclusively to the human ear: whilst Mr. Pilcher devotes his first chapter, consisting of six sections, to comparative anatomy.

In our analysis of the books on the anatomy of the ear, we shall follow the usual order of description, taking the external parts first and then the internal, which is the order followed by Mr. Pilcher, in his Anatomy of the Human Ear, and also by Dr. Lincke. By this means we shall not be obliged to introduce M. Breschet until his presence is actually required. We would wish it, however, to be understood, that we consider the order followed by Mr. Pilcher, in his "general observations,"—an order which has been carried out to its fullest extent by Mr. Wharton Jones,—viz. the internal parts first, and then the external, as being the more philosophical.

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* He has composed other three monographies on the structure of the ear in birds, fishes, and reptiles, of which the first two are published. M. B. also promises to publish the history of the cavity of the tympanum in man and the mammifera, with that of the development of the various parts of the ear, as a sequel to the work before us.—Rev.

In limine, we have to remark, that the external ear is formed of *true cartilage*, covered by a *perichondrium*, and not of *fibro-cartilage*, as Mr. Pilcher (pp. 28 and 36) and Dr. Lincke (p. 70) say. At p. 39, Mr. P. says "the *fibro*-cartilage of the pinna is highly elastic, and is formed of several pieces, &c." The *true* cartilage of the pinna, we believe, is composed of but a single piece; fissured here and there, certainly, but presenting a continuity of substance throughout, and also with the cartilage of the auditory passage.

Concerning the membrana tympani, Mr. Pilcher remarks (p. 47), that "the positive existence of a proper membrane between the two reflexions" (of the lining membrane of the auditory passage on the one hand, and the mucous membrane of the tympanum on the other,) "is not yet determined." And again he says, near the bottom of the same page, "most dissectors allow that an independent fibrous membrane does exist, and that it is not merely a thickening of the mucous surfaces." All this is unnecessary doubt about a comparatively simple matter. Let us compare what is said on the subject by Lincke and Wharton Jones. This, by the by, is one of the few subjects on which Dr. Lincke offers anything original of his own.

> "According to the investigations which I instituted on the subject, I recognize," says Lincke, "only three layers in the membrana tympani, one external, one middle, and one internal. The *external* is loosely connected, and is the blind end of the general integument of the inner surface of the auditory passage. The *middle layer* is a pretty strong, dense, fibrous, and elastic membrane, which may by maceration be separated into two lamellæ, the outer of which is much more delicate and finer than the inner. It is, as I suppose, formed by an intimate incorporation of the periosteum of the tympanic cavity, and the osseous auditory passage, and is not immediately fitted into the tympanic ring, but takes its origin directly from a proportionally very thick, firm, and ligamentous ring, which partly lies in the *groove* for the membrana tympani, and is very intimately and closely connected with it. The two lamellæ are so closely connected with this ring, that I have not succeeded, after weeks of maceration, completely to separate them from it. The muscular fibres, first described by Home, and admitted by Sprengel, Lenhossek, Meckel, Buchannan, and other anatomists and physiologists, I have not been able, even with the assistance of the microscope, to perceive." (*Lincke*, p. 94.)

"As regards the composition of the membrana tympani," says Mr. Wharton Jones, "it consists of a proper membrane and two borrowed layers, &c." Concerning the nature of the external borrowed layer, we think neither Lincke nor Wharton Jones are sufficiently explicit. We have seen above that the former calls it "the blind end of the general integument of the auditory passage." The latter describes it as "a delicate continuation in the form of a blind end of the lining of the auditory passage." But, again he says, in speaking of its connexion with the proper membrane, "it readily separates from it by putrefaction, and can be drawn out along with the rest of the *epidermis* of the external auditory passage in a cul-de-sac." The fact is, the layer, so easily separated from the external surface of the membrana tympani, is merely epidermis. This being so, we must have, also, in the composition of the membrana tympani, some sort of chorion. The fibrous layer of the membrana tympani has in its structure a continuation of the elements of the chorion of the skin of the auditory passage. This remains inseparably incorporated, whilst the epidermis comes away by putrefaction. We agree, in other respects, with the following remarks regarding the "structure of the proper membrane."

"The proper membrane can be divided into two layers, an outer thin one, consisting of radiating fibres, and an inner thicker layer, which is less distinctly fibrous, though when torn it does indicate a fibrous disposition, and that in a direction opposite to the former. The radiating fibres run from its circumference towards the centre, to be fixed to the handle of the malleus, along its whole extent. Towards the centre they become stronger, and being of course more aggregated, the layer which they compose is thicker and more compact in the centre than towards its circumference. The fibres which cross the radiating ones are also more aggregated at the centre. They run parallel with the handle of the malleus, and turn round its extremity. At the circumference of the proper membrane there is a thick, firm, ligamentous, or cartilaginous ring, which is fixed in the groove of the bone. This ligamentous ring appears to be formed by an aggregation of the circular fibres interwoven with the peripheral extremities of the radiating ones. The part of the membrana tympani, midway between its centre and circumference, is the thinnest. The radiating fibres have been supposed to be muscular by Sir Everard Home and others, but this has not been confirmed by microscopical examination." (*Wharton Jones, Cyclopædia,* &c. No. xiv., p. 545.)

The following is the same writer's account of the connexion of the malleus with the membrana tympani:

"That edge or ridge (on the handle of the malleus) which is continued down from the short process is turned outwards, and corresponds to the membrana tympani; into it, indeed, along its whole extent, the central extremities of the radiating fibres of that membrane are inserted. The extremity of the handle of the malleus, which is curved forwards and outwards, is compressed, but in a direction contrary to the rest of the handle; so that one of the flat surfaces, that corresponding to the outer ridge of the rest of the handle, is connected with the membrana tympani at a point below its centre, and nearer its anterior edge. It is at this point that the bottom of the concavity is, which the membrana tympani presents externally. At its upper part, the membrana tympani is pushed outwards by the short process of the malleus, which projects towards the auditory passage." (*Ib.* p. 547.)

The membrana tympani is a part of so much practical importance, that we make no apology for dwelling on it at this considerable length. Mr. Wharton Jones has given an exposition of the subject, useful alike in a surgical and in a physiological point of view.

"Mr. Shrapnell describes, at the anterior and superior part of the membrana tympani, above the short process of the malleus and its suspensory ligament, and where the groove in the bone is deficient, a flaccid tissue, composed of irregularly arranged fibres, to which he gives the name of *membrana flaccida,* in opposition to the rest of the membrana tympani, which he calls *membrana tensa.* This flaccid tissue is more developed in some of the lower animals, the sheep and hare, for instance, than in man, and can be readily made to bulge out towards the auditory passage, by blowing air into the Eustachian tube. But we cannot look upon it with Mr. Shrapnell, as properly forming any part of the membrana tympani. It is merely a mass of dense reddish vascular cellular tissue, surrounding the neck of the malleus, and continuous with a similar tissue, found under the lining integument of the upper wall of the osseous auditory passage. It is this same tissue which has been described as a muscle and sometimes as a ligament." (*Ib.* p. 546.)

It is of the greatest importance to the surgeon to know well the natural appearance which the membrana tympani presents in the living body.

"It is situated at the bottom of the external auditory passage, between which and the cavity of the tympanum it is interposed, like a partition. It is a thin, semitransparent, glistening, dry-looking membrane. Its shape is an oval, truncated at one extremity, the upper. Rather more than the upper half of its vertical diameter is traversed by the handle of the malleus, *which*, when the membrane is examined, in the living subject, by means of the speculum auris, *appears directed from above downwards and backwards*." (*Cyclopædia*, &c., p. 545.)

In direct opposition to this last assertion, Mr. Pilcher informs us, at p. 53, that the handle of the malleus descends from the cervix and processus brevis, being a little inclined forwards, &c.

We would recommend to our young friends to exercise themselves in examining the auditory passage and membrana tympani in the living person, and to note well the ordinary position and direction of the handle of the malleus. One or two examinations will, we think, be sufficient to inform them whether, in regard to the upright position of the body, the handle of the malleus be directed backwards or forwards.

We now turn to another part of great importance, in the surgical anatomy of the ear, viz. the Eustachian tube. The practical points to be considered, in regard to this, are its length, its width at different places, and the size and position of its guttural orifice.

Length. The length of the Eustachian tube is given, by Lincke, at one inch seven or eight lines, Parisian measure, the osseous portion of the tube being seven or eight lines, (p. 147,) and the cartilaginous portion about one inch. (p. 149.) Wharton Jones states the length of the Eustachian tube at one and a half inch, (p. 546,) the osseous portion being half an inch, (p. 546,) and the cartilaginous and membranous portion about one inch. (p. 550.) Pilcher says (p. 51) the Eustachian tube is upwards of one and a quarter inch long.

Width. In regard to the width of the Eustachian tube, it is known that its guttural orifice is wider than that by which it opens into the tympanum. It is also acknowledged that the narrowest part is where the bony and cartilaginous portions join, but there is much difference of opinion as to the exact degree of width of the narrowest part. Mr. Pilcher describes the tube as being slightly contracted from the "tympanum to the cartilaginous portion," (p. 51;) and delineates, in plate 9, (the different figures of which plate, we are given to understand, are "faithful representations of recent dissections of the human ear,") the narrowest part as being about one tenth of an inch. Lincke says on the point, (p. 147,) from the tympanum, the Eustachian tube gradually contracts, and is, as Valsalva has already correctly asserted, at the end of the bony portion, from three quarters to one line in its largest diameter.

Its transverse diameter, on the contrary, continues Lincke, amounts, at its opening into the tympanum, to about one line; it then becomes gradually smaller, and measures, at the end of the osseous portion, half a line. Sometimes this transverse diameter is still less, especially where the canal is yet lined by its membrane. In consequence of this, the caliber of the canal is at this place so narrow, as scarcely to admit the passage of a thin probe. Lincke further adds, "the diameter of a quarter of a line, at the narrowest part, as given by Kramer, is too small." We would remark, in regard to the criticism of Kramer by Lincke, that Kramer says the width, at the narrowest part of the tube, is one thirty-second of an inch or a quarter of a line. Kramer's error consists merely in speaking of one thirty-second of an inch as equal to one quarter of a line, whereas, the thirty-second of a Paris inch is rather more than one third of a Paris line.*

The following is Mr. Wharton Jones's observations on this subject:

> "Its (the osseous portion of the Eustachian tube) caliber contracts in its course forwards, and is compressed from without and below inwards and upwards. In the dry bone it is wide enough to admit the end of a quill stripped of its feathery part, about one twelfth of an inch thick. In the recent state, when lined by its mucous membrane, it is very much narrower." (*Cyclopædia*, p. 549.)
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> "The point of junction (between the bony and cartilaginous portions of the tube) is the narrowest part of the tube; in the recent state about one thirtieth of an inch in diameter, just sufficient to admit a small probe." (*Ib.* p. 550.)

We thus see that Kramer and Wharton Jones nearly coincide; for the Paris inch, which we suppose Kramer uses, is somewhat longer than the English inch, according to which the English author calculates.

The size and position of the guttural orifice are the most important points in the surgical anatomy of the Eustachian tube. On the subject, Lincke says, (p. 148,) that he has not found Kramer's account of it correct. The following is Mr. Wharton Jones's account of it:

> "The mouth of the Eustachian tube in the throat forms an oval-shaped fissure, about three-eighths of an inch long, bounded anteriorly and posteriorly by prominent swollen edges. The fissure is directed obliquely, from above downwards and from before backwards, and is situated at the upper and lateral part of the pharynx, behind the soft palate. In reference to the nasal passage, my observation agrees with that of Kramer, that the *lower angle* of the guttural orifice of the Eustachian tube *lies a very little deeper than the horizontal line of the lowest meatus, whilst the upper angle is a little deeper than the horizontal line of the middle meatus.*" (*Cyclopædia*, p. 550.)

Still another point in the anatomy of the ear, of consequence in a practical point of view, is the nature of the membrane which lines the cavity of the tympanum. Mr. Wharton Jones describes it as follows:

> "The lining membrane of the cavity of the tympanum is in continuity with the mucous membrane of the throat, through the Eustachian tube. Extremely delicate, and in some parts very vascular, it is not merely a mucous membrane, but is theoretically a combination of periosteum and mucous membrane, being what Bichat called fibro-mucous. It invests all the elevations and depressions observed on the walls of the tympanum, and extends into the mastoid cells. The outer layer of the membrane of the fenestra rotunda, *membrana tympani secundaria*, is a continuation of it. The base of the stapes is fixed by its circumference to the outer edge of the groove, which encircles the vestibular fenestra, by a membrane or ligament. The lining membrane of the vestibule, continued over the base of the stapes from within, also invests the inner surface of this annular ligament, whilst the outer surface of it is covered by the membrane lining the tympanum, as it is reflexed on the stapes. The membrane lining the tympanum invests the small bones and the tendons of their muscles, where they run free in the cavity. A fold of it fills up the space, bounded by the crura and base of the stapes. The chorda tympani, also, in its passage across the tympanum, is enveloped by it. Lastly, it forms the inner borrowed layer of the membrana tympani, covering and adhering closely to the handle of the malleus." (*Cyclopædia*, p. 549.)

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* Lines, French measure, are very often mentioned, in English books, with the most vague signification; and many authors seem to prefer making their calculations in French lines and half lines, without, however, any clear notion of the length of a line, to using tenths and twentieths of an English inch. Mr. Ross, the optician, in Regent street, makes a scale of lines and half lines, French measure, and of tenths and twentieths of an inch English measure. We find the scale very useful, and would recommend it to our friends.—REV.

Ruysch was the first who unequivocally demonstrated, by his injections, that the ossicles are invested by a periosteum. Mr. Pilcher, speaking of the ligaments of the tympanic ossicles, remarks,

> "H. Cloquet says the ossicles are unfurnished with ligaments, and are held together merely by the common mucous membrane of the tympanum, which he seems to believe also constitutes the only periosteum they possess. . . . . The anatomists of this country, on the contrary, have usually described ligaments; and there can exist but little doubt that some of the folds of the membrane are so strengthened as to merit this distinction; of these particularly may be mentioned the *triangular ligament*, occupying the space between the crura and base of the stapes, and the *ligament* of the short crus of the incus passing to the edge of the mastoid cells." (*Pilcher*, p. 55.)

"So delicate and thin," continues Mr. P., "are the other ligamentous attachments, that a doubt may be justified as to the existence of more than mucous membrane." Now, we are of opinion, that doubt is not justified, for surely our knowledge of tissues is now sufficiently far advanced to enable us to distinguish ligament from mucous membrane. "There are, however," adds Mr. P., "usually enumerated, 1, a ligament fixing the manubrium and short process of the malleus to the membrana tympani; it is very certain this process of bone is placed between the mucous and proper membranes, and its union appears also to be strengthened by cellular tissue." There is no ligament fixing the handle of the malleus to the membrana tympani. It is the fibres of the proper layer of the membrana tympani itself that are inserted into the handle and short process of the malleus, as we have already seen.

Before leaving this part of our subject, we must here notice certain circumstances relating to the accessory parts of the apparatus of hearing; and in doing so, we shall avail ourselves of the accurate description of Mr. Wharton Jones.

> "In some animals, such as the mole, the squirrel, the guinea-pig, the marmot, &c., there is an osseous canal, like a bar of bone, extending over the vestibular fenestra, and running through between the crura of the stapes. This was first observed by Sir Anthony Carlisle in the marmot and guinea-pig, who describes it as 'an osseous bolt to rivet it (the stapes) to its situation.' The canal is for the passage of an artery and nerve, which in some other animals are unprovided with an osseous canal in their course through the stapes. The artery running through the stapes was observed about ten years ago by Professor Otto, in hybernating animals; but Professor Hyrtl, of Prague, has shown that the artery is by no means peculiar to those animals, as it does not occur in all, and as it occurs in animals which do not hybernate.
>
> "Mr. Shrapnell describes in the human ear an artery, accompanied by a nerve passing through the membrane, which fills up the space between the arms of the stapes. Mr. Shrapnell was led to this observation from what he had seen in the rat, viz., a nerve and artery passing through the stapes, and supported by a minute channel of bone. Professor Hyrtl has more recently described three modes of distribution of the arteries in man, which he has met with, analogous to the artery running through the stapes in the animals above mentioned." (*Cyclopædia*, p. 556.)

Two or three years ago, Hyrtl published (in Müller's Archives; if we remember aright) a short notice of the artery running through the canal of bone, and thought he had been the first to notice it. Professor Otto, however, as is seen from the above extract, had described it ten years before; and our own countryman, Mr. Shrapnell, had described the substance of what Hyrtl, several years subsequently, brought forward in the Oesterreichische Jahrbücher. These papers of Mr. Shrapnell, in the Medical Gazette, (vol. x., 1832,) though defective in several respects, contain some very ingenious observations, and do not appear, hitherto, to have been so well known and appreciated as they deserve.

The Labyrinth now falling to be considered, we take up Breschet's work, and shall follow the order of it in our remarks. We had written a very full abstract of this part of Breschet's volume, but now find we have not room for more than notices of points prominent either in regard to their novelty or importance.

Breschet only notices slightly the osseous labyrinth. It is the membraneous labyrinth he dwells most on, and in doing so we think he has done service, by raising to a more prominent position a part of the ear, which, to judge from the way it is usually handled in our anatomical books, one would suppose to be, instead of the most important, as it really is, but a very unessential part of the organ of hearing.

Osseous labyrinth. The osseous labyrinth in man and the higher animals consists of three parts: the vestibule, semicircular canals, and cochlea; all of which cavities communicate. To the aggregate of them Breschet gives the name of *labyrinthic cavity*. The labyrinthic cavity is lined by a delicate membrane of a serous or fibro-serous nature. The membraneous labyrinth is not to be confounded with this pellicle. By a particular arrangement the lining membrane of the labyrinthic cavity completes the spiral septum of the cochlea, and contributes to form the membranes closing the vestibular and cochlear fenestræ. A very important point to be remembered in the anatomy of the labyrinth, and which is insisted on by Breschet, but particularly by Wharton Jones, is that the cochlea belongs to the system of the osseous labyrinth; that it is a compartment of the labyrinthic cavity; that it contains nothing of the membraneous labyrinth, only liquid of Cotugno.

"The cochlea," says Mr. Pilcher, in his General Observations, "is very frequently wanting, the canals (semicircular) being then the only part of the second division added to the vestibule, &c." And again, "In structure the cochlea corresponds to the rest of the labyrinth, in being a cartilaginous or bony canal, lined by a vascular membrane, which contains the liquor cotunni; but it is not usual to find a second membraneous tube within this fluid, the nerve being expanded upon the vascular lining." (p. 16.)

This does not exhibit a correct general view of the nature of the cochlea. The cochlea is, in fact, an appendage of the osseous or cartilaginous labyrinth, and has nothing in common with the membraneous labyrinth. The osseous labyrinth may be more or less perfect; the cochlea may exist or not, and yet the membraneous labyrinth (comprising the membraneous vestibular sacs, ampullæ, and semicircular tubes,) be even very highly developed, as in fishes. The lining vascular membrane of the cochlea is the same as the lining pellicle of the osseous or cartilaginous vestibule, but has nothing to do with the membraneous labyrinth. The nerve of the cochlea is expanded *outside* the vascular lining membrane. The membraneous labyrinth being situated inside the vascular lining pellicle of the bony or cartilaginous vestibule and semicircular canals, of course the vestibular nerve pierces that pellicle to arrive at the membraneous labyrinth.

It thus appears that a distinction ought to be kept in view between the labyrinthic cavity and the membraneous labyrinth, as without this there can be no correct general conception of the internal ear, and we are constantly liable to run into error when we attempt to estimate the nature of the parts of the ear of different animals. An example of this error is found in Mr. Pilcher's work, (p. 20), where he says, "As we examine the apparatus in the advancing series, we find, probably, a rudimentary cochlea in the sacculus of fish." On the contrary, the sacculus vestibuli of fish belongs to the system of the membraneous labyrinth, whilst the cochlea, when it does exist, forms a part of the system of the cartilaginous or bony labyrinth.

A point in the cochlea worthy of notice is the hole by which the two scalæ communicate at the apex; this hole Breschet calls *helicotrema*. The circumference of it is formed, two thirds by the free concave margin of the hamulus of the bony spiral lamina, the rest by the membraneous zone of the spiral lamina. In the recent state it is through the helicotrema only that the tympanic scala communicates with the rest of the labyrinthic cavity.

The aqueducts are appendages of the osseous labyrinth. Much difference of opinion exists in regard to them.

There are two humours contained in the labyrinth: one which belongs to the cavity of the osseous labyrinth, and which appears to be the secretion of its lining membrane; another, contained within the membraneous labyrinth. The former, commonly known by the name of liquid of Cotugno, is called by De Blainville and Breschet *perilymph*. The latter, Breschet calls *endolymph*. It is an old notion, and one which, in our time, has found for defenders Brugnone and Ribes, that there is an *aer ingenitus* in the labyrinth. "On this point," says Breschet, "the observations of P. F. Meckel appear to us quite conclusive, as to the non-existence of air in the labyrinth; and they are in perfect accordance with our own observations and those of Cotugno." In 1825, however, when, in conjunction with Jourdan, he published the translation of J. F. Meckel's Anatomy, Breschet appears, from a note in vol. iii. p. 189, not to have adopted the opinions of P. F. Meckel and Cotugno, but to have agreed with Ribes.

The liquid of the osseous labyrinth, or perilymph, occupies, in the vestibule and semicircular canals, all the space not taken up by the middle sinus, the saccule, and semicircular tubes. It fills, moreover, the cochlea, and as all these parts communicate, it is the same humour in each.

Some anatomists, in taking for the prototype of their descriptions the organ of hearing in fishes, have fallen into an error, inasmuch as not considering that in most fishes the membraneous labyrinth constitutes almost exclusively the auditory apparatus, and that what of the labyrinthic cavity does exist communicates freely with the cranial cavity, they have confounded the liquid of the membraneous labyrinth with that of the osseous labyrinth in the higher animals. "It is the encephalic liquid itself," Breschet justly remarks, "which in the greater number of fishes we must compare with the perilymph."

"Cotugno and Meckel," says Wharton Jones, "supposed that the aqueducts were a sort of diverticula, or cavities which served to let off the superabundant perilymph, when necessary, during the act of hearing. This opinion is, however, now-a-days, very much questioned; and several anatomists, Brugnone, Ribes, Breschet, &c. refuse to those aqueducts the uses which Cotugno assigned them, and consider them merely as canals destined for the passage of blood-vessels. Although they may be insignificant in a physiological point of view, still, if the description I have given of them be correct, they must be considered as something more than mere canals for the transmission of vessels. The constancy of the aqueducts, moreover, is another argument against their being mere vascular canals.

"Breschet (and, in Hildebrandt's *Anatomie*, by Weber, the same idea is concisely expressed,) explains the mode of formation of the aqueducts, by supposing that at first the labyrinthic cavity is nothing but a sac formed by a prolongation of the dura mater in the same way as the tunica vaginalis is of the peritoneum; that, as development proceeds, the tube of communication between the labyrinthic sac of the dura mater and general cavity of the dura mater is gradually contracted and elongated; and that, as ossification extends, the tube becomes surrounded by osseous substance, and presents itself under the appearance of an aqueduct.

"This view (says Breschet,) is rendered probable; for, in many fishes, the labyrinthic cavity forms one with that of the cranium. And if, in these animals, a prolongation of the walls of the cranium tended to separate the brain from the ear, there would result a small canal establishing a communication between the two cavities, and this canal would be nothing but an aqueduct." "According to this view," continues Wharton Jones, "the lining membrane of the labyrinthic cavity may be considered as a continuation of the arachnoideal layer of the dura mater, perhaps of the dura mater also." (*Cyclopædia*, p. 536.)

Membraneous labyrinth. As a type of the soft parts of the internal ear, Breschet adopts the membraneous labyrinth of the frog-fish (lophius piscatorius), in which all the component parts exist in a high degree of development. The membraneous labyrinth of fishes resembles, fundamentally, that of the mammifera. The component parts of the membraneous labyrinth are the common sinus, the membraneous ampullæ, with the semicircular tubes, and the sacculus. These constitute an apparatus, the counterpart of the vestibule, ampullary sinuses, and semicircular canals of the osseous labyrinth, in which they are contained. No part of the apparatus of the membraneous labyrinth is contained in the cochlea. The common sinus and saccule are fixed to the inner wall of the vestibule by the nervous filaments which they receive. Towards the outer wall of the vestibular compartment of the labyrinthic cavity, they are not in contact with the base of the stapes, but are separated from it by the intervention of the perilymph. This circumstance, first distinctly pointed out by Scarpa, is very particularly insisted on by Breschet, to show that it is only by the intermedium of the perilymph that the movements of the stapes can have any impression on the nervous expansion of the membraneous labyrinth.

It will be in the remembrance of our readers that we have already referred, in the article in our Twelfth Number, to the minute structure of the auditory nerve, and its expansions in the membraneous labyrinth. We shall not, therefore, recur to the subject.

"In the sheep, hare, rabbit, &c. the walls of the membraneous labyrinth present patches of black pigment, a circumstance noticed by Scarpa, Comparetti, and

Breschet. Before I knew of the observations of these anatomists I had myself observed the fact. I was not, however, led to the discovery of it by accident, but, being engaged in researches on the pigment of the eye, and considering the analogy which the organs of sense bear to each other in their general anatomical structure, I was curious to know whether pigment did not exist also in the ear. Examination proved to me that it did; for I found, as Scarpa and Comparetti had previously noticed, pigment deposited in the form of small black spots, in the membraneous parts of the labyrinth in different mammifera. In some I have found a distinct cellulo-vascular layer of a black or brown colour, forming the outer surface of the membraneous labyrinth. And, contrary to what Breschet asserts, I have found pigment in the membraneous labyrinth of the human ear also. It appears, especially on the ampullæ, under the form of a slight but perfectly distinct brown tinge, similar to what is seen around the ciliary processes in the eyes of albinoes."

(*Wharton Jones*, *Cyclopædia*, p. 538.)

We have noticed the humour of the osseous labyrinth. The membraneous labyrinth contains, also, a limpid humour, which Breschet calls *endolymph*, in contradistinction to *perilymph*. The humour of the membraneous labyrinth was first distinctly pointed out by Scarpa.

Contained in the endolymph are small masses of calcareous matter: in some animals solid, in others pulverulent. In the mammifera the chalky matter is in small quantity, and pulverulent; in fishes (with the exception of the chondropterygenous, with fixed gills,) it presents itself in the form of solid, and sometimes large masses.

"These calcareous masses," says Wharton Jones, "are best known in osseous fishes, in which they are hard but brittle bodies of a determinate shape. In these animals, indeed, they have been erroneously considered as analogous to the tympanic ossicles of the higher vertebrata. MM. Breschet and Huschke have lately called particular attention to the subject, and have described masses of calcareous matter in the ear of reptiles, birds, and mammifera. Scarpa and Comparetti had observed them in the human ear, without, however, detecting their nature. But they had been unequivocally noticed before by De Blainville; and, previously to the first publication of Breschet's papers on the ear, in the 'Annales des Sciences Naturelles,' I had also studied them throughout the animal series." *Cyclopædia*, p. 539.)

" For the sake of greater precision and perspicuity in our descriptions," says M. Breschet, " we have given the name of Otoconies (from οὖς, ὠτὸς, *auris*, and κόνις, κόνιος, *pulvis*,) to the concretions which are pulverulent; and the name of Otolites (from οὖς and λίθος, *lapis*,) to the solid stony concretions."

The following extract from Wharton Jones, is a summary of all that is known of importance regarding the calcareous matter of the ear.

"In the ear of man and the mammifera in general, there are two masses of calcareous matter; one in the common sinus and the other in the saccule. According to Huschke and Barruel, they are composed of mucus, carbonate and phosphate of lime, and some animal matter. They are said to be more distinct in the fœtus than in the adult. From my own observations, I should say that they exist in the human adult as distinctly as in the fœtus. Concretions are never found in the ampullæ, or semicircular tubes, either in man or any of the lower animals. Examined in man and the mammifera, the concretions are suspended in the endolymph, and correspond to the points of the common sinus and saccule, where the nervous filaments are implanted. The grains composing the calcareous mass are held together by a soft mucous tissue. Huschke describes the grains as crystalline, small six-sided columns, pointed at the ends, with three surfaces; they appear to me, under the microscope, to have an oval form, more or less elongated, in man and the mammifera, passing into a spindle shape in birds and reptiles; and, though transparent, they do not present any very decided crystalline form. . . . . . .

"The grains of the ear are of different sizes. Of a mass which I removed from the ear of a middle-aged man, the greatest number had their longest diameter equal to that of the globules of the human blood, that is, about the three-thousandth part of an inch.

"There is found in the cochlea of birds a mass of calcareous matter; Breschet says he has found, in cochleæ of the human fœtus, which had been dried but not macerated, small masses of cretaceous matter, deposited near the summit of the cochlea; and Huschke once found, in the fluid of the cochlea of a child, a collection of microscopical crystals." (*Cyclopædia*, p. 539.)

On turning to Mr. Pilcher's work, the most recent of all those at the head of this article, to see if he had adduced any additional information on the subject, we were surprised to read at p. 70, that Mr. P. has not been able to see the *otoconies* at all, but he *grants* that, "as they are so generally met with in the lower animals, *yet most probably not universally in mammalia*, it is possible that *something of the kind* may exist, even in the human subject; and it is certain that a whitish matter is often found upon the surface of the membrane." Again, at p. 146, Mr. P. says, "Itard has described a calcareous body in the vestibule, as a mal-deposit producing deafness. It is possible that this appearance may be the otolithe of Breschet, although we have doubted its existence in the human ear. If it should occur as a malformation, it affords another example of the occasional approximation in peculiar structures of man to those of the lower animals." Allowing that Mr. Pilcher never succeeded in demonstrating, to his own satisfaction, the existence of the *otoconies*, we own we are surprised to find him stating his opinion thus strongly, after what has been written by Breschet, Huschke, and Wharton Jones.

II. PHYSIOLOGY OF THE EAR.

DR. LINCKE gives an historical sketch of the progress of the physiology of the ear in like manner as he did of its anatomy. "The new ideas and remarks," he says, "incidentally made by Berengarius of Carpi, Vesalius, Ingrassias, Colombus, Fallopius, and Eustachius, on the function of the parts discovered by them in the ear, are not, indeed, without interest, but are in general so unimportant and unsatisfactory, that it is not worth the pains to notice them in detail." (p. 265.) Volcher Koyter was the first who attempted, by collecting and arranging all that was known on the subject, to give a complete explanation of the mechanism of hearing; and this he did in a way pretty satisfactory, considering the time. He left his predecessors and contemporaries far behind; and though his notions regarding the proper act of hearing be very defective, still his explanation of the function of the accessory parts of the ear is, for the most part, what is still admitted as by many physiologists correct. Fabricius ab Aquapendente and Casserius introduced some modifications into the views of Koyter; and most of the contemporaries, and many of the successors of these three men, either wholly coincided in their opinions or differed but little from them. Among those who, in the last half of the seventeenth century, strove to enlarge our knowledge of the physiology of hearing, Molinetto, Willis, Schelhammer, and Duverney, deserve to be named.

A peculiar subtile, innate air, *aer ingenitus*, supposed to be contained in the ear, played a most important part in all the explanations of hearing, given by the earlier anatomists and physiologists, Democritus, Diogenes, Appolonia, Aristotle, Koyter, &c. The existence of this *aer ingenitus* had been already called in question by Bauhin, but it is to Schelhammer that science is indebted for having first forcibly combated the opinion.

Duverney, in the second part of his treatise on the ear, discussed what was known of the physiology of hearing with accuracy and perspicuity, and made several new observations of his own in regard to the membrana tympani and labyrinth. It was not, however, until 1757 that anything like a correct physiology of hearing existed. The author of the new light shed on the subject was Dominico Cotugno. He gave his voice in favour of the correctness of Schelhammer's assertions as to the non-existence of an *aer ingenitus*, and established incontrovertibly the presence of a liquid in the labyrinth. Viewing it as performing some office in the exercise of hearing, he laid the foundation for a new theory of that function—a theory which, with some modifications, has been generally adopted by all physiologists since.

The study of the physiology of hearing necessarily embraces the further history of the subject. It must be confessed that, notwithstanding all that has been done, we do not yet satisfactorily comprehend the whole mechanism of the act of hearing.

> "As the apparatus of vision," says Wharton Jones, "naturally admits of being divided into two parts, viz., the eyeball and its appendages, so we can distinguish in the apparatus of hearing a fundamental organ, and parts accessory to the perfect performance of its function. The fundamental organ of hearing is what is commonly called the internal ear, or, from the complexity of its structure, the *labyrinth*. The accessory organs consist of the middle ear or tympanum and external ear." (p. 529.)

One part of the eyeball transmits and concentrates the rays of light, which, falling on the retina, produce an impression, which is propagated along the optic nerve to the brain. It is the mind that perceives the impression and draws inferences from it. We are not to expect to find in the labyrinth an organ of sense doing more in regard to sound than the eyeball does in regard to its object. In short, we must not expect to perform with the eye or the ear what is evidently the function of the brain. "The eye of a painter" and "the ear of a musician" are merely figurative expressions. It is the mind that perceives colour and proportion in the one, and harmony of sound in the other. If this be the case, then it is evident that there is as little use for a musical part in the organ of hearing as there is for a painting part in the eye. To distinguish clearly what share the mind has and what share the ear, in the act of hearing, must be a great means of arriving at accurate notions on the subject.

Use of the external ear and auditory passage. Some have asserted that the external ear is of no use in hearing; others again endeavour to make it out to be an almost indispensable part of the ear. As in many other cases, truth will be found between these two extreme opinions. The external ear collects the undulations of sound, and reflects them into the auditory passage, by which they are transmitted to the middle ear. As the auditory passage first becomes narrower towards the middle, and then increases in width to the membrana tympani, sound will first be concentrated and then moderated in its progress. The external ear and auditory passage are, in fact, as Dr. Todd observes, a hearing trumpet. Savart's experiments to ascertain how far the external ear and auditory canal serve to increase the vibrations of the tympanic membrane, though good, have this defect, that a conical tube was used instead of one contracted in the middle, and dilated at the extremities, as the auditory passage is.

Does the external ear assist us in judging of the direction of sound, as Treviranus supposes? It can only do so indirectly, by receiving, (when the head has been properly directed,) and then conveying to the internal ear the sound, and all the different modifications produced on it in its passage through the interposed medium or media; then, according to the impression on the nervous expansion of the membraneous labyrinth, will the mind, assisted by past experience, judge of the direction of the sound. Ventriloquism, by imitating the peculiarities which characterize sound, issuing from this or that direction or place, produces its illusive effects.

The ear-wax appears to be of use in hearing only by maintaining the lining integument of the auditory passage in its proper organic condition. The secretion of the Meibomian glands, &c. does not actually make the eyeball see better, but it renders the eyelids more capable of all those minute and delicate actions which assist in perfecting the act of vision.

Use of the membrana tympani. "Besides the undeniable use of the membrana tympani, in protecting the middle ear against external injury, it has the still more important one of being thrown into vibration by the undulations of sound coming along the auditory passage, and thus contributes to the finer tones. . . . . Cases of perforation or destruction of the membrana tympani, only prove that the membrane is not indispensably necessary for hearing; but that in persons in whom it is either partially or wholly destroyed by ulceration, fine and distinct hearing undergoes a modification, and admits of no comparison with the ordinary hearing power. (*Lincke*, p. 454.)

In consequence of the connexion of the membrana tympani with one end of the chain of ossicles, the tension of it is capable of being varied by the action of the muscles of the ossicles. The membrane of the fenestra ovalis being connected with the other end of the chain, the tension of it is varied at the same time. Hence, there must be some relation between the state of tension of the soft parts within the labyrinth, and that of the membrana tympani. According to the state of tension of the membrana tympani, so will the vibrations of sound be communicated to the labyrinth. Professor Müller is of opinion that increased tension of the membrana tympani causes dulness of hearing. "Not unfrequently," says he (p. 438), "deaf persons have lost the power of hearing the graver tones only, whilst they retain the faculty of perceiving acute though low tones. A deaf colleague of mine hears acute tones better than grave. In such a case we may, with great probability, suspect too great tension of the membrana tympani. This circumstance may be of important use in the obscure diagnosis of diseases of the ear." We would hint to Professor Müller to pay a visit to his townsman, Dr. Kramer, who, we are persuaded, would make the Professor acquainted with phenomena, of which he appears at present not to have the slightest idea. We do not mean to deny that tension of the membrana tympani has great influence over the power of hearing, but we assert, that any increased tension that may occur, can never be the sole or even principal cause of a great degree of deafness.

Mode of propagation of sound to the labyrinth. Through what medium do the undulations of sound pass from the membrana tympani? In regard to this question, three different views are entertained by physiologists: 1. Some assert that the vibrations of the membrana tympani, excited by sound, are communicated to the ossicles, and through them conducted to the vestibular fenestra. 2. Others suppose the vibrations to be propagated merely through the air in the cavity of the tympanum to the membrane of the fenestra rotunda. 3. Others again admit a combination of both views.

The opinion that the vibrations of the membrana tympani are communicated through the ossicles to the vestibular fenestra is the oldest. First broached by Koyter, as Dr. Lincke informs us, it was objected to by Fabricius ab Aquapendente on the ground that the ossicles do not form a continuous whole. On the same grounds, Treviranus doubted the propagation of sound through the ossicles. And whatever Treviranus asserts, on a subject like this, must command our highest consideration, as he was what few have been or are, at once, an anatomist, a physiologist, and a mathematician.

The second opinion, that the vibrations of the membrana tympani pass through the air in the cavity of the tympanum, and affect the labyrinth by acting on the membrane of the fenestra rotunda, was first enounced by Schelhammer. It was maintained, as we have seen, by Treviranus. Scarpa supposed sound to pass through the fenestra rotunda, but he also admitted that it was propagated by the ossicles to the labyrinth; he must, therefore, be classed among the supporters of the third opinion.

Those who maintain this second opinion limit the use of the ossicles to the putting of the membrana tympani, as also the membrane of the vestibular fenestra, and indirectly the membrane of the fenestra rotunda, together with the membraneous labyrinth, into different states of tension.

The third opinion is the most generally received. A great partiality seems to be entertained for the view that sound is propagated through the chain of small bones. But driven from this exclusive view, by the fact that the membrana tympani may be more or less destroyed, and the malleus and incus lost, without absolute deafness being the consequence, physiologists have taken refuge in the middle view, that sound is conveyed to the labyrinth both by the air of the tympanum and the ossicles.

It is always to be kept in remembrance, that the base of the stapes forms part of the walls of the osseous labyrinth, and that with the loss of it the whole labyrinth would become disorganized by the evacuation of the perilymph, and complete deafness, therefore, would be the consequence.

The persistence of some degree of hearing along with loss, partial or complete, of the membrana tympani, and of the malleus and incus, is an *experimentum crucis*, that sound may be conveyed to the labyrinth by the vibrations of the air alone. On the other hand, an impediment to the free circulation of air in the middle ear, from muculent obstruction, produces often a greater degree of deafness than might have been expected, had sound been propagated principally through the chain of ossicles. The question that remains, therefore, is this: Is the deterioration of hearing, consequent to the loss of the membrana tympani, together with the malleus and incus owing to their no longer conveying vibrations to the labyrinth? Or is it owing to the following circumstances singly or conjointly?—to the air in an injured tympanum, not being in the same condition for propagating sound, as it is when contained in an entire tympanum?—or owing to the organic change in the membrane lining the tympanum, and consequently, that part of it closing the fenestra rotunda and fenestra ovalis, effected by the disease which produced the destruction of the membrana tympani, or by the subsequent exposure?—or owing to the contents of the labyrinth and membrane of the fenestra rotunda being no longer put into the proper degree of tension, which it is admitted to be one office of the ossicles and their muscles to do?

Before leaving this part of our subject, we would touch upon one other point. At p. 576, Dr. Todd says, "the muscular apparatus of the tympanic ossicles receives its nerves partly from the facial and partly from the otic ganglion, thus exhibiting an analogous arrangement to that of the muscular structure of the iris. Such an analogy renders it extremely probable, that the actions of the muscles of the ossicles are excited in a similar way to that in which the iris is prompted to act." At p. 108, Mr. Pilcher says, on the same subject: "The otic ganglion was imagined, by its discoverer, Arnold, to influence those muscles in a manner similar to that which the lenticular exerts upon the iris, and doubtless this is correct." The looseness of this supposed analogy between the membrana tympani, the internus mallei muscle, and otic ganglion on the one hand, and the iris and lenticular ganglion on the other, is exposed by the anatomical fact, that the tensor tympani muscle has been found by Müller, as well as ourselves, to present the same microscopical characters as other animal muscles, and must, therefore, be considered as voluntary. "This," says Müller, "is also confirmed by the origin of the nervus tensor tympani, from the third branch of the trigeminus, viz., from the nervus pterygoideus internus, and the origin of the nervus stapedius from the facial."

The membrana tympani, in as far as regards its anatomy, cannot for a moment be compared to the iris. We admit with Müller that it is probable that the tensor tympani contracts on the occurrence of a very loud sound, much in the same way that the iris and orbicularis palpebrarum contract, in consequence of the impression of a very strong light on the eye. We think that Mr. Wharton Jones, in his parallel between the ear and the eye, (*Cyclopædia of Anatomy and Physiology*, part xv. p. 562,) has determined the real analogies which subsist anatomically between the different parts of the eye and ear. These analogies are boldly but not loosely drawn; and we think we can discover in them views, which taken in combination with other data, and especially those obtained from the observation of the phenomena attending the employment of injections of air into the tympanum, may yet lead to a more correct physiology of hearing; and we cannot but think, that had Professor Müller taken advantage of the experience and labours of Dr. Kramer, his illustrations of the physiology of the ear, drawn from pathology, would have been more to the point.

Use of the Eustachian tube. There are several opinions in regard to this point.

1. "One of the oldest opinions," says Lincke, (p. 484,) "as to the use of the Eustachian tube, is that it serves as an excretory duct for the fluid secreted by the mucous membrane of the cavity of the tympanum, seeing, that when this is not carried off, there takes place, in consequence of the increased accumulation, gradual diminution of hearing, or even deafness." This is certainly one use of the Eustachian tube, and a very important one in a practical point of view. Müller thinks that it is by the motions of the vibratile cilia of the mucous surface that the mucus is carried off.

2. Another function of the Eustachian tube, admitted by many excellent physiologists, is that it serves to give vent to some portion of the air in the cavity of the tympanum, when compressed by the tension of the membrana tympani, supposed to take place during a very loud sound.

3. It is agreed that the principal function of the Eustachian tube is to admit the entrance of air, raised by its passage through the nostrils, to the temperature of the body, into the tympanic cavity, so that an equable pressure may be kept up, and the membrana tympani, as well as the air contained in the cavity, (which air must be looked upon as an indispensable component part of the apparatus of hearing,) thus kept in a state to vibrate in the due degree. Müller says that obstruction of the Eustachian tube always causes dulness of hearing and buzzing in the ears. In this assertion, Müller is mistaken; buzzing in the ear is not always an accompaniment of obstruction of the Eustachian tube; and as to dulness of hearing, we have met with a case to show that the power of hearing a watch, at the distance of fifteen feet, is compatible with obstructed Eustachian tubes.

4. It has been supposed, but with no good reason, that the undulations of sound are propagated, not only through the external auditory passage, but also through the Eustachian tube, into the cavity of the tympanum. With no better reason it has been said, that the sound of one's own voice is carried through the Eustachian tube to the proper organ of hearing, whilst the external auditory passage served to convey every other sound.

Labyrinth. The labyrinth is the organ of hearing. As, on entering the eyeball, light has new media to traverse, so, on entering the labyrinth, new media present themselves for the transmission of the undulations of sound to the nervous expansion. The transmitting media of the eyeball have *shapes*, &c., adapted to the purpose they have to effect. We cannot but suppose that the transmitting media of the labyrinth and the passages which contain them are shaped &c. conformably to the laws regulating the transmission and concentration of sound.

Functions of the auditory nerve in the labyrinth. According to the experiments of Flourens on pigeons, the nervous substance expanded in the vestibule is the essential part of the organ of hearing, and all other parts may be removed without the sense being thereby completely annihilated.

In regard to these experiments, Dr. Todd remarks (p. 571), "One can scarcely imagine vivisections less likely to lead to useful results than those which involve the exposure of the deep-seated internal parts of the ear, a dissection which, even on the dead subject, demands no ordinary skill; nor can we refrain from expressing our opinion that, had M. Flourens never attempted these experiments, physiology would have been none the worse, and our respect for his humanity would have been all the greater." We do not look upon the results of Flourens' experiments of such inconsiderable value as is here expressed. It requires no great skill, nor any great disturbance of the parts to expose the labyrinth in pigeons. In regard to the inhumanity of the proceeding, the question resolves itself into this: Are vivisections allowable at all? While we answer, "only under circumstances, when the point to be determined appears to be, according to the calculation of probabilities, within our reach," we would say, that M. Flourens has not, in the case under consideration, exceeded the just limits.

As to the part which the semicircular canals play, many hypotheses have been broached, but nothing is really known.

Cochlea. Weber considers it probable that the undulations of sound, propagated through the bony substance of the skull to the organ of hearing, especially act on the nerves of the cochlea, and that, on the contrary, the undulations of sound, received from the external air, through the membrana tympani, especially act on the membraneous labyrinth. In regard to this opinion of Weber, Dr. Todd says, the following considerations favour these views:

> "It is an admitted fact in acoustics, that sounds are most perfectly conducted by substances of uniform elasticity, and that when propagated from air or water to a solid, or from a solid to air or water, they are conducted much less completely. Now, inasmuch as the cochlea may be regarded as part and parcel of the cranial bones, the sounds which are propagated by these bones, would reach the nervous expansion in that portion of the labyrinth, by the most direct route; whereas, to affect the remaining parts of the labyrinth, the sound must be conducted from the bone through the perilymph to the membraneous vestibule and semicircular canals. Moreover, when it is considered that the cochlear nerves are disposed in a radiated manner in the lamina spiralis, it will appear evident that the oscillations propagated to the petrous portion of the temporal bone, must exert a direct influence on the cochlear portion of the auditory nerve." (*Cyclopædia*, No. xv.)

All this reasoning is founded on the assumption, that the cochlear nerve is as much the nerve of hearing as the vestibular nerve. Comparative anatomy, and the experiments of Flourens, so much underrated by Dr. Todd, teach us that neither it nor the cochlea is essential to hearing. And we would humbly submit, for the consideration of physiologists, the following peculiarities of the cochlear nerve, and then ask (with Mr. Wharton Jones), whether the cochlear nerve is the same in function with the vestibular?

In the first place, the cochlear nerve appears to differ somewhat, in its microscopical structure, from the vestibular nerve; and in the second place, it is distributed in a stratum of the body quite different from the vestibular nerve—quite different from the retina, and, indeed, different from any other nerve of sense.

At p. 528, Dr. Lincke says: "If we consider more closely what has been said regarding the cochlea, we cannot be inclined to view it as an organ of particular tones, whether musical or articulate; and the less so, as the distinction of different objects is a purely psychical function, and, therefore, not to be looked for in the individual parts of an organ of sense. Hence, nothing further remains for us but to recognize (with Scarpa, Joh. Müller, Esser, and some others,) in the cochlea an apparatus, intended to present, in the smallest space, a large surface for the expansion of the nerve." This may be all true, but the question still remains, "what is the function of the cochlear nerve?" Moreover, the shape which the perilymph, contained in the cochlea, receives from the latter is not explained.

In considering the function of the waters of the labyrinth, it is essential to keep in view the shape they acquire from the cavities containing them.

We shall close this part of our subject with a few short analytical extracts from the physiological considerations with which M. Breschet follows up his description of the structure of the labyrinth.

"The membraneous walls of the semicircular tubes, of the middle sinus, and of the sac, held suspended between two liquids, are in the most favorable condition for receiving and transmitting the sonorous undulations; the sac, the middle sinus, and the semicircular tubes are composed of a tissue, of a nature between the membraneous tissue properly so called and cartilaginous substance. Hence, these parts are endowed with such elasticity and resistance, that the walls of the canals do not fall together when the liquid they contain is evacuated.

"The properties of tissue must be of a high degree of importance in the functions of these organs; for, on the degree of elasticity and rigidity of the walls of the semicircular tubes, of the sac and middle sinus, placed as they are in the midst of a fluid, must depend the degree of sensibility of the organ."

"If the description of the vestibule, of the two liquids, of the membraneous pouches, and of the nerves which terminate at them, has been well comprehended, we are naturally led to infer that the sonorous undulations can arrive at the expanded branches of the acoustic nerve only by the intermedium of liquid strata, and that in fact these nerves are placed between two distinct strata of liquid. The first liquid is situated between the osseous walls of the labyrinth covered by their periosteum, and the membraneous labyrinth, and in the cochlea. The second is contained in the semicircular tubes, the middle sinus, and the sac.

"The small disc of the stapes corresponding to the vestibular fenestra, instead of transmitting directly the sonorous undulations to the acoustic nervous filaments, which are expanded on the membrane forming the semicircular tubes, the middle sinus and the sac, transmits them only to the *perilymph.* The *endolymph* has not only for its function to concur in maintaining the membraneous walls of these tubes in the best condition for the reception and transmission of the sonorous vibrations, but it also holds in suspension lapilliform concretions, or a pulverulent matter, with which the extremities of the nerves correspond. . . . . I presume that the otolites and otoconies have for their use to communicate to the nervous extremities a more vivid and energetic impression than a simple liquid like the endolymph could do; for the vibrations of a solid body are much more sensible for their force and degree of intensity than those of a liquid body." (*Breschet.*)

III. DISEASES OF THE EAR.

The history of the progress of ear-medicine we have sketched on a former occasion. (*Br. and For. Med. Rev.* vol. III. p. 79.) Here we would make only a few supplementary remarks. In consequence of the deep and concealed situation of the principal parts of the organ of hearing, it is very difficult to investigate its diseases properly; indeed, it has been looked upon by most surgeons as a thing impossible; and they have, therefore, with few exceptions, hopelessly abstained from making any attempts to overcome the difficulties. It would have been a sad thing if the progress of medicine, which has opened up to us new modes of investigating the diseases of other organs equally concealed and deep seated as the ear, should have failed to shed some light on its pathology. Happily this is not entirely the case, although the amelioration has been slow and partial. Now, thanks to the labours of Itard, Deleau, and Kramer, we are enabled to explore the diseased states of the ear pretty successfully.

As, in treating diseases of the ear, our efforts are in too many cases only partially successful in restoring hearing, as there is little opportunity for a display of dexterity in operating, and as no brilliant results are obtained, like those from tying an artery for the cure of aneurism, cutting for the stone, or extracting a cataract, there has been little inducement to the ambitious surgeon to bestow even a passing glance on the subject. The consequence has been, that the practice in diseases of the ear has been left almost entirely in the hands of persons unprepared by education for the exercise of a scientific profession. Hence the name *aurist*, which has been employed to designate a person who professes to cure diseases of the ear, is in some degree synonymous with *quacksalver*, and that, perhaps, even more than the names *oculist*, *rupture-doctor*, *bone-setter*, and the like.

Regular medical men do not disdain to operate for hernia, reduce a luxation, and even couch a cataract, or lay open a fistula *in ano*; but it would appear that there is something so peculiarly repulsive about that unfortunate organ, the ear, as to render it unworthy of such favour at the hands of the profession. Almost all seem to shrink at the idea of having anything to do with it, lest they should be put down as *aurists*. They are right, if an attempt to treat the diseases of the ear must necessarily make them act the part of the quack; but very wrong if the practice of ear-medicine involve no such alternative. And wherefore, let it be asked, should it do so? The ear is capable of being studied anatomically, and why not also surgically and medically? If, by doing so, we shall not always be able to cure, we shall at least discover how far the interference of art is capable of being of service: and that in medical matters, we hold to be great knowledge. Is not the ear as noble an organ as the eye? And are these two admirable organs of sense of so little consequence to the moral, intellectual, and physical man, as to deserve less consideration from the regular surgeon than a leg or an arm?

It is worthy of remark that, in the schools of medicine, great pains are taken in giving instruction on the subjects of hernia and lithotomy; and it is a *sine qua non* with the young man, going in for examination, to be able to describe exactly the inguinal and crural canals, the position and connexions of the urethra and prostate gland, and, above all, the origin and course of the epigastric and pudic arteries; and all this in reference to operations which not one surgeon in twenty is ever called on to perform; whilst diseases of the eye and ear, which are always occurring in the daily routine of general practice, are passed over, in the course of general medical and surgical instruction, commonly with a very superficial and inadequate notice. Far be it from us to imply by this, that hernia, lithotomy, or any other great operation in surgery, should be less studied than it is; but surely, while the one is done, the other need not be left undone.

Having, in our notice of Dr. Kramer's work, already referred to, considered pretty fully the diseases of the ear in general, we shall, on this occasion, confine our attention to one part of the subject only, in order that we may be able to discuss it the more fully. We do this the rather, as our knowledge of the diseases of the ear is not yet so far advanced as to allow us to pronounce a general summary judgment on any work on the subject. All that can properly be done is to examine some one of the principal points in detail.

The point we mean to take up is the affections of the middle ear; these being, perhaps, when compared with the affections of other parts of the body, the most peculiar, both in their diagnosis and treatment; a knowledge of them, moreover, being of paramount importance, for a perfectly just comprehension of the other diseases of the organ.

As being indispensable for the diagnosis and treatment of the diseases of the middle ear directly, and indirectly as a means of diagnosis at least in many kinds of deafness, catheterism of the Eustachian tube, and the employment of injections of atmospherical air, will, of course, fall to be particularly considered. The books before us, which will serve as a text, are principally Pilcher's and Deleau's.

Under the head of diseases of the middle ear, Dr. Kramer admits:

1st. Inflammation of the mucous membrane of the middle ear: *a*, with accumulation of mucus; *b*, with stricture of the Eustachian tube; *c*, with obliteration of the Eustachian tube.

2dly. Inflammation of the cellular tissue and of the periosteum in the cavity of the tympanum or true otitis interna. This occurs in two forms; *a*, the acute, and *b*, the chronic.

Before going further, we beg the reader to take up Kramer's work, and read over what is said on the above subjects, as the proper understanding of them is of immense consequence in ear-medicine; and as the criticism and counter-criticism, into which we are about to enter, bear particularly on the point.

In the first place, then, the reader will remember that we have already remarked, in the anatomical section of this article, that the lining membrane of the cavity of the tympanum is fibro-mucous—at once a mucous membrane and periosteum. This membrane may be the seat of simple blenorrhœal inflammation, and it may also be the seat of a more violent inflammation, of what is called *otitis interna*. In the former case there is an undue secretion of mucus, which, accumulating, becomes a cause of deafness, by impeding the entrance of air, and obstructing the free movements of the parts contained in the cavity of the tympanum. In the latter case, the bony walls of the tympanum sooner or later participate in the inflammation and its consequences.

It may, perhaps, be allowable to speak of the first inflammation, as "inflammation of the lining membrane of the cavity of the tympanum in its capacity of mucous membrane;" and the second, as "inflammation of the same membrane in its capacity of periosteum." Kramer describes otitis interna as "inflammation of the cellular tissue and of the periosteum of the cavity of the tympanum;" but, as there is scarcely any cellular tissue, and as the periosteum is not distinct from the mucous membrane, ours is the more correct pathology. With these remarks, we beg to say that our experience leads us to agree generally with Kramer.

Not so Mr. Pilcher:

> "That acute inflammation of the tympanal cavity occurs, commencing ordinarily in its mucous membrane, and extending to the other structures, almost every day's experience convinces us. Dr. Kramer has arranged it under two heads, that of inflammation of the mucous membrane, with mucous accumulation, and that of the sub-mucous cellular tissue; which arrangement corresponds to Itard's *catarrhal and purulent internal otitis*. This division is with difficulty recognized in the majority of acute cases, and experience leads the author to justify the criticism of M. Itard's reviewer in the Edinburgh Journal." (*Pilcher*, p. 179.)

The criticism in the Edinburgh Medical and Surgical Journal, here alluded to by Mr. Pilcher, is the following:

> "Although our author has divided the internal otitis into catarrhal and purulent, we must acknowledge our inability to discover the difference, either from his account of the symptoms, or from the little which we have had occasion to know personally of the disease. We conceive it, indeed, essentially irrational to attempt to establish a distinction of this kind, in a morbid state of a part which must be the same in the beginning and through the course of the disease, and seems to differ only in the degree to which it may proceed. Our author, indeed, himself seems to overlook the difference, unless in the termination, and, if we understand him aright, makes one history of symptoms serve for both varieties." (*Ed. Journal*, vol. xix., p. 90.)

This criticism of the Edinburgh reviewer is correct, as regards the object it was written on; but, in applying it to Kramer, Mr. Pilcher loses sight of the fact that, concerning Itard's division, Kramer expresses himself to the following effect:*

> "Itard establishes two forms, *otite interne catarrhale* and *otite interne purulente*, quite correctly as to name, but incorrectly according to his description. Thus, according to Itard's account, the difference between the two lies only in the different degree of severity of the symptoms, but no difference in regard to internal character is stated, though such a one really exists between catarrhal and phlegmonous inflammation. Itard's *otite interne catarrhale* is said, in by far the greater number of cases, to end in perforation of the membrana tympani and evacuation of the matter in this way, because the Eustachian tube has become closed by the inflammation;—an altogether erroneous opinion, as in perforation of the membrane of the tympanum, which always takes place in consequence of inflammation of it, the Eustachian tube is generally found open, thus, the escape of the matter through it would not have been stopped, if an accumulation had really existed in the cavity of the tympanum. On the other hand, we have observed many very inveterate catarrhal inflammations of the middle ear, with actual obstruction of the Eustachian tube, without any of the violent symptoms attributed to them by Itard, and without destruction of the membrana tympani having ensued, even when the case had been treated with the greatest neglect. Lastly, Itard cannot make good his opinion, because, in the cases which he relates as examples of his *otite purulente interne*, he did not examine either the perforated membrana tympani or the cavity of the tympanum, which he pretends to have been the seat of the suppuration, or the Eustachian tube, the obstruction of which he presupposes."

From this extract we find that, substantially, Kramer confirms the justice of the strictures on Itard's division of otitis, contained in the Edinburgh Journal, but at the same time vindicates the correctness of his own admission of a distinct *catarrhal otitis*.

In regard to the doubts expressed by the reviewer as to a *catarrhal* or *blenorrhœal otitis*, distinct from a *purulent otitis*, it is to be remarked, that he does not appear to have employed catheterism and injection of the Eustachian tube, and could, therefore, know nothing practically of any such distinction; particularly, as in the former, the inflammatory symptoms are very seldom considerable, as, in fact, the purely inflammatory stage is not unfrequently overlooked. Hence, as Dr. Kramer says, the catarrhal otitis is in general indicated by no other subjective symptom but deafness; and it is only by means of catheterism and injection that its diagnosis can be established. We shall see that Deleau gives the same view of the matter.

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* We quote from the original, not having at hand Dr. Bennett's accurate English version of the work.—Rev.

The excuse we have made for the Edinburgh reviewer can hardly be extended to Mr. Pilcher, as he is accustomed to the employment of catheterism and injection of the Eustachian tube. He appears to confound Kramer's catarrhal otitis with Itard's, notwithstanding the care Kramer has taken to point out the difference.

Mr. Pilcher's description of true phlegmonous *otitis interna* is illustrated by some very valuable cases, chiefly "supplied by friends to the essayist." Mr. P. discusses, at one part of his work, acute otitis interna, and at another part the chronic otitis interna, not a very good plan, as the two forms of disease are not always independent of each other. This is seen from the cases themselves, for all those which Mr. P. relates, in illustration of acute otitis interna, are cases of the acute disease supervening on a chronic one. The best marked case of idiopathic acute *otitis interna*, in Mr. Pilcher's book, is given at p. 165, under the head of *otitis externa*. It is adduced as "a case which points out the great destruction in which the ear may be involved from surrounding disease." Mr. P. further remarks, "it appears that in this case an abscess originated in the cellular tissue, between the meatus and parotid gland, which, in its extension ulcerated into the auditory canal, reached the cavity of the tympanum, and eventually the brain, the pressure upon the nerves in the neighbourhood having interrupted their function."

We doubt this pathology; and think that if any one will read carefully the case he will come to the same conclusion as ourselves regarding it, viz., that it was an inflammation and abscess of the tympanum; that the abscess burst through the membrana tympani, and also pointed at the mastoid process; that the petrous bone and aqueduct of Fallopius, and, of course, the portio dura, became involved in the disease; and, lastly, that inflammation extended to the brain or its membranes.

We now come to consider some details in Mr. Pilcher's account of catheterism of the Eustachian tube.

> "Both authors, Itard and Kramer, propose to sound the tube, by means of a catgut run through the instrument; for which purpose, and particularly to inject fluids into the tympanum, it is necessary to fix the catheter by a frontal bandage, which is furnished with a pair of strong holders or forceps. The author, when he first practised this operation, was desirous to dilate a contracted tube, and, therefore, gave to his instrument a longer and more gradual curve; the bend near the handle allows it to rest more conveniently against the tip of the nose. This shape was determined upon, after trying a wire, bent in various degrees, upon a preparation of a perpendicular section of the head and face, when that depicted in the sketch, (plate xiv. fig. 4,) was found to enter the tube, and to run along nearly an inch of its extent with great ease. A graduated silver-wire stilette is also appended to it, which may be introduced beyond the catheter, if the surgeon should deem it advisable, or a catgut string

may be used, which, at the same time that it is safer, will answer the purpose as well; and the author has lately used stilettes of whalebone slightly enlarged at the point, which he finds much more convenient and useful than when made of other materials." (*Pilcher*, p. 304.)

"When used for the purpose of investigation, the wire may be carried further onwards, or, what will be much the safer, the catgut or whalebone sound may be passed through the catheter. If it should reach the tympanal cavity, it will give rise to pain, often severe, to a loud cracking noisy sensation, extending to the mastoid cells; these feelings will be more marked in proportion to the healthy state of the tympanum. The surgeon must be especially careful not to injure the ossicula, the avoidance of which will require great caution, passing, as they do, across the cavity; the stilette must, therefore, just reach the tympanum without entering it." (*Ib.* p. 306.)

It appears to us that Mr. Pilcher is rather free in passing sounds through the Eustachian tube into the tympanum. Cases in which it is really indicated are comparatively rare; and how little benefit is to be obtained from it, even in those cases, may be learned by referring to Kramer or Deleau. We hold it to be a very unnecessary and reprehensible proceeding to push a probe, whether it be a metal or a whalebone stilette or a catgut string, along the Eustachian tube into the tympanum, until a well-directed examination, by means of streams of air, directed from an air-press, along the Eustachian tube, has unequivocally proved the existence of a stricture. A sound can never afford the same information, regarding the state of the middle ear, as the air-douche.

"Deleau," continues Mr. Pilcher, "introduced the air-douche for the purpose of removing matter from the cavity, and also distending the contracted tube, which Kramer considers a great improvement upon the injecting of fluids. Both these intelligent aurists used an air-press, for the purpose of increasing and regulating the force employed. The author, however, daily experiencing the great facility with which air and fluids may be introduced, both as to quantity and force, by means of a common syringe, accurately fitted to the catheter, does not hesitate to declare his conviction, that the ceremony and inconvenience of the air-press may be dispensed with." (p. 307.)

We cannot but admire the boldness which dictated this, unsupported, as the assertion here is, by the history of a single case effectually treated, and gainsayed, as it is, by the experience of Deleau, Kramer, and, we would add, of ourselves.

"It is of course requisite," continues Mr. Pilcher, "that the operator should steady the instrument with his left hand, while using the syringe with his right." To say nothing of the pain unnecessarily inflicted on the patient by this means; we would ask how, in this way, does Mr. Pilcher listen to the sounds produced, supposing any be produced, by such a feeble stream of air? Does Mr. P. venture to inject air without listening to the sounds produced? If Mr. P. had ever used the air-douche properly, as a means of exploring the state of the middle ear, we are confident he would not have spoken so heroically of sounding with stilettes.

Mr. P., it is seen, says the syringe must be *accurately fitted to the catheter*. On this point we feel called upon to give a practical caution. Not that we dread much harm from the feeble stream of air, projected from Mr. P.'s *common syringe*, whilst the nozzle is "accurately fitted to the catheter," but lest any one, having an air-press, should suppose it necessary, as we have observed it to be the common impression, to push home the nozzle of the tube of the air-press into the dilated end of the catheter. The nozzle of the tube of the air-press should be held, during the operation, so loosely in the dilated end of the catheter, that there may be room for air to regurgitate. If the nozzle of the tube of the air-press ought not to be pushed home into the catheter, much less should the point of the catheter be pushed far up into the Eustachian tube. If it were so, and if a stream, necessary to overcome an obstruction, were injected, what would be the result if no room were left for regurgitation? We shall answer this question in our analysis of Deleau's work.

Obstruction of the Eustachian tube is not removed by the mere force of the air-douche, but by the continued battering of the stream breaking up the obstructing mucus, and exciting the mucous membrane to increased secretion, for the time by which the thickened mucus is softened, and thus disposed to be more readily dispersed at subsequent operations by the air-douche.

Dr. Deleau is distinguished for having introduced the injection of streams of atmospherical air through the Eustachian tube into the tympanum as a means of diagnosis and treatment in the diseases of the ear. We have, in a former Number, alluded to the claims which might be set up in favour of our countryman Cleland as the first proposer of injections of air into the middle ear. It is nevertheless but justice to concede that all that has been awarded to Deleau, whether in the way of honorary or more substantial testimonials, has been fully deserved by him. We, however, confine our eulogium to the simple circumstance of introducing the air-douche. We cannot join the reporters of the Institute in their unqualified approbation of other details in Deleau's practice, such as the preference he gives to gum-elastic catheters. We entirely concur with Dr. Kramer in his remarks on Dr. Deleau's frivolous objections to metallic catheters.

The first Report of the Institute on Deleau's labours is dated Dec. 19th, 1822. At this time Deleau had not thought of the air-douche; he merely employed catheterism of the Eustachian tube and liquid injections, and that in a manner not at all superior to what had been done before in this country; and yet the labours of Cleland, Douglas, and Wathen are very unjustly and incorrectly stated in the report. They are in fact stated in a way which shows that the reporters had not taken pains to inform themselves on the subject, but hastily took it upon them to give an opinion in a matter they were quite ignorant of. We, however, fully and heartily concur in the following judgment delivered by Savart, in the Report No. 4, dated 7th Dec. 1829:

"In short it appears to us that M. Deleau has done real service to the medical art by his ingenious invention of injections of air for the purposes of forming the diagnosis and prognosis of affections of the middle ear. We, therefore, consider his work deserving the approbation of the Academy."

Though Dr. Deleau's work abounds in verbiage, it contains many shrewd remarks, and shows that the author is a man of exquisite practical tact—a man who might have written a more generally useful book, if he had condescended to enter more into descriptive details. In regard to his cases, we would hint, that they are not related with all the definiteness and precision desirable.

The first chapter of the work contains a physiological enquiry into the part which the atmospherical air contained in the middle ear plays in the process of hearing. The author insists, correctly, that it forms a necessary component part of the apparatus of the sense.

> "Before employing," says Deleau, "in the diagnosis and treatment of the diseases of the ear, pure air at the temperature of an inhabited room, I had tried air charged with emollient or aromatic vapours: but whatever the qualities were which it acquired in traversing a reservoir of liquid, there almost always resulted an augmentation of the deafness; and it frequently happened that the medication which I endeavoured to apply to the affected organ, gave a result contrary to my expectations. The vapours which I judged sedative, and which are so for other organs, excited pains and often inflammation. Hence, experience has led me to the almost exclusive employment of douches of dry air. (*Deleau*, p. 46.)
>
> "After having studied the action of the air on the middle ear in the natural state, we proceed to examine what is the mode of action of the lesions of the organ on the presence and on the circulation of this fluid in the Eustachian tubes, the cavity of the tympanum, and the mastoid cells. The diseases of the pharynx which are communicated to the mouth of the Eustachian tube, and contract and alter it, shall first fix our attention. We shall then examine the numerous lesions of the middle of this canal: these will conduct us to the exploration of muculent accumulations in the cavity of the tympanum. Lastly, we shall see that the perforation of the membrana tympani is far from being advantageous to the circulation of air in the apparatus of hearing." (*Ib.* p. 62.)

All these are very important points in the study of the diseases of the ear; we therefore think it necessary to devote a page or two to their consideration.

At p. 44, Deleau says, "Surgeons occupied in treating cases of cleft palate must have met with many deaf persons among those afflicted with this congenital infirmity. No one has made mention of the fact." We would remind M. Deleau that Sir Astley Cooper relates in his paper on perforation of the membrana tympani, in the Philosophical Transactions for 1801, (vol. xci. p. 443,) the case of Mr. Round who had a congenital affection of the palate so that he could not blow his nose; this case by Sir A. Cooper is related incidentally; but Dr. Dieffenbach of Berlin has dwelt professedly on the fact in a paragraph in his Chirurgische Erfahrungen, dritte und vierte Sammlung, (Berlin, 1834, § 205, pp. 261, 262,) under the title "Ueber eine bis jetzt nicht erkannte Ursache der Taubheit."

Deleau attributes the deafness in such persons to a dryness and almost always a chronic inflammation of the mucous membrane of the throat, in consequence of its exposure to the contact of the cold atmospherical air, which chronic inflammation extends to the middle ear. Dieffenbach says, "I am of opinion that the cause of deafness in persons with cleft palate is a closure of the Eustachian tube. By the fissure of the palate, the lips of the guttural orifice of this passage are so much relaxed that they lie upon each other and hinder the free entrance of the air to the inner ear. . . . . Staphyloraphy always completely removed this. When the operation was successful there occurred in some cases at first a sensitive fineness of hearing, which afterwards gradually subsided."

Dieffenbach's explanation of the proximate cause of deafness occurring along with cleft palate may be in some degree applicable, but we are inclined to attribute the principal share to the causes mentioned by Deleau.

In regard to the influence of tumefaction and induration of the tonsils on hearing, Deleau remarks as follows:

> "The tonsils, when they become double, triple, or quadruple the usual size, by the action of the causes which give rise to inflammation of the throat, have not in general, by reason of their development, a prejudicial action on the mouth of the Eustachian tube, and consequently on the entrance and exit of air in the middle ear. I have met with tumefactions so great that they threatened the life of the individuals, without causing the slightest deafness. . . . . . It is rather when these glands are subject to pass into the state of acute inflammation, or when they are surrounded by a red and tumefied circle which invades the lateral walls of the pharynx, that hardness of hearing is perceived, or noises in the ear which the patients compare to the boiling of water, or to the rustling of leaves shaken by the wind. A condition still more serious which these glandular bodies take on, is their development from before backwards, so as to separate the pillars of the fauces. Hardness of hearing almost always accompanies flattened glands which tend rather to sink into the flesh than to project into the throat." (*Deleau*, p. 70.)

The following is Deleau's remarks on the inflammation of the lining membrane of the middle ear, to which we have in a preceding page referred.

1. As to accumulation of mucus, he says, "the mucous membrane, the seat of this too abundant secretion, rarely passes into the acute phlegmonous inflammation, called *catarrhe douloureux de l'oreille.* The only signs of this kind of lesion are drawn from the variations of the hearing, which are pretty frequent." (p. 76.)

2. On strictures of the Eustachian tube, our author thus expresses himself:

> "Strictures of the Eustachian tube, accompanied by sub-acute inflammation, are principally met with in individuals of a sanguineous temperament, subject to frequent attacks of otitis. Frequently, however, the patients do not remember having experienced the slightest pain; it is, then, that the inflammation is developed in an insensible manner, has passed into the inflammatory state, and has degenerated into stricture without the manifestation of any other symptoms than diminution of hearing; I have often seen such cases confounded with affections of the auditory nerves. . . . . It is true that they are appreciable only by means of catheterism and the air-douche. . . . The treatment by sounds ought to be seconded by the most active derivatives; frequently the success corresponds neither to the patience of the patients nor to the sagacity of the practitioner." (*Ib.* p. 78.)

3. *Chronic inflammation and muculent obstruction of the cavity of the tympanum.*

> "It may readily be conceived, without any great knowledge of the pathology of the ear, that the privation of atmospherical air, in those cases of inflammation of the tympanic walls, is rarely the only proximate cause of the deafness, for the tissues through which the sonorous undulations have to pass, have acquired a degree of sensibility so great, that the least sound is insupportable. In other cases, this vital property is almost annihilated, either in consequence of thickening of the membranes, or by the compression resulting from effusions into the tympanum." (*Ib.* p. 87.)

We might add, the mere change in the common physical properties of the membrane lining the cavity of the tympanum, and, consequently, of the membranes closing the fenestræ, appears to be a very likely proximate cause of deafness.

Diseases of the membrana tympani, in which the air-douche is required. Kramer says, on this subject, that in ulceration and perforation of the membrana tympani, the Eustachian tube is generally open.

This, however, will not be found always the case. But whether it is or no, it might be supposed indifferent, as the air enters the tympanum by the perforation in the membrane. The experience of Deleau, however, proves the contrary; for, in cases of the sort, he has found "immediately that the obstruction of the Eustachian tube was removed, and the air permeated freely the whole middle and external ear," the hearing distance was considerably raised.

Deleau's third chapter is entitled "*Of the insufficiency of former modes of diagnosis and treatment in diseases of the middle ear.*"

> "In the first chapter," he says, "I have studied the functions of the atmospherical air in its relations with the solid parts of the ear; I have demonstrated that it must, as well as they, possess certain qualities, which I have called physiological, else it injures the tissues with which it comes in contact, and it is little adapted to the exercise of hearing. Viewed under this aspect, in its relations with the ear, this fluid ought to be submitted, more than has been hitherto done, to the investigations not only of physiologists but also of anatomists. Do the latter not examine whether the labyrinthic canals are filled with a liquid or gaseous fluid? Do they not strive to guess the composition of the fluid? Would it not be useful for them to know whether it is transported from a reservoir to a canal, or from one canal to another, during the succussion which the whole organ receives from the sounds which surround us? Yes, truly: why then have they neglected to study the presence, the movements, the qualities of the air introduced into the middle ear?" (*Deleau*, p. 97.)

Our author goes on to say that in his second chapter he has considered the morbid states of the middle ear, which impede the free circulation of air; but he says his opportunities for making *post-mortem* examinations have not been so great as those of his "honorables confrères."

> "However," he continues, "I have succeeded in exploring this organ better than has been done before, by modifying the exploring instruments of my predecessors; . . . . and especially employing, as a principal exploratory means, a fluid, set in motion in the middle ear, indicating, by the sounds there developed, whether the passages be free, contracted, or obstructed; I have found in flexible instruments of extreme simplicity, in the address of a hand endowed with a delicate sense of touch; and in the observations made by my ear applied to that of the patient, wherewith to make up for the sight, which in all surgical operations is the most certain guide of the operator." (*Ib.* p. 99.)

The following observations, in reference to the peculiarities of the affections of particular organs and treatment on general principles alone, are important.

> "Could they not comprehend that the conformation alone of certain organs gives origin to lesions which cannot be met with in any other part? Could they forget that special functions are found impeded, annihilated by their products, or by the presence or absence of the bodies which put them into play? Ought a stone in the bladder, a flattened auditory canal, an Eustachian tube, which has lost its conformation from an inflammation, long ago subdued, be treated by setons and moxas? If you see in these maladies only retention of urine in the one case, and deafness in the other, leave the treatment of them to more experienced hands, or, at least, listen to and follow their advice. If vision could not be exercised for want of light, would you seek to apply your medicaments to the eyeball? If air is not introduced into the middle ear, how could you succeed in making the sound arrive at the labyrinth? Remove the obstacles to the introduction of this air, indispensable to hearing, and leave at rest the organic tissues when they are not diseased. What would you think of a practitioner who should insert a seton in the neck, or who should purge every two or three days, in order to remove an accumulation of cerumen in the auditory passage? Surely you would blame his conduct. Permit me, then, to do the same, every time you treat, in

a similar way, an obstruction, quite as palpable, in the Eustachian tube." (*Deleau*, p. 109.)

Diagnosis. "I now proceed to point out the effects of douches of air on the middle ear in the state of disease, and enumerate the sounds which they produce according as this or that part of the organ is affected. This simple exposition will serve as an introduction to the chapter dedicated to the diagnosis. The *bruit de pluie*, produced by a moderate douche, is sometimes accompanied or followed by a slight pain and momentary increase of the dulness of hearing. It indicates an exaltation of sensibility—it is a commencement of inflammation. This state can only be recognized and well appreciated when one is certain of having performed, without hesitation and fumbling, the operation of catheterism; for it is easy to suppose that an awkward hand might, by throwing in the air with too great force, give rise to this morbid irritation in the ear; in the same way that too strong light would over-irritate the retina predisposed to inflammation by a delicate operation." (*Ib.* p. 133.)

The condition of the ear, above described, in which Deleau's *rain-sound* occurs, it will be seen, is the same as Kramer's nervous deafness; of which more anon.

"Sometimes," continues Deleau, "the catheter is engaged in the tube some lines only; if we do not take precautions in withdrawing the stilette, the end of the catheter escapes into the pharynx. In this case the air which serves for the douche is dispersed in the pharynx; if the catheter remains in the Eustachian tube, the air regurgitates immediately after having arrived at the extremity of the catheter, and makes the lips of the guttural orifice of the Eustachian tube vibrate. I call the sound which is heard, and which is more or less mucous, *bruit du pavillon*. When it is simple, it indicates a stricture or a complete obstruction, situated in the guttural half of the tube. When the obstacle to the introduction of air into the tympanum is near that cavity, we perceive, on applying our ear to that of the patient, the *bruit de la trompe*. If the douche is given at the pressure of about one atmosphere, both the *bruit du pavillon* and *bruit de la trompe* will be heard simultaneously. The *bruit de la caisse* is more or less mucous; it is general or confined to one point of the tympanic wall. Little practice is required to appreciate the different characters of it." (*Ib.* p. 134.)

Besides the above sounds there are also those of the membrana tympani, which are named by our author *éclats* and *sifflements*.

In chapter vi., which is devoted to the diagnosis of the diseases of the middle ear, Deleau gives (p. 144) the following *Table of the known lesions of the middle ear, which occasion deafness.*

FIRST CLASS.

Affections of the guttural orifice of the Eustachian tube, from disease of the pharynx.	*First Order.* Chronic inflammation. *Second Order.* Tumefied and indurated tonsils.

SECOND CLASS.

Diseases of the Eustachian tube.	*First Order.* Simple obstruction.	
	Second Order. Chronic inflammation,	without secretion. with secretion.
	Third Order. Stricture situated in the	inner half. outer half.

THIRD CLASS.

Diseases of the cavity of the tympanum.	*First Order.* Inflammation without secretion.	
	Second Order. Muculent accumulation	from obstruction of the Eustachian tube. from increased secretion.

FOURTH CLASS.

Diseases of the membrana tympani.	*First Order.* Inflammation.
	Second Order. Perforation.

FIFTH CLASS.

Complications attending disease of the middle ear.	*First Order.* Combination of the above-mentioned affections.
	Second Order. Diseases of the middle and of the external ear.
	Third Order. Diseases of the middle ear and of the labyrinth, of the auditory nerves, and of the brain.

Prognosis. The subject of prognosis is a very important one in all diseases; but in the case of the eye and ear it is peculiarly so, because a correct prognosis is oftentimes of so much consequence to the worldly concerns of the patient; and the means of giving it correctly are, besides, so much within our reach. Deleau is so fully sensible of this that he commences his chapter on the prognosis of the diseases of the middle ear with the following remarks:

> "It is perhaps to the prognosis I give my patients that I owe a part of my practice. Many patients are sent to me by persons whom I had pronounced incurably deaf, but who are not the less ready to recommend me than are those whom I have cured. This is not surprising: for indeed it is as useful to humanity to prognosticate the impossibility of curing certain cases of deafness, and not to subject to treatment the numerous incurables, as to cure those who are curable. Consider how many wounds, bloodlettings, and operations still more painful, one should practise, if he subjected indiscriminately to treatment that immense number of individuals who are constantly running about to consult all the medico-chirurgical notabilities and all the inventors of new nostrums!" (*Deleau*, p. 172.)

M. Deleau's tenth chapter is entitled "Of the accidents sometimes caused by catheterism of the Eustachian tube, and injections of air into the middle ear, and of the relapses which are apt to take place."

The accidents which sometimes occur are: 1. Inflammation of the throat, and catarrh of the tympanum. 2. Emphysema. 3. Rupture of the membrana tympani.

Emphysema has occurred to Deleau six or seven times. Mr. Pilcher mentions that it once occurred to him, and we have heard of other two cases. Kramer says nothing about it; and, of the many times we have catheterized the Eustachian tube, and injected air from an air-press into the tympanum, we have never met with the accident. The emphysema must take place in consequence of laceration of the mucous membrane of the Eustachian tube or posterior nostrils, and will, therefore, be likely to occur when the catheter is either rudely introduced or forced too far into the tube.

Rupture of the membrana tympani is not to be wondered at if the catheter be pushed far up into the Eustachian tube, or the nozzle of the pipe of the air-press pushed home into the dilated end of the catheter, and the air-douche allowed to play with force.

Deleau says, when laceration of the membrana tympani takes place, it is exactly in the middle of its lower half; the opening is round, and is almost always a line in diameter. " It is to be wished," he continues, " that in many cases of deafness, we could produce at will such effects. The patient feels no pain; he does not even know what has happened to him—oftentimes he is agreeably surprised at hearing perfectly well." (p. 330.)

We should not be inclined to perform the operation of perforating the membrana tympani in this roundabout way; this reasoning of Deleau's is something like making a virtue of necessity. Our author continues, " To avoid this rupture during the douche, it is necessary to measure the force of the current, and listen at the ear, to be assured that the column of air returns into the throat with more or less ease." How this is to be accomplished, we have above pointed out, and remarked on the impropriety of pushing the catheter far into the Eustachian tube, so very far as Deleau pretends to do.

On the occasion of our review of Kramer's work, in our Third Volume, we remarked (p. 100), that Mr. Swan wishes to establish that, in a great proportion of habitually deaf people, the auditory nerves are not affected. We stated his opinion to be that, in many such cases, the deafness is owing to a thickened state of the lining membrane of the cavity of the tympanum, involving the small branches of the tympanine nerves; and we still think that this theory of Mr. Swan's accords very well with the good effects derived from Dr. Kramer's practice in nervous deafness; as it appears probable that the ethereal vapours may act directly on the morbid lining membrane.

Dr. Kramer objects to this, saying that it is erecting one hypothesis to support another. We reply that Dr. Kramer has no direct, and but very doubtful inferential evidence, to show that it is the auditory nerve which is affected in those cases which he puts down as nervous deafness. We, therefore, hold that many of his arguments are hypothetical—more so than Swan's, because the opinions of the latter are drawn from actual dissections. Perhaps, however, Swan infers too much from his observations.

In the state of our knowledge of the physiology and pathology of the ear, we can as readily conceive some other part of the organ to be at fault in many of the cases of so-called nervous deafness as the auditory nerves. The enormous proportion of 152 cases of nervous deafness out of 300 cases of diseases of the ear in general, according to Dr. Kramer's own table, staggers us, when we reflect on the comparative rarity of cases of amaurosis.

We think it of immense consequence to the farther progress of ear-medicine to leave the question open as to the condition of the ear in those cases commonly called nervous deafness. We do not deny that many are actually cases of disease of the auditory nerve or brain, but we have no doubt that many are also affections of other structures in the labyrinth, and that some are affections of the membrane lining the cavity of the tympanum, and, consequently, of the membrane closing the fenestra rotunda; and that contributing, with the base of the stapes, to close the fenestra ovalis.

Affection of the membrane lining the cavity of the tympanum may consist in thickening from sub-acute inflammation without increased secretion. Many of the cases related by Deleau as such, seem to be what Kramer would call nervous deafness.

In regard to nervous deafness, Deleau remarks (p. 296), "I cannot comprehend what is meant by paralysis of the auditory nerve, which may, it is said, occur in a child enjoying otherwise perfect health." If what we have above said have any reason in it, it shows the importance of keeping attention fixed on the investigation of this very difficult subject; and it also shows that the injection of ethereal vapours into the tympanum, as recommended by Kramer, good as we can from our own experience acknowledge it to be in some cases, is as yet used only as an empirical remedy.

Deleau's ninth chapter treats of the exploration of the middle ear, and of the treatment of its maladies in persons born deaf and dumb. This is a subject to which our author has devoted great attention. In October, 1837, he communicated to the Academy of Sciences "A General View of his Discoveries in the Semeiology and Therapeutics of the Diseases of the Middle Ear in the Deaf and Dumb." This general view forms the introduction to the work before us, the latter being, he says, the development of the former.

In regard to the treatment of the deaf and dumb, it is to be remarked, that Deleau's cases show that he has, in certain instances, been successful in eliciting some degree of hearing; but in general neither great nor permanent.

We have to find fault with our author for mixing up, as he does, the question of restoring hearing with that of teaching the deaf and dumb to speak. Experience has proved, long ago, that the deaf and dumb may be taught to speak, and even sometimes to carry on conversation without the hearing being in the slightest degree restored. In this case the eye watches the motions of the lips of the speaker, and the deaf person knows from them the words expressed; or, as our old friend J. B., the Chirosophus, will have it, the deaf and dumb person hears with the eye, and thence learns to speak with the tongue.

We fully admit the necessity of *teaching* the deaf and dumb to speak, even supposing hearing enough for the purpose were restored. And if M. Deleau pretended merely, and stated the thing broadly and openly, to teach his pupils to speak, by taking advantage of all possible ways—among others, making them, by surgical treatment, more sensible to the impression of sounds—there would be no room for the objection of which he complains, viz., that "his pupils ought to learn to speak of themselves, without masters and without methods."

Always keeping in mind that, as M. St. Hilaire says, (*Third Report of the Institute on Deleau's Labours*, 23d Oct., 1826,) "M. Deleau a donc ajouté à ses fonctions de médecin celles d'instituteur," we think Dr. Deleau is deserving of all praise for his zealous exertions in the cause of such unfortunate objects as the deaf and dumb.

Art. V.

1. *Some Enquiries in the Province of Kemaon, relative to Geology; including an enquiry into the causes of Goitre.* By John M'Clelland, Assistant Surgeon, Bengal Army.—*Calcutta*, 1835. 8vo, pp. 384.
2. *Some account of the Bronchocele of Nipaul, and of the Cis and Trans-Himalayan Regions.* By the late M. J. Bramley, Principal of the Calcutta Medical College, (Transactions of the Medical and Physical Society of Calcutta, Vol. vi.)—*Calcutta*, 1833.
3. *Treatise on English Bronchocele, with a few Remarks on the use of Iodine and its Compounds.* By James Inglis, m.d.—*London*, 1838. 8vo, pp. 95.

Mr. M'Clelland has prefixed to the enquiry into the nature and causes of Bronchocele, which constitutes the third part of his work on the geology, natural history, and medical statistics of the sub-Himalayan district of Kemaon, a maxim of Lord Bacon, the truth of which we hesitate to admit, as its adoption would go far to remove medicine from its place amongst the sciences. If "true knowledge is the knowledge of causes," how little do we know of the nature of any disease, or of those remedies on which we place our firmest confidence? But if Laennec, who confesses that the causes of disease are almost entirely unknown, was yet able to throw such clear light on many of the most important disorders to which the human frame is subject, we may pause before we accept the dictum of the founder of inductive philosophy in its application to medicine; and zealously pursue the course of minute and accurate observation, although it may not actually lead us to the knowledge of causes. When we have learned that iodine removes a goitre of a particular kind, and fails to do so when prescribed in a different form of the disease, and when we have laid down rules by which these varieties are distinguished, we have acquired a species of knowledge as true, if not so extensively applicable, as if we could explain the vital or chemical actions by which this singular substance exerts its influence over these mysterious tumours.

While we think it right to make these observations, on the error of applying some of the principles that ought to guide physical researches to medical enquiries, we cannot appreciate too highly the labours of those who, like Mr. M'Clelland, have set themselves the irksome task of carefully collecting facts by which the cause of any diseased action may in time be ascertained. Bronchocele is a disease which has always appeared to us to be particularly interesting, and the study of which ought to be valuable, not only because its external properties and site give it a distinct and positive character which very few other diseases possess, but also as being evidently connected with certain physical circumstances in the countries in which it prevails, which admit of being determined, and through the study of which we arrive at facts not only sufficient to explain the causes of this disease, but also to throw light on many important questions relative to the influence of physical agents on animal life.

Nor will limited enquiries, confined to those countries with which our local circumstances may bring us acquainted, be sufficient to enable us to arrive at any conclusions that will bear the test of time and enlarged research; on the contrary, we must trace its prevalence, not only through our own inland districts, along the valleys of the Alps and the Pyrenees, but pursue the enquiry on the elevated plains of Columbia, amongst the lofty and damp valleys of Sumatra; and, as displayed in the works before us, in the deep and narrow dells of Kemaon, the great almost tropical valley of Nipaul, and the lofty table lands of Tibet and Tartary. Before the philosophy of medicine can deserve the name, pathology must be investigated in all the various climates of the world; and the profession must cease to turn a deaf ear to the researches of those who have to contend with disease, under circumstances of climate and modes of life very different from those in which they are met with in England or in Europe. In treating of bronchocele we may be allowed to use a geological illustration, and compare such a restricted course of study to that of the followers of Werner, who arranged rocks and speculated on their formation, from the appearances presented to his examination in a corner of Germany. Of the necessity for this enlarged enquiry, no disease affords a better example than goitre, which, from the time of Pliny till Mr. Marsden published his History of Sumatra, was ascribed to the use of snow-water, a theory at once overthrown by his account of the complaint in the moist valleys of that great island, in which snow never falls.* We would, therefore, urge those of our readers, who reside in foreign countries having peculiar climates, geological structure, or other physical distinctions, to spare no labour in accumulating facts, and in submitting them to the profession, assured that, sooner or later, they will meet with attention, however much they may be overlooked for a time by the periodical press, which too often sacrifices matters of great and permanent importance to the pressure of subjects of mere temporary interest. We know that vast accumulations, the result of most careful observation, have been lost to the public, by a feeling on the part of our countrymen practising abroad, that their labours would either be overlooked or contemned by the retailers of medical information; and we fear that too much cause has been given for the impression prevalent among them, that except they lend their views to support some fashionable system of practice, however injurious, or flatter some reigning prejudice, they will either be passed over unnoticed or unjustly condemned. Had the methods of practice followed in India early in the present century, and the success attending them been given to the public, we are satisfied that much severe suffering and loss of life, caused by the successful advocacy of a rude and indiscriminating use of the lancet, large doses of calomel, and the abuse of drastic purgatives would have been prevented.

Mr. M'Clelland's work is divided into three parts: the first is dedicated to a detailed physical and geological description of the district of Kemaon; the second to its zoology and meteorology; and the third, consisting of 130 pages, to the investigation of the causes of bronchocele, and contains the most minute statistical details on this subject with which we are acquainted. The geological portion of the work, although much tinctured with the doctrines of the Wernerian school (which Mr. M'Clelland has since renounced), and with an error natural to a pupil of that school, of attempting, on very insufficient grounds, to reduce to European types the rock-formations of the Himalayas, yet abounds with those minute details of structure and composition, so much neglected by many of the best English geologists, and so essential to the physician, who would examine into any alleged connexion between the nature of the rocks and the diseases prevalent amongst the inhabitants who live on them. In this view, Mr. M'Clelland's geological researches may be considered as introductory to his enquiry into the cause of goitre; and we cannot but admire the ardent zeal which enabled him, while actively engaged in professional duties, to investigate the structure of nearly 1000 square miles of a wild mountainous country, intersected by deep unhealthy valleys. The overcoming such difficulties deserves our warmest praise; and it reflects credit on the present Indian government, to have rewarded his unaided exertions, by appointing him a member of the committee to explore the tea-country to the N.E. of Bengal, and secretary to the recently constituted commission for investigating the mineral resources of our eastern empire. It is, however, to be regretted, that Mr. M'Clelland did not publish his researches on goitre in a separate form, as few medical readers will be able to procure, or have time to peruse, the copious details of a non-medical nature that constitute the bulk of the volume. We shall therefore give a copious abstract of the important facts contained in the work, in the hope that it may induce those of our readers, either in this country or on the continent, who are favorably situated, to follow Mr. M'Clelland's example, and to communicate accurate statistical details, accompanied with geological and meteorological observations, and information on the habits of life of the inhabitants of affected districts, with the view of testing the truth of his conclusions.

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* See also Phil. Trans., 1784.

The snowy peaks of the Himalayas, from 18 to 25,000 feet high, bound the district of Kemaon to the north, while the rich plains of Rohilkund extend from the opposite side of the province to the Ganges, the intervening country being occupied by precipitous ranges of mountains from 4 to 8,000 feet in height, inclosing deep and narrow valleys, and giving passage, by narrow and tremendous defiles, to the tributaries of the Gogra, one of the great feeders of the Ganges. These streams are sometimes seen but oftener heard rushing in furious torrents down the river-valleys which divide the mountain groups from each other, and which are usually from 2 to 5,000 feet below the adjoining summits. Mr. M'Clelland divides the province into three districts; the first extending from the Burmodea pass, by which the road enters the mountains from the low and moist tract, of which Heber has given so appalling a picture, to Balket, and is composed of a rugged group of mountains, 8,000 feet in height, mostly composed of clayslate, and covered with dense forest.* This tract is nearly uninhabited, and the deep valleys near the plains cannot be entered during six months of the year, on account of the deadly fevers that then prevail.

Mr. M'Clelland was not able to prosecute his researches to any extent amongst the few migratory inhabitants of this dismal tract, who spend the summer on the mountains, and when the sickly season is past, return to the valleys to avoid the rigors of winter; we shall, therefore, proceed to an analysis of the remainder of the paper. In the following extract he states his opinions as to the cause of goitre, and the course of enquiry by which he was led to adopt them.

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* It may be proper to state that, by some mistake, Mr. M'Clelland mentions extensive beds of gypsum, which turned out to be a rock of a very different kind. See paper, by the author, Journal of the Asiatic Society of Bengal, August, 1837.

"During the course of the enquiries into the geology of the province, contained in the first part of the work, I was struck with the frequency of goitre in one portion of the district, while the other was almost exempt from the complaint, although an equality of moral as well as physical circumstances appeared to affect the whole. The external Alpine characters of the province are the same in every part, the inhabitants all belong to the same tribes of Hindoos, and are subject to fewer irregularities in their mode of life than any other people in the world. In such a field there could be little merit in eliciting highly important facts connected with this intricate subject.

"That portion of Kemaon which lies on the south of the Ramesa river is composed of siliceous and argillaceous rocks of the primitive class. In the centre of this ridge there are numerous small valleys, some of them 7,000 and others as low as 3,000 feet above the sea, inhabited by persons who, some to avoid the winter's cold of their native mountains and some to avail themselves of pasture for their cattle, descend into the plains, and are absent from their villages five months of every year. From enquiries which I made amongst these people, I found them to be affected with goitre in the proportion of 1 in 500; but, as they do not constantly reside in the mountains, they are excluded from the more minute statistic details.

"The north-eastern acclivity of this chain is intersected by numerous deep river valleys and ravines, as well as by low mountain ridges, which afford a climate more congenial to the feelings and wants of the inhabitants, who here reside constantly in their villages. Of these villages 46 have been visited; but two of their number, having been occupied for only three or four years, are excluded from the general view; so that the number of villages on the south of the Ramesa river, which we are now to consider, amount to 43, and contain a population of 3,700; of this number I found only 17 persons affected with goitre, and these were exclusively adults. The different localities of these villages are as diverse as can well be imagined. Some are erected on narrow ridges, others in deep valleys, surrounded by abrupt and lofty mountains; others on rugged declivities, between lofty peaks on the one side and dark ravines on the other, into some of which the sun can scarcely penetrate. The different altitudes of these villages vary from 2,000 to 6000 feet.

"Let us now cross the Ramesa river, and enter the district of Shore, whose geological distinctions have been pointed out in a former part of this work; and we find that one eighth part of the people are affected with goitre. Yet the whole of the inhabitants of the province are equally circumstanced with respect to religion; they intermarry, have the same customs, and are affected alike by moral and political influences; and finally, the tract in which the disease prevails is the richest and most fertile portion of the province.

The natives themselves impute to the quality of the waters a powerful influence over their health; and when it is recollected that water and farinaceous vegetables constitute the chief diet of the Hindoos, any impurity of that fluid would produce effects more readily upon them than on persons whose food and habits are less simple; but whether they are right or wrong, in ascribing the prevalence of goitre to the impurity of particular waters, I shall not here stop to enquire. A subject on which so many conflicting opinions exist requires to be elucidated by such facts as, from their number, force, and simplicity, can lead to no erroneous interpretation; and in collecting these facts, the method I adopted, on observing the prevalence of the disorder in one great section of the district, and its absence in another, was to mark the physical characters by which these places were distinguished from each other. The consequence was, a perfect agreement in external aspect, altitude, and chimatology, but a very marked difference in their geognostic relations: and this distinction, which was even traced down to the very villages in which the disease is found, with such perfect nicety, as to enable one almost to predict *a priori*, on examining the rocks of a neighbourhood, whether the inhabitants are affected with goitre or not. In pursuing the enquiry further, it is found that every village in the same neighbourhood is not equally affected, but that some are quite exempt, and others affected to the extent of half their population; and this difference is not found to depend on any accidental or transitory cause, such as usually influence epidemic complaints; but has always affected the inhabitants of a particular village, while those of adjoining hamlets have continued perfectly and permanently free from the complaint." (pp. 265 to 270.)

The primitive district above referred to, constituting the second division of the province, is composed of granite, hornblende slate, gneiss, mica, and clayslate, each of which impresses a particular character on the ranges they compose. The sides are usually precipitous, and covered by a thin and unproductive soil, giving support to a poor and scanty population. The following Table exhibits the result of the detailed observations contained in the second section.

Districts, and groups of Villages.	Number of high-caste inhabitants.	Number of low-caste inhabitants.	Total.	Number of high-caste inhabitants with goitre.	Low-caste inhabitants with goitre.	Rocks composing the sites of Villages, and in which the Springs are situated.
A. Patan	200	0	200	0	0	Clayslate.
B. and C. Gomedoce	850	100	950	3	1	Transition-slate.
D. Pausall	700	100	800	3	0	Clayslate.
E. Agee	550	50	600	4	0	Clayslate.
F. Bentally	460	40	500	4	2	Clayslate.
G. Jeercoonee, north side	200	0	200	0	0	Siliceous sandstone.
Ditto, south side	150	50	200	0	0	Clayslate.
H. Popoulee	50	4	54	0	0	Hornblende-slate.
Rigong	170	26	196	0	0	Transition-slate.
	3330	370	3700	14	3	

To the abstract here given might be added a population of about 4,000, who inhabit the gneiss and granite district, and who reside a few months of the year in the plains. These persons are affected with goitre in the proportion of 1 in 500, which would make the whole population of the primitive mountains, embraced by the map, on the south of the Ramesa river, 7,700 souls; of these, about 25 are affected with goitre, or about 1 in 308. Those villages are excluded from the table which have not been inhabited more than nineteen years, or since the period when the province fell into the hands of the British, as also the villages connected with the military posts.

The following is a specimen of the facts of which this table contains an abstract. *b.* Nine villages, situated at a mean height of 4,300 feet, on the N.W. acclivity of a mountain of primitive and transition slate, and having a mean annual temperature of about 64°, contain 800 inhabitants, of whom 4 have small goitres, acquired, they said, in their youth, when residing in a distant part of the country. *c.* Four small villages, on the east of the same mountain, in the hot valley of the Maha-kalee river, at a height of 3,800 feet, has no case of goitre. *g.* On the rugged mountain of Jeercoonee, inclosed by deep ravines, are seven villages, containing 400 inhabitants, without a single case of goitre. The altitude of these villages is from 2,200 to 3,500 feet; but, being exposed to the sun and sheltered from the prevailing winds, their temperature is high. Nothing can be more frightful than the situation of these villages, overhung by lofty cliffs and mountains, while deep chasms lie below.

That this remarkable exemption does not depend on hereditary predisposition, Mr. M'Clelland properly infers, from numerous cases having occurred to him of persons acquiring the disease on removing to an affected district, and from the disease becoming stationary or disappearing entirely during a residence in a healthy village. This is further confirmed by a comparison of the health of 400 troops stationed for three years at Lohoogât, in the part of the province included in the table, of whom not one acquired the disease; while at Petovagur a similar detachment, which entered the hills at the same time, and was in every respect similarly circumstanced, had no goitre during the first year; 5 during the second; and before the end of the third, no less than 16. The former station is in a small valley, 5,562 feet above the level of the sea, having an east and west direction, and surrounded by hills that rise from 300 to 1000 feet above it. The mean temperature is about 60°. Clay-slate and granite rock, covered by a stratum of red ferruginous clay, are the only rocks to be seen; and from the former numerous springs, whose temperature varies at different seasons, issue and supply the wants of the troops. By some experiments recorded in a different part of the work (p. 355 and 259), it appears that the solid contents of the water amounts only to about $\frac{1}{13000}$, and that it is nearly deficient in carbonic and sulphuric acid, iron, and lime, but contains a little alkaline muriate. The waters of eleven springs in the clayslate in different parts of the province have the same properties. Petovagur, fifteen miles north of Lohoogât, stands on the summit of a low ridge, that extends into the centre of Shore valley, throughout which goitre is very prevalent. The general level of the valley is 5,000 feet above the sea, and that of the cantonment 460 feet higher, and separated by several miles of rich cultivated plain from any loftier hills; its situation is, consequently, more open than that of Lohoogât. The mean annual temperature is 1° 40′ higher. The approach to Petovagur passes along a small river valley, over rocks of primary limestone, forming the most frightful precipices that can be imagined. These precipices compose broken and seemingly tottering mountain acclivities, that ascend in some places 3,000 or 4,000 feet; and, as the only road lies along the verge of these cliffs for several miles, it is impossible for the most indifferent traveller to pass insensible either to the danger of his situation or the beauties of the scene, increased by the contrast of the rich valley into which it leads, and from which the ridge of Petovagur rises. This ridge is composed of clayslate supporting extensive deposits of limestone which give a rugged aspect to the surrounding country, the scenery of which is thus described.

> "Primitive clayslate forms the basis of this part of the district, and ascends to elevations which are occasionally above 8,000 feet, on which limestones rest, and bestow their particular stamp on the aspect of the neighbourhood. The mountains are here more majestic than in either of the other sections; each individual standing almost detached from the group to which it belongs, and bearing some well-marked charac-

ter, which leaves on the mind an impression not easily effaced. Thus we find, in the Shore district, every mountain distinguished by some traditional name, derived from a sacred work or ancient temple, which usually caps the summit. At certain festivals crowds of the superstitious population resort to these romantic caves and temples; and on more private occasions the solitary devotee often ends his life, in the attempt to gain an almost inaccessible summit, in order to invoke some grotesque representation of the deity to which the mountain is dedicated. How forcibly the relation of such localities, for religious purposes, attests the influence of what is awful in nature over the mind of man, even in his rude and nearly savage state! In the infancy of civilization (says Humboldt) high places were chosen by the people to offer sacrifices to their gods; the first altars—the first temples were erected on mountains. This remark was suggested by his intercourse with the aboriginal inhabitants of the Andes; its accuracy is confirmed by the customs that prevail among the Hindoos of the Himalayas." (p. 36.)

Copper and iron pyrites are interspersed between the limestone and slate, and disseminated through the strata, seams and rifts of the limestone; while the lower levels of the valley are composed of beds of gravel, the debris of surrounding mountains, cemented by calcareous matter.

The waters of four springs at this station, and of a number of others where goitre prevails, were examined, and nearly agreed in their character. They were copious, less numerous than those in the clayslate, and of a temperature corresponding to the mean of that of the place. The water often rushes from the earth with ebullition, crystalline and sparkling, and deposits immense quantities of calc-tuff; and the rocks around have usually a ruined and broken character. Carbonic acid and lime, with a little iron, and earthy alkaline muriates were detected. No sulphuric acid was indicated by the usual tests.

Mr. M'Clelland has given a statement, showing the admissions from all diseases for three years, into the hospitals of Lohoogât and Petovagur, from which it appears that the sick were nearly twice as numerous at the latter as at the former, and that fever and bowel complaints were one third more prevalent; which circumstance indicates a greater intensity of endemic causes in the district where goitre prevails: an important fact, regarding which we could have wished more information in the statistical details that follow, but which, we are aware, it may have been impossible to obtain.

Throughout the district of Shore and the neighbouring villages, the prevalence of goitre follows, in a remarkable manner, the situation and nature of the strata; the affected villages running along the limestone rocks parallel to the central ridges, composed of clayslate, and which are nearly or altogether exempt from the disease, except in some instances, where the inhabitants are supplied with water from springs, rising in limestone rocks. Mr. M'Clelland enters into many minute details to show that this relation, which is so striking when the grand divisions of the country are considered, is equally so when neighbouring villages are examined; *and even when different parts of the same village, or different classes of the inhabitants use the water of springs issuing from different rocks.* The following Table contains an abstract of these enquiries regarding forty villages (p. 298):

Paragraph under which the villages are described.	Brahmins and Rajpoots.	Domes (low caste.)	Total inhabitants.	Brahmins and Rajpoots affected with goitre.	Domes affected with goitre.	Total in each village affected with goitre.	Rocks from which the water is derived for the use of the inhabitants of each village.	Rocks on which the villages are erected when different.
1	60	...	60	...	...	...	Clayslate	
2	25	...	25	...	...	...	... ...	
3	7	7	14	2	4	6	Transition limestone	
4	25	...	25	...	...	...	Clayslate	
5	40	30	70	2	3	5		
6	10	8	18	7	5	12	Transition limestone	Conglomerate of calc.-tuff, slate, and limestone.
7	...	20	20	...	7	7	... ...	Clayslate, coated with calc.-tuff.
8	...	40	40	...	2	2	Clayslate	
9	48	6	54	3	1	4	Limestone and slate	Clayslate.
10	20	50	70	3	13	16	Limestone (?)	Clayslate, incrusted with calc.-tuff.
11	50	30	80	8	10	18	Limestone (?)	Clayslate incrusted, with calc.-tuff.
12	70	20	90	1	6	7	Limestone and slate	
13	60	...	60	...	...	...	Clayslate	
14	25	...	25	...	...	..	... ...	
15	18	22	40	...	...	...	... ...	
16	70	...	70	2	...	2	... ...	
17	30	...	30	...	...	..	... ...	
18	40	10	50	...	...	...	... ...	
19	...	40	40	...	...	...	... ...	
20	100	...	100	...	...	...	... ...	
21	15	...	15	...	...	...	... ...	
22	25	...	25	...	...	...	... ...	
23	100	...	100	...	...	...	... ...	
25	15	...	15	5	...	5	Transition limestone	Clayslate, coated with calc.-tuff.
26	...	14	14	...	7	7	Limestone	Clayslate, coated with calc.-tuff.
27	...	16	16	...	10	10	Transition limestone	Conglomerate of calc.-tuff, clayslate, and limestone.
28	60	40	100	27	33	60	Limestone (?)	... ...
29	80	...	80	...	...	...	Clayslate	... ...
30	25	...	25	7	...	7	Limestone	Conglomerate of calc.-tuff, and fragments of slate.
31	10	...	10	5	...	5	... ...	Clayslate (?)
32	10	15	25	4	9	13	... ...	Clayslate and calc.-tuff.
33	25	...	25	4	...	4	... ... (?)	... ...
34	25	...	25	10	...	10	... ... (?)	... ... —
35	24	...	24	10	...	10	... .. (?)	Conglomerate of calc.-tuff, serpentine, &c.
36	40	...	40	...	..	...	Clayslate	Clayslate and scattered blocks of limestone.
37	40	..	40	..	...	...	Clayslate	Clayslate and scattered blocks of limestone.
38	40	...	40	...	...	...	... ...	
39	100	...	100	...	...	...	Partial sandstone	
40	40	..	40	...	...	...	Clayslate	Clayslate and magnesian limestone.
	1372	368	1740	100	110	210		

But this Table will by no means convey a sufficient idea of the minute information on which it is constructed; to some examples of which we must, therefore, request the attention of our readers. 1st. *Beesty*, a village situated on a low group of clayslate hills, in the centre of the valley, has 60 inhabitants (Rajpoots), of whom only one old man has goitre, and he is a stranger, in whom the tumour has diminished since he came to reside in the village. 3. *Salmora*, situated like the former, a little above the valley, but on limestone. Its inhabitants consist of only two families, of different castes, of whom six have goitre, and one (a Rajpoot) is deaf and dumb, with a large head and idiotic expression of countenance, 6. *Goseragong* contains 18 inhabitants, of whom 12 have enormous goitres, and one had died of the disease a few days before Mr. M'Clelland's visit. They assured him that they were generally cut off by the disease before the age of 50. It stands 200 feet above the level of the valley, on a coarse conglomerate of calc.-tuff, and lofty masses of limestone rise abruptly behind it, to the height of 2,000 feet, causing the atmosphere to be confined and hot; but in none of these respects is it more objectionable than the healthy village of Jeercoonee, already mentioned. 9. *Panera.* Of 30 Dome inhabitants using a spring which descends from the limestone cap of a neighbouring mountain, 4 have goitre; while 24 Rajpoots, who obtain it from clayslate, are free from the disease. 12. *Panorah* is built on clayslate, slightly coated with lime, and contains 70 high-caste inhabitants and 20 Domes; of the former, one only is affected with goitre, while six of the latter labour under it. " The Brahmin inhabitants of this village derive their water from a spring in clayslate; and, as the prejudice of the Hindoos denies to the Domes the privilege of partaking of the water of the same spring, the excluded caste is forced in this, as in many other cases in Kemaon, to use this fluid from what they, as well as the Brahmins, believe to be impure sources; and in this instance it is taken from a stream that issues from the same limestone cap that affords water to the two last-described villages." (p. 284.) 13. *Paruree.* This village is situated very low in the valley, on clayslate, affording very fine springs, and no case of goitre exists amongst its 60 inhabitants. The localities of Nos. 17, 18, and 19, amongst whose 120 inhabitants there is no goitre, bear a striking resemblance in external aspect and geological structure to Nos. 25, 26, and 27, of whose 45 inhabitants no less than 22 have goitres. These last are erected on a conglomerate of calc.-tuff, inclosing fragments of clayslate, and partly on a clayslate hill, which rises 300 feet above them, and is covered by a massive cap of limestone, pouring out numerous fountains, charged with carbonic acid and lime, the water of which is taken for use as it jets from the rock. The altitude, aspect, temperature, religion and morals of both groups of villages are the same, but the former derive their water from springs in clayslate.

The account of No. 28 we shall give in Mr. M'Clelland's own words:

> " *Deota*, a lengthened village, which occupies half a mile of the foot of Durge mountain. One extremity of it is inhabited by Brahmins, the other by Rajpoots and Domes. Of the first caste, there are about 20 persons, all of whom are free from goitre; there are 40 of the second, and two thirds are affected more or less; and of the third caste, 46 in number, nearly the whole are affected. To what cause can we ascribe the immunity of one caste of the inhabitants of this village, and the almost

universal affection of the other two castes? They are all alike well fed, and have little toil: their land producing the requisites of life almost without labour. Difference of caste does not here imply a difference of pecuniary circumstances, and, consequently, of the comforts of life. In these respects the three castes in this village are on perfect equality; nor will hereditary predisposition, acquired by intermarriages, be sufficient to explain the interesting fact; for the affected parties are confined to the Rajpoots and Domes, who cannot intermarry, while the Brahmins and Rajpoots may. The village is raised about 100 feet above the level of the valley, and the mountain, at the foot of which it is situated, rises with a gentle slope, and is not, in this vicinity, at all rugged. It is chiefly composed of transition limestone, and the village is erected on a conglomerated rock, composed of calc.-tuff, inclosing fragments of other rocks. There is a spring situated in the valley, about 100 yards from the village, which, from its first appearance, has the character of a mineral spring. The water bursts forth with strong ebullition, in the quantity of at least 40 gallons a minute, and agglutinates the sand and gravel by which it is surrounded by the deposition of calcareous tuff. The temperature and quantity of the water is the same at all seasons. The former inhabitants of this village, aware, perhaps, of the noxious effects of the spring, had an aqueduct formed, by which water is conveyed into the Brahmin portion of the village, from a distant source. The aqueduct being allowed to go out of repair, the quantity of water it transmits is reserved exclusively for the Brahmins; but during the rainy season, when water is plentiful, the Rajpoots also use the water of the aqueduct; but the Domes have no alternative at any season but to use the water from the spring." (pp. 285-292.)

Mr. M'Clelland has omitted to state from what rock the water of the aqueduct is derived; but we infer from what follows that it is from clay-slate. 30. The accidental circumstance of a stream depositing calc.-tuff being conducted to the village by a circuitous route to irrigate the fields, seems to have lessened the prevalence of the disease. Nor are the Brahmins exempt when they derive their supplies of water from the limestone; No. 31, containing 10 inhabitants of this caste, having one half affected with the disease. 32. *Oliel.* Two small hamlets, situated in a most pleasing amphitheatre of limestone mountains. Of 25 inhabitants, 13 have goitre; of whom 10 *are cretins, and of these a whole family are deaf and dumb; they seemed also deficient in sight, and quite unsusceptible of the passions of joy and fear.* 35. *Gooraght,* partly built on clay-slate and partly on a calcareous conglomerate, containing with other rocks blocks of *serpentine.* Of 25 inhabitants, 10 have goitre, *and a father and two sons are cretins: the two sons are deaf and dumb.* 40. *Deoneolla.* Situated in the lower part of the valley, partly on clay-slate, and partly on *magnesian limestone, and surrounded by high mountains: had no case of goitre.*

We regret that Mr. M'Clelland's work does not furnish the means of instituting a comparison of the prevalence of bronchocele in villages situated on common limestone, and on the dolomitic rocks he has described (but of which he has given no analysis), as we might thus have had more light thrown on a suggestion of Dr. Inglis's, that the latter is the rock chiefly concerned in producing the disease.

"I cannot say," observes Dr. Inglis, "that in all lime districts, goitre prevails; on the contrary, I know that it does not in many places where the *blue vitrified mountain limestone* is very abundant. Nevertheless, I think that I am correct in supposing that the presence of the magnesian limestone always predicts the co-existence of the disease. Take, for example, that ridge of magnesian limestone running from north to south, through the centre of Yorkshire and margining the shires of Derby and Nottingham. All along that line we have goitre to a very great extent, whereas, on our diverging to either side, the disease is found to diminish. The towns situated on this ridge are Nottingham, Chesterfield, Rotherham, Ackworth, Pontefract, Abberford, Wetherby, Knaresborough, Boroughbridge, and Ripon. After this the magnesian limestone dips, then reappears in the county of Durham; it continues its course almost due north from Darlington to South Shields, where at Tynemouth it meets the sea. In many of these towns I know goitre prevails; *in the others, I should suppose it did,* excepting *where we approach within the influence of the sea, when the morbid action would be counteracted,* for we found before, from Dr. Richardson's statement, that the disease itself was removed by a sea voyage." (p. 23.)

The vagueness of the expressions quoted in italics, and several of the places mentioned not being situated on the magnesian limestone, do not justify us in placing much reliance on these opinions, especially as Dr. Inglis has instituted no comparison between these places and the chalk valleys of Hants and Surrey, and of those parts of Devonshire, and other counties, where there is none of this rock, and where the disease is nevertheless very prevalent, nor are such general assertions as the following properly admitted into enquiries like the present: "Amongst the Alps, in greatest perfection, is met with that double carbonate of magnesia and lime, called Dolomite," of which the snow "water must doubtless carry with it a portion." (p. 15.) Yet the prevalence of the disease, along the upper division of the Lake of Como, where Dolomite rocks are extensively distributed, renders it important to obtain accurate statistical details regarding these very accessible localities. At Colico, for instance, which stands on an unhealthy swamp near the mouth of the Adda, almost the whole inhabitants are afflicted with goitre, while those of other towns are comparatively healthy.

The result of Mr. M'Clelland's enquiries, in various other valleys, is contained in a table, (p. 310;) but we must content ourselves with noticing one or two of the most important observations contained in this section. The valley of Kalapany is amongst the lowest inhabited places in Kemaon, closely surrounded by lofty mountains, and the presence or absence of goitre is marked by the same circumstances as in Shore valley: thus, in the village of Beechelly, having 70 inhabitants, there are 30 persons affected with bronchocele; this village is closely surrounded by mountains of transition limestone, and is erected on alluvial soil, cemented by calc-tuff; while Remina, a mile distant, on slate, has only one case out of 50 inhabitants. In the wild valley of Roilputty there are only two villages, containing 25 inhabitants each, and both situated on slate-rock. The first derives its water from a stream falling over limestone precipices, and a third of the inhabitants approach to the condition of cretins, six of them having goitre; the second, which derives its water from the same stream, one mile and a half farther down, has only one goitre. In this instance Mr. M'Clelland has neglected to communicate the requisite information, as to the course of the stream previous to its entering on the plain, by a short course, through which it is supposed to have lost its noxious qualities.

We shall conclude with a short notice of the valley of Baribice, elevated 4,000 feet above the sea. The eastern extremity is composed of *clayslate*, and in five villages, *containing* 152 *inhabitants, there is not one goitre.* The other extremity of the valley is *partly composed of limestone, and of* 192 *inhabitants, distributed in six villages,* 70 *are affected with goitre*; but Ducygong, one of these villages, *supplied with water* *from clayslate, has not a single case of the disease; while Ager, only half a mile distant, and containing* 50 *inhabitants, has no less than* 40, *and of that number* 20 *are cretins.* They use the water which issues from an old copper mine in limestone, which contains carbonates of lime, soda, &c., but no sulphate: they were earnestly solicited to discontinue its use, and to employ that of a spring at a little distance, in the full confidence of their being benefited by the change. (p. 304.)

These unfortunate beings, oppressed by their owners or superiors, are reduced to labour in the mines, from infancy to old age, for an allowance hardly sufficient to support life; and the drifts of these works are so low, that the ore can only be dragged out by children, who are, for the most part, frightfully deformed, (Chapter on Mines, p. 168;) a state of things, however, which exists elsewhere in the province, without producing the same effects.

Mr. M'Clelland has devoted a chapter to the circumstances in which cretinism occurs in the Kemaon, which is the more necessary, as Dr. Inglis has stated, apparently on the authority of Mr. Bramley, that cretinism is quite unknown throughout the Himalayan regions, although, in many places, 48 out of 50 inhabitants are goitrous, (p. 94;) and on this has grounded an argument for the distinctness of the diseases, and of their causes. He has ascertained that where goitre prevails, *to a certain extent*, the people exhibit a want of enterprise and bodily vigour, as compared with their immediate neighbours, who are exempt from the disease; and the distinction increases, "not always, but in general, with the extent and severity of goitre," until at length the body is found generally deformed, the head often of enormous size, the spine distorted, and the limbs short and crooked; or swellings of the elbows, knees, or other joints, take place; as also of the lymphatic glands, especially those of the lower part of the neck, and perhaps of the thymus; showing the disease to be complicated or connected with a strumous habit. More frequently the deformity is confined to the head; the features are bulky, and more or less vacant and idiotic, and all the sensations are blunted, yet the individuals seem in general to retain some portion of intellect, but their minds seem never to advance beyond the state in which they were in early infancy, during which this form of the disease always commences, generally about the third year. Congenital swellings of the neck (and sometimes of other parts, as the lobe of the ear,) are observed in animals in Kemaon; and Dr. Campbell has described 22 examples of this in Nipaul, in 17 of which the tumours were composed of cells, containing a glairy fluid, and in five, had a glandular structure. Dr. Campbell has known this to occur in the young of healthy goats and sheep, brought from parts of India, where goitre is not known, (*Calcutta Transactions*, vol. vii.) Both authors are of opinion that it is never congenital in the human subject, but Mr. Bramley and Mr. Baillie Fraser, in his instructive work on the Himalayas, assure us that it is so, and that they have seen it in the youngest infants, a fact supported by the statements of Dr. Inglis and other European authorities, and of which we consider Dr. Campbell's interesting paper to afford a striking confirmation.

Such are some of the facts on which Mr. M'Clelland grounds his opinion, that the predisposing cause of goitre is the use of water impregnated with calcareous salts; and he states that in the course of his personal enquiries, extending over 1,000 square miles, without a view to any theory, no instance has occurred, in which goitre prevails to any extent, where the villages were not situated on or close to limestone rocks. But while we admit the force of these facts, and admire the industry with which they are collected, we are not prepared to admit, without more extended enquiry, conclusions so important, and which leaves unexplained the fact, that the disease is nearly unknown in many districts where lime abounds in the waters, as amongst the poor peasantry of the Roman states, where marble is deposited amidst the foam of the cascades of Tivoli and Terni, or where the sluggish Sarnus and Salso, loaded with lime, pass through the rich and salubrious territory of Naples, or the pestiferous environs of ancient Pæstum. Our own island affords admirable opportunities for investigating the subject, and we are therefore glad of the appearance of Dr. Inglis's pamphlet, as showing that attention has been directed to it, and because this pamphlet contains some valuable facts relative to the prevalence of the disease in several of our towns and districts, and at various periods of life. We are constrained, however, to observe, that we shall expect, in the promised continuation of the enquiry, more caution and greater minuteness of detail; and would recommend Dr. I. to prepare himself for the investigation, by acquiring the requisite information on the geology of our island, without which he must constantly fall into error. We have already alluded to the careless manner in which he refers to the towns in which bronchocele either prevails or in which *he supposes it does;* and in the appendix communicating the result of new investigations, the absence of goitre from the northern counties of Scotland is thus noticed: "Lime, to a considerable extent, exists in these counties; but, as in other parts of Scotland, it is the *pure mountain*, and not the magnesian limestone." (p. 79.) We are at a loss to understand how the great siliceous and calciferous conglomerates and sandstones, with their subordinate beds of cornstone, of the old red sandstone formation, so extensively distributed over these counties, and so well described by some of our most eminent geologists, or the primitive limestones of the Grampians, could have been confounded with the mountain or carboniferous limestone. If such gross errors find their way into medical works relative to Britain, the geology of which is of all countries best known and most accessible, what hope have we of advancing one step in any professional enquiry connected with other physical sciences? Equally futile is the analogy drawn between the coal formation of England and the tanks of India, as producing goitre.

In the prosecution of his enquiry, in addition to endeavouring to ascertain the whole of the physical and moral circumstances of the inhabitants of each place; we would suggest to Dr. Inglis a particular investigation of the supposed influence of the sea-air in preventing the disease, of which we have heard stronger proofs than the fact quoted from Dr. Richardson; and whether any impregnations, such as iodine, exist in certain waters, to which exemption may be ascribed. The details of analysis of waters should also be put on record, when these have been performed by chemists of known accuracy. But to do all this with any prospect of advantage, the author must give up all idea of writing "in some measure, as well for the people as the profession," regarding a disease for which he proposes no method of prevention, and recommends no remedy but iodine: in this, however, he has so much confidence, and thinks it so safe, that he would wish to see "the people" employ it without medical advice.*

We shall now turn to Mr. Bramley, whose position, as surgeon to the ambassador at Nipaul, offered many facilities for extending his enquiries into the surrounding regions, through the numerous travellers who meet in this intermediate post between the east and west of Asia. This lamented physician has the glory of having founded, and almost matured, the first medical school in which the natives of Asia are instructed in all branches of European medical science; and his varied acquirements and his knowledge of bronchocele as it occurs in Switzerland, peculiarly qualified him for the enquiry, and has enabled him to produce a most instructive memoir. The style, however, is too diffuse, and the observations of authors are occasionally so mixed with his own, as not easily to be distinguished from them; nor has he attempted to account for the striking facts he records of villages where goitre is very prevalent being situated close to others in which it is unknown, and which irresistibly forces us to refer the disease to a local cause. The distribution of rocks in Nipaul are more simple than in the neighbouring district of Kemaon, and we hope that the present distinguished resident,† or some of the gentlemen attached to his mission, will include this enquiry in their other important researches into the natural history of this most interesting district. Mr. Bramley enlisted the chemical skill of Mr. Prinsep, in an enquiry into the composition of some of the waters of Nipaul. Sulphate of lime is almost entirely absent from the whole, and the carbonates are by no means abundant; but there is no information as to the precautions taken to prevent the escape of the carbonic acid, and the deposition of lime, previous to the analysis, nor of the prevalence of goitre amongst the inhabitants using them. Of the seven waters examined, however, one, which contains only a little muriate of soda and a trace of iron, is from Sanchoo, a considerable town, situated on an eminence in the valley of Nipaul, and surrounded by a luxuriant forest, in which it appears, from another part of the paper, that goitre does not occur although very prevalent in the surrounding villages: the Nipaulese also universally ascribe the disease to the water. (Compare p. 188 with table, p. 202.) Mr. Bramley did not find goitre much more common amongst women than men, but in the latter the tumour was often small, and frequently unnoticed by the subjects of it: of 65 goitres in the town of Handygong, 29 were in men, 31 in women, and 5 in children; and in Deopatan, 34 were in men, 38 in women, and 11 in children. He confirms the general opinion that the periods of adolescence and of parturition favour the development of the disease; thus, in 124 males and 140 females, 15 of each were under ten years, a proportion not very different from that given by Dr. Inglis, from an excellent report by Dr. Paley, of Bishoptown.

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* Surely Dr. Inglis does not mean the assertion that goitre is as prevalent in some parts of Yorkshire as in any of the Alpine villages to be taken literally; and why allege, as a reason for addressing his book to the people, that the disease is "evidently on the increase" (Preface), of which no proof is adduced? A good account of the disease, as it prevails to the north of the Dartmoor hills, while the southern limestone tracts are exempt from it, would be a valuable addition to our knowledge.

† Mr. Hodgson, the distinguished zoologist and orientalist.

Bronchocele has been ascertained to prevail from the left bank of the Ganges, in the plains of Hindoostan, to the northern boundaries of Tibet, from Assam,* in the 27th degree of north latitude, and 93d of east long., the northern boundary of Oude to Huredwar, in 30° north latitude, and 78°, 25′ east long. It occurs, along with cretinism, beyond the great wall to the N.E., and at Yarkmid to the N.W. of China, and, as Mr. Bramley was informed by Tibetan travellers, in the Mongolian and Tibetan countries lying between them; and it may be traced in various places near the lake of Baikal, and on the Lena. Over such vast countries all varieties of climate prevail, and the food of the inhabitants differs from a nearly pure farinaceous diet to one almost exclusively animal. To show this more distinctly, Mr. Bramley divides the country, to which his personal observations or enquiries extended, into three districts, of which the first, extending from the Ganges to the mountains, consists of two parts, the one richly cultivated and salubrious, partaking of the seasons common to the rest of India, and whose inhabitants may be regarded as a healthy race of men, having the common physical attributes, and living on the ordinary food of Hindoos and Mussulmans. Goitre occurs at Tirhoot and other districts of this tract; and Mr. Evans mentions that European children recover from it on being removed to situations where it is not prevalent. The other portion of this tract is that dismal region called "Terai" (the moist), which bounds the plains, and in which an excess of vegetation and humidity, and the want of ventilation, caused by the dense forests that cover it, prove most destructive to health and life; and while it prevents the access of strangers, causes the inhabitants to have a mean and stricken appearance, and to be debased in form, size, and strength. Amongst them, Mr. Bramley states the disease to be very common, but in this he differs from Mr. M'Clelland, whose observations, however, refer to a different part of this unhappy tract. The second region is that central mountainous country, including Nipaul, Kemaon, &c., between the plains of India and the elevated table-land of Tibet, consisting of irregular masses of very precipitous mountains, varying in elevation from 500 to 25,000 feet, and in some places inhabited at an elevation equal to that of Mount Blanc. It is subject to temperatures as various, both in changeableness and intensity, as its infinitely diversified elevations and physiognomy; for while the sugar-cane flourishes in the lower valleys, the cerealia disappear from the higher elevations. The distribution of the seasons is tropical, but influenced, even in the valleys, by the cold blasts from the higher mountains; its humidity is less than that of Bengal, and greater than in Europe, and at the end of autumn fogs prevail. The soil is deep, produces abundance of good grain, and the people live in tolerable ease, and consume a good deal of animal food, especially the aboriginal and inferior tribes. Goitre prevails very extensively, particularly amongst the poorer classes, and on the borders of the great valley at an elevation of from 500 to 5,000 feet above it; as, at Phirphen 15 per cent., "Chitlong 40, and at a small village, named Chirphu, on the crest of a high mountain, 48 individuals had the disease out of a total of 53. These were all counted by myself, and included men, women, and children, excepting only those in the arms." (*Bramley*, p. 186.) And natives, coming from those parts of India where the disease does not prevail, become subject to it, to the extent, even at the capital, where it is comparatively rare, of 15 per cent. per annum.

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* Leslie on the medical topography of Gomattee, in Lower Assam. (Calcutta Medical Transactions, vol. vi.)

The Trans-Himalayan region, to which these remarks apply, is a vast and nearly uniform plain; the soil is sandy, and like that of Bischir, where Mr. B. Fraser found the disease so common, is much impregnated with salt. It is a bare, bleak, cold, and dry region, subject to high winds, but salubrious; and the inhabitants, who are remarkably athletic and muscular, live almost entirely on animal food, which is very abundant. The prevalence of goitre amongst these people is a remarkable fact, when contrasted with the authentic statements, that at a certain height above the Vallais, cretinism and goitre disappear; and the important observation of Marsden, that it is only known in Sumatra amongst the deep valleys subject to dense morning mists, and is cured by removal to the clear and pure air of the higher summits. Can this difference be accounted for by the peculiar geological structure of the Himalayas, in which limestone, in some places abounding in fossils, reaches above the highest inhabited spots; while in the Alps, especially in the Vallais and along the Arve, primary rocks form the sides of the higher valleys. ?[*] At Geneva, certain wells, now filled up, excavated in a recent *calcareous* sandstone, the waters of which abounded in sulphate of lime, were known to produce the disease in a few weeks or months in natives and in the French soldiers formerly quartered near them, who were exempt from it while they drank the nearly pure water of the lake. At Cluses, on the Arve, also 1,600 feet above the level of the sea, numerous cretins and goitrous persons are seen in the streets; lofty cliffs of limestone tower over the town, and through its caverns copious streams of water find a passage; higher in the valley, goitre becomes rare, and the granite of Mount Blanc, gneiss, and mica slate form the prevailing rocks. In ascending from the Vallais along the Val Valorsine we find similar rocks with clay-slate and the peculiar siliceous conglomerate called "poudingues de Valorsine," to succeed the calcareous strata of the Rhone. So it is in Westmoreland and the Grampians, where goitre is unknown. In the south of India, also, the Neilgherry mountains, rising 9,000 feet from the plains, and enjoying a climate like that of Europe, are exempt from the disease; as are the narrow and deep valleys of Coong, closed in on every side by precipitous mountains, from 3,000 to 5,000 feet high, covered with dense forest, or rising in magnificent precipices. In 1835-6, at Mercera, the capital of Coong, 119·14 inches of rain fell; the mean temperature was about 65°, the quantity of moisture 188, and the dew point 11·4. These countries are entirely composed of syenite and other primary rocks, and no lime is found in the Neilgherries.[†] It should however be observed, that bronchocele is extremely rare in the south of India even where limestone abounds; but this rock rarely attains a great elevation, is often impregnated with salt, and does not often afford water to the inhabitants. The case of a lady, however, has been mentioned to us, who twice contracted the disease in the hilly country of Travancore where fossiliferous rocks, probably tertiary, occur; and recovered on returning to Tranquebar on the eastern coast. The people of Tibet are said to suffer little from scrofula, and rachitis is nearly unknown in Nipaul, yet Mr. Bramley is of opinion, that the predisposing cause of the disease is often a want of tone in the living fibre, and some defect in the lymphatic system connected with a strumous habit; an opinion not very different from that still more strongly advocated by Mr. M'Clelland.

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[*] In the journal of a tour in the Himalayas published in the Asiatic Journal for August (p. 240), the inhabitants of the district of Gurhwal are stated not to suffer from goitre. The rocks seen were mica slate, quartz, and quartz-ore sandstone.

[†] The medical topography, meteorology, and geology of the Neilgherries, and also much information regarding Coong has been published by Dr. Baikie in a separate work, also in various papers by him, Dr. Benra, &c. in the Calcutta Transactions and in the Madras Quarterly Journal of Science. Meteorological science is cultivated with great success in India; yet, notwithstanding the opinions of Sir John Herschel and the British Association, such writers as Dr. Prout, &c., continue to quote foreign writers for uncertain accounts of the climate of these regions.—See Bridgewater Treatise on Chemistry and Meteorology.

Of the treatment of bronchocele we have space to say little. Mr. M'Clelland places in a proper light the distressing and even fatal consequences of the disease, and its injurious effects on the head, heart, and organs of voice and respiration; regarding which, strange misconceptions have found their way into various works. No mention is made of extirpation of the gland before it has attained a great size, as noticed by Mr. Fraser; an operation, the performance of which by Hindoos we might have been inclined to doubt, notwithstanding the recent accounts of their practice of lithotomy and extraction of the crystalline lens, had not Mr. Fraser kindly informed us, that he could not have been deceived in the information he received, and that the scars he saw could not have arisen from the actual cautery, even if that could be supposed capable of removing the disease. The use of protection and support to the throat is strongly recommended by Mr. Bramley, who, besides the notice of several cases successfully treated by this alone, adduces the singular fact of the Hill Coolies, who carry loads over the mountains in baskets attached to straps passing over the forehead, which causes an unusual development of the cutaneous muscles of the throat, never having the disease of any size. It deserves to be noticed, however, that this does not prevent altogether the enlargement of the gland, which spreads under the thickened muscular fibres. Stimulating frictions alone will occasionally succeed, and Dr. Campbell, Mr. Bramley's successor at Nipaul, found Croton oil very useful; but both pressure and friction have failed where iodine ointment has effected a rapid cure. The most common variety has the form of a shapeless protuberance consisting of a congeries of cells, occurring chiefly in women of feeble habit, and attaining a large size, although its growth is slow. Its cure is tedious and difficult; indeed Dr. Campbell asserts that he has seldom been able to remove it even by iodine, (*India Journal of Medicine*, Jan. 1837,) and we are sorry to observe that a similar statement is made by Dupuytren in his clinical lectures. Mr. Bramley was either more fortunate or more persevering, as he seldom failed to relieve the patient; and in one case not only did the enlargement of the gland disappear, but the neck became shriveled from the absorption of the subcutaneous adipose tissue. When the tumour is pulsatory, the whole of the vessels of the part partake of the increased action, and are remarkably increased in size; it has often an inflammatory character, and may arise from local injury, or the suppression of habitual discharges, but seldom occurs before the age of puberty. Depletory measures will often check this affection, and it yields very quickly to the internal use of iodine. In one case in which this remedy was given in the form of tincture and produced alarming constitutional disturbance, the tumour was discussed in six days, during two of which the medicine was omitted. When we see a disease like this, in which the vessels are not only excited to undue action, but absolutely enlarged, removed in so short a time by a remedy of such recent discovery, we cannot but indulge the best hopes of the progress of medicine. Dr. Prevost, of Geneva, extirpated one of the thyroid glands of a dog affected with goitre (in this animal the two lobes of the gland are distinct), and found the vessels greatly enlarged; and then administered iodine for the cure of the other; in a short time the swelling was removed, and on dissection, the enlarged vessels were found almost obliterated. The lymphatic variety is smooth, firm, and elastic, seldom attains a large size, and occurs most frequently in men. It is easily cured by the local application of iodine. Of 116 cases of all kinds treated by Mr. Bramley, only three or four occurred in which the patients were not either cured or so much relieved as to show that perseverance in the use of the remedy would have been successful: and these were old persons in whom the disease was of long standing, and in one of cartilaginous hardness. Mr. Bramley makes some good observations on the effects of the internal use of iodine on some constitutions, viz., vascular excitement, palpitation, anorexia, tremors, diarrhœa, difficult respiration, absorption of glandular structure, &c., proceeding to an alarming extent, and not easily counteracted, or their occurrence accounted for, as other patients take larger doses without their having any effect. We believe that aggravated examples of the same kind are often produced by the indiscriminate use of the preparations of iodine in this country for all obscure or obstinate affections; and we have witnessed its abuse so often, that we are little surprised that some of the most eminent physicians and surgeons now discourage its use, even in cases for which it certainly forms a most valuable remedy.

We have greatly trespassed on the limits that some of our readers may think should be assigned to such a subject, but we could not allow ourselves to doubt, that the minute and careful researches of our oriental brethren into a disease prevalent more or less in every region of the earth, evidently connected with local causes, and presenting peculiar facilities for the study of the action of external agents on the bodies of men and of animals exposed to them, would be interesting to many both in England and on the continent. It is not often that individuals so variously gifted for pursuing the enquiry, will visit those magnificent but often inaccessible regions; and Mr. M'Clelland has unfortunately chosen a form of publication which prevents his work ever reaching the medical libraries of this country. It is also true, that of the Transactions of the Calcutta Medical and Physical Society, only a few copies are to be found in England, and we believe not one in the whole of those parts of the continent where bronchocele is endemic. We lament that the Medical Society of Calcutta has substituted a medical journal of a miscellaneous character, placed entirely under the control of the secretaries, for the Transactions previously published under the authority of a "committee of papers;" as this plan must place still more out of the reach of their European brethren the labours of our countrymen in the east, by mixing selections, reviews, and matters of merely temporary interest with original communications.

Art. VI.

1. *Cases and Observations, illustrative of Renal Disease, accompanied with the Secretion of Albuminous Urine.* By Dr. Bright. (Guy's Hospital Reports, No. II., 1836.)

2. *De l'Albuminurie ou Hydropisie causée par Maladie des Reins, &c.* Par le Dr. Martin Solon, Médecin de l'Hôpital Beaujon, &c. *Paris*, 1838. 8vo, pp. 480. *Avec planches coloriées.*

On Albuminuria, or Dropsy, caused by diseased Kidney. By M. Solon, M.D.

3. *Traité des Maladies des Reins et des Altérations de la Secrétion Urinaire, &c., avec un Atlas in Folio.* Par P. Rayer, Médecin de l'Hôpital de la Charité, &c. Tome 1er, pp. 625. *Paris*, 1839.

Treatise on Diseases of the Kidneys and the Morbid States of the Urinary Secretion, &c. By P. Rayer, M.D., &c.

4. *On Granular Degeneration of the Kidneys, and its connexion with Dropsy, Inflammations, and other Diseases.* By Robert Christison, M.D. F.R.S.E., &c. *Edinburgh*, 1839. 8vo, pp. 288.

5. *Observations on Abdominal Tumours and Intumescence; illustrated by cases of Renal Disease.* By R. Bright, M.D. F.R.S., &c. (Guy's Hospital Reports, No. VIII., April, 1839.)

M. Martin Solon, author of one of these volumes, is physician to the Hospital Beaujon, at Paris, an institution, comparatively speaking, very rarely visited by Englishmen, on account of its remoteness from the School of Medicine, and other more important scenes of medical action. Hence it happens that its physician is much less known on this side the Channel than he deserves to be. Some experience in these matters enables us to affirm that there are no wards in the French hospitals where the student will enjoy a better opportunity of acquiring a sound acquaintance at once with diagnosis and the principles of rational treatment than in those of M. Solon. We do not mean to assert that there are not more profound masters of diagnosis in Paris, but we do maintain that in the union of the two arts—in the power of detecting and of combating disease—he is, generally speaking, superior to most of his countrymen, and entitled to a very high rank among contemporary practitioners. M. Solon's mind may be characterized as especially of a practical kind. The remaining writers, whose names are enumerated in our list, are sufficiently well known to fame to render the ceremony of introduction superfluous.

Among the works of which the titles appear at the head of this article, M. Rayer's is the only one purporting to embrace a complete view of the pathology of the kidney; we shall, therefore, take it as our groundwork in the present review, and notice, in due season, with the full detail their importance merits, the monographs devoted specially to the illustration of Bright's disease.

M. Rayer's volume opens with a very extensive section, entitled *prolegomena*, wherein, in addition to a general view of the diseases of those organs, we find the normal anatomy of the kidneys, the alterations they undergo from the progress of putrefaction, and the various changes of constitution to which the urine is subject, treated of with very considerable care. Through this part of the work we cannot, for very obvious reasons, follow our author closely, but we shall advert to such of its contents as appear likely to interest the English reader, from their novelty or practical bearing.

In imitation of his countrymen, who have made other viscera the subject of particular study, our author appears to have examined with much perseverance (and we by no means think such researches valueless, though it is certain they have not in other instances led to any useful conclusions,) the weight of the healthy kidney at different ages. The results of his enquiries are laid down in eight tables; and, though implicit confidence in their correctness may be reasonably refused, on the ground of the limited number of individual cases furnishing them, yet the following seem worthy of attention. We learn, for example, that the weight of the kidneys is never accurately the same in different subjects of the same age; that the development of those organs, though it is progressive in new-born infants, is in them subject to very remarkable individual varieties; that as a result of the foregoing propositions it is hardly possible to feel certain of the existence of renal atrophy or hypertrophy, unless in cases of very notable deviation from the normal standard; that the kidneys are lighter in females than in males; that the left is generally heavier than the right; and that in old age both organs ordinarily weigh as much as in adult age. The mean weight in thirty males, from æt. 16 to 76, was, for the right organ, ℥iv. ʒiij.; for the left, ℥ivss.; this weight is not always proportional to the size of the organs; in other words, the density of their substance is variable.

In the minute description of the anatomy of the kidneys, we observe some statements valuable from the light they throw on the pathology of certain renal affections, concerning which our knowledge has hitherto been exceedingly obscure. These anatomical details will be most usefully referred to in our account of the diseases in which the tissues described are concerned; but we may meanwhile instance the discovery of a distinct cellular membrane, between the fibrous capsule and substance of the kidney. The existence of this membrane explains the tendency to suppuration now and then observed in the locality it occupies, as also the pretty frequent occurrence of hemorrhage in the same situation, and the occasional investiture of the organs by a very thick and firm lamella—the membrane in question in a state of hypertrophy—independently of the true capsule.

The very moderate degree of sensibility possessed by the kidneys in health is familiarly known, and easily ascertainable by pressure, percussion, and other similar means. But the point has been further demonstrated in the lower animals by a number of experiments originally executed by M. Comhaire, in 1803, and lately repeated, somewhat needlessly, as it appears to us, by M. Rayer and his pupils. The kidneys were drawn out from the abdomen by these experimenters, manipulated, cut, torn, and burned with a variety of caustics, without the subjects operated on evincing signs of suffering. The practical inference from these facts is that very trifling pain or tenderness on pressure in the region of the kidney is a point of considerable consequence, and one which the medical observer should not rest satisfied without tracing to its cause; they also explain how it is that nephritis is frequently a latent disease, and in part account for the prevalent belief as to the rarity of renal inflammations.

The researches of Müller, of Straus Durckheim, and of others, on the conformation and structure of the urinary organs of animals of simple organization appear to M. Rayer unlikely to elucidate, in any marked degree, the mode of production of urinary disorders and renal lesions in the human subject; nevertheless, he conceives that, inasmuch as they have proved the fact of the secretion of urine by organs of extremely different appearance, they render intelligible the persistence of their function in kidneys that have undergone material and extensive disorganization. Now we consider it worth our while, as this view is founded on analogical reasoning, the most delusive as well as the most attractive from its simplicity of the processes whereby conclusions are arrived at, to show that it is fallacious. In truth, if such be the explanation of the fact adverted to, the inference is just, that extensive lesion of organs, possessing a closely similar structure throughout the animal series, ought not to be found to coexist in the human being with continued manifestation of activity on their parts. Now is the fact so? Clearly it is not. Do we not find that the liver continues to secrete bile when a very minute portion of it remains in a healthy condition; that the lungs exercise their wonted influence on the blood traversing them (in an imperfect manner, it is true,) when they are almost totally disorganized; that the power of thought and of reason is occasionally manifested by brains of which a large proportion is unsound? Nay, more; is it not an incontestable fact that individuals may live—and live with a certain amount of enjoyment after every important organ in their frame has become profoundly disorganized? M. Rayer's erroneous notion is an effect of the doctrine so generally held by French pathologists, that functional derangement is uniformly proportional to the organic lesions cognizable by our senses,—a doctrine which has exercised and still continues to exercise a most baneful influence on their bed-side practice.

The importance of a thorough acquaintance with the changes induced in our tissues by the progress of putrefaction is now fully acknowledged by morbid anatomists; nevertheless, only a one-sided view of the subject has as yet been taken. Enquiry has been limited to pseudo-morbid appearances, and to the signs distinguishing these from conditions originating in disease; nowhere do we find a serious and methodized attempt made (incidental allusions to them are occasionally met with) to appreciate the *pseudo-healthy* appearances, if we may be allowed the expression, which the cessation of vital action sometimes induces in really diseased parts. Yet this is a field which may be most profitably cultivated by those desirous of giving the study of morbid anatomy all the precision of which it is capable. Our author does not swerve from the ordinary plan in his general view of putrefactive changes in the kidney, and enumerates alteration of colour, separation of the fibrous capsule, emphysema, and the effects of desiccation and of maceration as the chief points for consideration.

The phenomenon of hypostatic engorgement of the cadaveric species is of such regular occurrence in the kidneys, that, according to M. Rayer, whenever their anterior is more injected than their posterior portion, in subjects who have been laid, as is usual, on the back, this condition is to be ascribed to morbid action. One of the most remarkable effects of imbibition of blood, when considerable and affecting *both* substances, is, we are told, the assimilation of their respective colours, a condition which has been very frequently mistaken for the effect of disease. The separation of the fibrous capsule is, in cases where abdominal putrefaction advances with rapidity, a very usual phenomenon, and is directly produced by the softening of the cellular membrane already alluded to. This separation of the capsule does not occur spontaneously in kidneys affected with chronic inflammation, in consequence of the thickening and abnormal adhesion of the proper cellular membrane to its internal surface.

There are some peculiarities in the cadaveric emphysema of the kidney. That it should rarely exist if there be little blood in the vessels, and not be developed at all when they are almost free from that fluid, is sufficiently intelligible; but why it should be absent, as M. Rayer avers is the fact in some cases, when the organs are perfectly rotten, disengaging a strong odour, of greenish hue externally, while both substances are saturated with blood, is not easily explicable.

We are given no original instruction on the method of distinguishing putrefactive from morbid softening; and the description of the changes to which the *colour* of the organs is liable increases the perplexity one is likely to feel pretty often in deciding on its causation. So rapid are the alterations of tint, that M. Rayer has seen an artist sadly puzzled to catch the original colour. The only way of avoiding effectually this source of error consists in examining and noting down the colour of the surface the moment the fibrous capsule is removed.

Maceration has, as might be foreseen, the effect of rendering the peculiarities of some morbid conditions unusually distinct, those of others more obscure, and of depriving a third set of all their characteristic qualities. Among those rendered more apt for examination are the granulations in the affection described by Dr. Bright, as that observer had himself stated. The impregnation of the surrounding cortical substance with water renders it semi-transparent, and so throws out the dull, milky hue of the granulations. According to M. Rayer, those bodies are usually more distinctly visible at the inferior part of the right kidney than elsewhere; this he attributes to putrefaction commencing earliest in this portion of the organs.

M. Rayer's classification of renal diseases is comprehensive, and gives fair evidence of his possessing a spirit of order and method. There is no possible morbid condition to which the kidneys are liable that does not here find its particular place; but at the same time we must remark that it can hardly be considered correct to class congenital anomalies and mechanical injuries as diseases; nor is it easy to understand why certain abnormal states should occupy the ground they do. Why, for example, hyperæmia and anæmia should follow not precede inflammations, or why gangrene should be separated widely by totally dissimilar conditions from those same inflammations. Possibly the concluding volume may explain, though it will be difficult for it to justify this departure from natural arrangement. We subjoin a table of the principal heads, omitting all their numerous subdivisions.

A. Diseases of the Kidneys.

1. Wounds, Contusions, Lacerations.
2. Inflammations
   - Nephritis, Pyelitis, Pyelonephritis, Perinephritis,
   - Renal Fistulæ.
3. Hemorrhages.—Idiopathic, Symptomatic, Endemic.
4. Hyperæmia. Anæmia.
5. Hypertrophy. Atrophy.
6. Tumours formed by retention of urine
   - Hydronephrosis,
   - Kysts.
7. Diseases of elementary tissues
   - Veins,
   - Arteries,
   - Lymphatic glands and vessels,
   - Nerves.
8. Morbid tissues and products
   - Homologous,
   - Heterologous.
9. Foreign bodies
   - Animate,
   - Inanimate.
10. Gangrene.
11. Anomalies in
    - Number,
    - Situation,
    - Configuration.

B. Special alterations of the urinary secretion.

1. Quantity
   - Suspension, Diminution, Augmentation,
   - Diminution of one or several natural principles,
   - Increase of some natural principles.
2. Composition
   - Presence of adventitious principles, whether of morbid formation or introduced from without.
3. Mode of excretion. Uroplania.

The same arrangement is followed for the morbid states of the ureters and supra-renal capsules as for the kidneys themselves.

A glance at this table will suffice to show the practical sagacity evinced by M. Rayer in abstaining from a regular system of generalizations on a set of diseases so utterly dissimilar in cause, nature, course, and termination. The few he indulges in, especially those regarding the relations of renal affections to those of other organs, are interesting and apparently well founded; but more important matter for notice is elsewhere furnished in abundance.

In proceeding to examine a patient whose urinary organs are suspected to be the seat of disease, inspection, manual examination, and percussion of the renal regions should first be had recourse to; of this all will admit the truth; but so important is it for practitioners to act on the admission, that we may be permitted to state in a few words some of the diagnostic information derivable from those methods of exploration. Thus, for example, in certain cases of cancerous diseases of the kidney, of pyelitis (inflammation of the pelvis and calices), of hydronephrosis (tumour formed by retention of urine), &c., those regions become manifestly prominent or otherwise deformed; whether the altered shape really depend on a renal affection, or, as may with almost equal *primâ facie* probability be the case, on an intestinal, hepatic, splenic, or ovarian tumour, or subperitoneal abscess, &c., must be determined by other means, among which manual examination first presents itself. By this the degree of sensibility of the kidneys, and the extent, form, direction, mobility, and resistance of visible intumescences may be ascertained. The importance of any notable tenderness has already been adverted to; we may be guided in regarding such tenderness as dependent on the kidney in contradistinction to the surrounding organs, if it be most distinctly marked posteriorly, "as pain in the corresponding part of the peritoneum, large intestine, liver, or spleen is in general most felt by the patient when pressed or percussed on the antero-lateral part of the flanks." Percussion will further aid in making out the origin of tumours in the lumbar regions; but the dull sound elicited by that process may, it must not be forgotten, be due to enlargement of other solid organs; the commemorative symptoms, as well as those present at the time of examination, must therefore be diligently scrutinized. It is needless to dwell on the importance of a careful and thorough enquiry into the state of the urine; the normal or abnormal characters of that excretion may in certain cases at once settle the question of the absence or presence of serious disease in the kidney, a fact with which the labours of Prout and Bostock have long familiarized English practitioners. But, as our author very justly remarks, the whole process of examination we have thus briefly noticed will not, even if extended to the ureters and bladder, prostate and urethra, and followed up by an enquiry into the causes of the affection, be sufficient, in all cases, for our purposes of diagnosis; the state of the constitution must be well ascertained before we can safely determine that point, or the mode of treatment advisable. Two instances will be enough to show the practical truth of this statement: in many cases of nephritic colic, and lithic acid deposition, it is the hereditary or acquired arthritic diathesis that the physician must labour to remove; the mere exhibition of alkalies would, in such cases, relieve a symptom, but nothing more; again, in cancerous or tuberculous diseases of the kidney, it is manifest that our therapeutic measures must be directed against the vitiated state of constitution giving rise to those disorders.

The next 200 pages of M. Rayer's volume are devoted to general considerations on the urine in the state of health; on the different characters it assumes from differences in respect of diet, age, &c.; on the modes of examining it, the modifications of its natural constituents, as well as the accidental introduction of foreign principles; on the clouds, sediments, and cremors occurring in it; on the phenomena of its putrefaction, and the relations subsisting between its morbid states and those of the blood and other fluids. We are bound to express our most unqualified approbation of this part of the work; it gives evidence of the soundest and most extensive erudition, as well as of a degree of practical familiarity with the subject treated, not easily to be surpassed. It is pleasing to observe, from its contents, that the labours of our own countrymen have very clearly had the most important share in giving its existing perfection to urinary pathology; while it is not a little creditable to M. Rayer, living as he does in a country where the Practical stands extremely low in general estimation, to have comprehended the sterling excellence of our contributions to useful medicine.

M. Rayer shows that, historically considered, the progress of knowledge respecting the urine may be divided into three periods: the medical, the chemical, and the medico-chemical. The first commencing with

Hippocrates, extended to the time of Van Helmont, and was marked by the exclusive attention given to the outward appearances of the urine. The second, of which M. Rayer is probably correct in fixing the origin with Van Helmont, has been chiefly illustrated by the analytic labours of Bellini, Willis, Boerhaave, Rouelle, Scheele, Wollaston, Berzelius, Marcet, &c., and cannot be said to have yet terminated. The medico-chemists made their first appearance in the person of Cruickshank, who set the example of studying the urine especially in its relations to morbid states of the system, basing his researches on accurate chemical analysis; he has been worthily followed by Nysten, Prout, Brande, Henry, Bostock, Christison, Rees, and many others. Further, a new or rather a revived method, for it appears to have originated with Leeuwenhoeck, of studying urinary deposits, namely, with the microscope, has been of late pressed into the service by M. Donné; and, judging from the valuable results already obtained by that gentleman, and MM. Vigla and Quevenne, the microscopical is likely to exercise quite as important an influence on urinary pathology as any other mode of investigation.

The results of M. Rayer's experiments on the urine of infants, performed in conjunction with the distinguished chemist Guibourt, do not accord with the opinions expressed on the subject by a variety of writers. Thus, instead of finding the urine of infants muddy at the moment of emission, they distinctly ascertained, as they state, that it is then perfectly transparent, and further, that it does not become turbid more rapidly than that of adults. The urine of infants at the breast they found to be neutral, and without urinous smell when voided, though it evolves that peculiar odour during the process of evaporation; no appreciable formation of nitrate of urea takes place when its extract is treated with nitric acid. The urine of a healthy infant, three years old, furnished, on the contrary, a full proportion of urea, but none of the benzoic acid, which almost all chemical writers, on the authority of Fourcroy, enumerate among the constituents of the renal secretion in children. We think it right to produce this statement of our author, especially as it is confirmative of Dr. Prout's opinion on the point. M. Rayer also declares his disbelief of the current notion that an excess of phosphate of lime is characteristic of the urine of persons advanced in years; his incredulity seems to rest on a very limited number of experiments.

The subject of specific gravity is examined at some length by our author;—without containing anything positively new, his summary embraces all the facts regarding it, which are firmly enough established to be relied on in practice. The different degrees of confidence with which this character of the urine is regarded by different observers as a diagnostic sign is extremely remarkable; while some reject it altogether as the most deceptive of guides, others look upon it as furnishing most valuable indications. Those who adopt the former mode of thinking (instance M. F. d'Arcet who has made a series of experiments on the subject, whence it would result that the specific gravity of normal urine varies from 1,001 to 1,060)* have evidently done so out of mere disgust at the difficulties attending the proper appreciation of the various circumstances capable of influencing the character in question; the holders of the latter have unwisely generalized the results of a few favorable cases. The truth is, that diseased conditions do exist, but they are limited in number, wherein it is possible to obtain useful information from the variations in density of the urine. The first point to be ascertained is the average healthy specific gravity: now the multitudes of circumstances influencing this, such as temperature, the quantity and quality of food and drink, the state of the bowels and of the cutaneous and pulmonary exhalations, the period after exercise and meals at which the urine is voided, the length of time it has remained in the bladder, as well as that which has elapsed between the emission and examination, the hydrometer employed, &c. render it doubtful, whether any one will be found willing to undergo the vast labour such an investigation would require. Meanwhile, whose approximate estimate is to be regarded as the most accurate? Are we to accept as correct the calculation of Dr. Prout, which fixes the mean density at from 1,010 to 1,015; or trust to that of M. Rayer, who gives us the number 1,018, or to Drs. Bostock and Martin Solon, who consider 1,020 nearer the real mean; or to adopt the opinion of Drs. Gregory and Christison, who assert that it is so high as 1,024—1,025? Trifling as these differences may appear, they are capable of causing real discrepancy of opinion as to the morbid or normal condition in this respect of certain specimens of urine: a density of 1,020 will show that fluid to be normal in the patients of Dr. Bostock; from five to ten degrees above the healthy standard in those who consult Dr. Prout, and five or six below it in individuals who chance to be under the care of Dr. Christison. Now, in illustration of this, we may state that the last-named observer and M. Rayer do not accurately agree respecting the condition of the density of the urine in the early stage of Bright's disease, Dr. Christison declaring that it is moderately lower, the French writer that it is occasionally superior—as is the case in the only examples given in his accompanying table of densities—to the healthy mean. This want of accordance arises in some degree at least from their different notions respecting that mean.

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* L'Expérience, No. 55, Aug. 1838. This paper deserves to be read.

The sources of inaccuracy increase materially in various conditions of disease; the density of the fluid undergoes marked changes, which we know not how to explain in such cases. An interesting example of this is given by M. Rayer, in the case of an individual affected with chronic pyelitis; the urine of this man marked 1,008·4 and 1,016·4 at eight o'clock in the morning on two consecutive days, though no change had taken place in the conditions known to affect the specific gravity of the fluid. Again it varied from 1,007 to 1,018 from one day to another in a patient of M. Solon affected with Bright's disease; no cause for this peculiarity was discovered.

From these considerations it results that, in order to possess diagnostic value, the variations in specific gravity must be at once well marked and persistent; in diabetes and in the advanced stages of Bright's disease, where all are agreed as to the real utility of this sign, it is thus characterized. M. Rayer gives a table of the densities observed in a variety of local and general, trifling and serious disorders; we confess we do not see its value, for it expresses the mean results of *five* examinations only in each disease.

We shall not pursue this subject further for the present than to state that the specific gravity of the urine may be lower than that of the liquids used as drink. Thus M. Rayer informs us, that one of his patients, who drank a bottle of artificial Vichy waters every morning, frequently voided transparent urine in his presence, of lower density, even when cool, than that mineral solution.

Some points in M. Rayer's section devoted to the history of that very important principle, urea, deserve notice. Amongst these is the method—valuable from the facility of its employment—of estimating roughly the proportion in which it is present in any given urine: a drop of the fluid is poured on a plate of glass; in warm weather this small quantity is in a few minutes sufficiently condensed by spontaneous evaporation (in the winter the aid of gentle heat is required), to allow of the formation of a magma or crystalline mass on the addition of a similar quantity of nitric acid. Examined under the miscroscope this is found to be composed of brilliant acicular crystals of nitrate of urea.

Respecting the variations to which the proportion of urea is liable in disease, our author's experience has convinced him of the rarity of cases attended with an excessive formation of that principle, and, on the contrary, satisfactorily proved the frequency of morbid states in which its proportion falls below the healthy standard; the common occurrence of the latter he is probably right in ascribing as much to the abstinence and lowering regimen enforced for their cure, as to the direct influence of the diseases themselves. But there is another cause of diminution in the proportion of urea discoverable by analysis, which has only been of late years suspected to exist; we allude to the decomposition presumed to be undergone by the principle in question under certain conditions. The notion of its possibility seems to have originated with Wöhler, who remarks that "the transformation of urea into cyanic acid and carbonate of ammonia, and that of uric acid into urea and cyanic acid, may at a future period become important in a physiological point of view, and throw light on the origin of certain diseases and abnormal sediments of the urine."* The plausibility of this view is recognized by M. Rayer, for he inclines to attribute the deficiency of urea and lactic acid, (not uric, as erroneously printed in his book,) detected by MM. Cap and Henry† in a specimen of viscous urine, to the occurrence of such decomposition, on the ground that viscidity is often the result of the action of ammonia on mucus and pus, and may have been so induced in the case adverted to. Further on, he speaks more unreservedly; and affirms that in the majority of cases of inflammation of the mucous membrane of the pelvis and bladder, consequent on retention of urine in those cavities, the urea undergoes decomposition, and that the carbonate of ammonia resulting therefrom must be taken into account in estimating its primitive proportion. He even considers it probable that, in a certain share of the cases in which the urine possesses an alkaline reaction, independently of the action of medicines, immediately on being voided, the quantity of urea obtained by analysis will be less than that primitively secreted. Nevertheless, he hardly appears firmly persuaded that alkalescence is in such cases induced subsequently to the act of secretion. He elsewhere, indeed, (in alluding to the fact ascertained by M. Guibourt, that urine which has remained in the bladder for a certain period, and proves alkalescent at the moment of emission, contains proportionally less urea than the healthy fluid,) remarks that, "as urine which is not exposed to the air, does not putrefy even at a temperature of 32° R., it is probable that that fluid, when long retained and containing carbonate of ammonia, owes this peculiarity to an act of secretion rather than to putrefaction." But the experiments on which this statement is founded, were, it must be remembered, performed on healthy urine; whereas, that spoken of by M. Guibourt had been retained in the bladder with blood, mucus, pus, or other principles acting as alkalies, and capable of promoting the decomposition of urea: the two kinds may be demonstrated to comport themselves differently, when placed in hermetically sealed vessels. On the other hand, a certain number of facts have been observed, tending to show that urea may be converted into carbonate of ammonia during the act of secretion; or, if this mode of expression be objectionable, that the latter may be elaborated by the kidney instead of the azotised principle. The strongest fact of this kind—its conclusiveness, as admitted by Drs. Graves and Willis, may be doubted—is one published some years past by the former of these physicians. The patient in this case was made to empty his bladder thoroughly, and half an hour after to discharge the urine that had accumulated in the interim; this last was instantly analysed and found to contain an abundance of carbonate of ammonia, but not a trace of urea.

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* Journal de Pharmacie, vol. xvi. p. 300.

† On detecting the deficiency of these principles, it occurred to the above-named experimenters, that an artificial combination of them might be administered advantageously in certain conditions of the urinary passages. Accordingly, they set about the manufacture of the salt, and succeeded in procuring it by decomposing oxalate of urea with lactate of lime: they promise to make public the result of its exhibition. It is right to mention that these chemists suppose urea to exist in the urine as a lactate, and that to its state of combination is due the difficulty of separating it from the fluid without the aid of nitric acid. They represent that when they had removed the free lactic acid from the urine with an excess of hydrate of zinc, they were enabled easily to obtain natural lactate of urea, crystallized, and perfectly identical with specimens of the salt directly prepared.—For further details we must refer to *L'Expérience*, No. 39, vol. i. p. 621.

While on this subject, we may state that M. Rayer completely denies the correctness of the common notion as to the alkalescence of the urine in typhoid fevers. In *fifty* cases examined at the most marked periods of the disease, he only found it alkaline *twice;* even when retention of the contents of the bladder occurs in this affection, he affirms they are almost invariably acid. How are we to reconcile these very confident declarations with general belief,—how especially with Dr. Willis's experience, which authorizes him in sometimes basing a flattering prognosis on returning acidity, even in cases where nothing else betokens improvement?*

We shall scarcely be accused of travelling out of the region of purely practical disquisition, if, with M. Rayer, we enter slightly into the question of the cause of the state of solution in which lithic acid exists in healthy urine. Various chemists have each offered a different explanation of the well-known difficulty attending its dissolved condition therein: Dr. Prout's theory is admitted to be the most plausible and ingenious yet advanced.

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* On Urinary Diseases, p. 128.

This gentleman, as is well known, conceives that the acid in question is not present in a free state, but in combination with ammonia in such proportion as to form a superlithate of that base. Now, as that salt possesses a comparatively high share of solubility, if the notion of its existence be correct, the solution of the lithic acid ceases to be a mystery. It is needless to recapitulate Dr. Prout's well-known arguments, but we shall lay before our readers such evidence against and in favour of the combined condition of the acid, as more recent enquirers have supplied.

M. Donné, whose interesting contributions to microscopical studies have more than once been noticed in this Journal, espouses the opinion of Dr. Prout. He maintains that lithic acid is always found in the urine crystallized, and not in an amorphous powder. On the other hand, M. Quevenne, while he admits the possibility of their occasionally containing a certain share of urate of ammonia, affirms that all the amorphous sediments of acid urine observed by him, whether they were gray, pink, or lateritious, were essentially composed of lithic acid in union with animal matter; and that, when recent, they gave but very slight indications of the presence of ammonia. "No doubt," he adds, "if they are examined some time subsequent to emission, the alkali may be very distinctly perceptible, and may even be found to have given rise to the formation of blackish globules consisting of lithate of ammonia." These globules occasionally appear a few hours after the urine has been discharged, a circumstance which, as M. Quevenne admits, justifies a suspicion of a considerable quantity of lithic acid having been in combination with bases at the moment of emission.

M. Donné adduces the two following strong facts in favour of Dr. Prout's theory: first, the sediment of acid urine redissolves, exactly as lithate of ammonia is known to do, if the fluid be boiled, whereas lithic acid once crystallized cannot be so dissolved; secondly, the action of a weak acid, such as the nitric diluted with from eight to ten times its weight of water, has no effect on lithic acid, but decomposes the amorphous sediments and transforms them into perfect crystals of that acid. M. Quevenne, however, is not without his answer to these allegations; both facts, according to him, admit of a different explanation from that proposed by his opponent. Suppose, he argues, the amorphous powder to be composed of lithic acid in the condition of a hydrate, and there will be no greater difficulty in accounting for its solution by ebullition than for that of a lithate; and again, whether the acid be combined with ammonia, animal matter, or water, the production of crystals of lithic acid, by the action of a weak agent of the same class, is equally intelligible. M. Rayer professes himself, as we think, on very just grounds, best satisfied with the reasoning of M. Donné.

Passing over a variety of useful matter, as our limited space obliges us to do, we arrive at the section treating of the organic products accidently introduced into the urine. It results from recent microscopical investigations—the discovery originated, we believe, with M. Raspail—that the genito-urinary epithelium, as well as that of other mucous membranes undergoes continual desquamation in the natural state, and that in certain diseased conditions, this physiological process acquires morbid activity. Examined under the microscope, the resulting scales are found to be of the extremest delicacy, and transparent, except where marked by fine lines; some points appear to correspond to minute openings; their form is irregular, their border ragged. In the female, the use of the catheter will decide whether they come from the pudendum or the proper urinary passages: that their separation does really take place in the most remote parts of those passages is proved by their being discoverable in the urine found in the calices and pelvis after death.

Among the adventitious matters appearing in the urine, the blood in substance, or its constituent principles individually, hold the first rank in point of importance. The researches which have of late years been made in this country, and followed up with vigour in France, further show that not one of these claims so large a share of the practitioner's attention as *albumen*. Our readers will therefore not be surprised, especially when they consider its comparative novelty, if we enter at some length into the subject of albuminous impregnation of the urine. Our remarks shall for the present be strictly of a general kind.

The presence of albumen in the renal excretion may be established by the majority of reagents which serve for its detection in pure water; but the evidence of many of these cannot in the case of the urine always be trusted to. The combination of three characters, coagulability by heat and by nitric acid, and non-precipitation by acetic acid, affords and alone affords incontestable evidence of the presence of albumen. The necessity of the coexistence of these characters is frequently overlooked, and deserves illustration at our hands. Urine, containing milk or casein, whether naturally or by artificial introduction, (a trick which, in these days of extraordinary attention to urinary pathology, it is reasonable to expect may soon become a favorite resort of *malingerers**) will coagulate by heat, and precipitate with nitric acid; but, unlike albuminous urine, will also coagulate on the addition of acetic acid. Again, when albuminous urine is alkaline, no matter how it has become so, whether by long standing or otherwise (hence the importance of making our analysis as soon as possible after emission), it does not ordinarily lose its transparence under the action of heat, unless the quantity of the foreign principle present be very considerable, and even in this case simply assumes a milky tint. The addition of a certain quantity of nitric acid will cause instantaneous coagulation; but, according to M. Rayer, it is a mistake to suppose that the presence of alkalies will always cease to influence the coagulation of the albumen by heat the moment the fluid containing them is *neutralized* with an acid. He declares that he has ascertained that nitric acid, in the proportion of a drop to a drachm, will not ensure turbidity under ebullition: but that, on the other hand, precipitation takes place in alkalescent albuminous urine with or without the aid of heat, if the quantity of acid be increased to "several drops." Curious to discover the cause of the non-coagulation under these circumstances, MM. Rayer and Guibourt made some experiments on white of egg dissolved in water. Their researches have not been successful in this point of view, but have so far established the conditions under which it occurs, as to show that nitric acid in small quantities, (i.e. from $\frac{1}{72}$ to $\frac{1}{100}$ of the tested fluid) acetic in large, and phosphoric acid in small or large proportion, deprive albumen of the property of being coagulable by caloric, but not of being precipitable by nitric acid. It follows, according to M. Rayer, as a direct consequence of these experiments, that, as specimens of urine containing at once albumen and a little phosphoric acid may be met with, nitric acid is a more certain and generally applicable test for that principle than heat. Dr. Christison's experience does not quite accord with that of M. Rayer on this point, for he has "found that sometimes even nitric acid added in excess did not separate albumen which had been present in large quantity." The experiments of the French observers were not conducted on perfectly correct principles; Dr. Wells long since showed that the effects of reagents are not the same on albumen contained in water and in urine.

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* It has already been had recourse to by at least one of M. Rayer's patients.

It must also be remembered that, if alkaline urine be rendered turbid by heat, the loss of transparence is usually due to the precipitation of phosphates, as is proved by its complete restoration on the addition of nitric acid. For this fact we are, we believe, indebted to Mr. Rees.

If the urine be muddy, from the deposition of lithic acid and the lithate of ammonia, heat will, according to Dr. Christison, remove the turbidity by dissolving those compounds: we doubt this as respects the first of them. Mucus, if present, will not disappear under the action of heat, and, as it causes muddiness, should be removed by filtration before the test is employed.

On the other hand, precipitation by nitric acid alone will not prove the matter thrown down to consist of albumen; such precipitate may be composed of lithic acid or lithate of ammonia. Here has, doubtless, been a source of very frequent error, and, according to M. Rayer, the additional one has more than once been committed, in consequence of the precipitate of lithate of ammonia being soluble in an excess of acid, of attributing that character to albuminous coagula. All this we are somewhat at a loss to reconcile with the declared superiority of nitric acid as a test; but, however this may be, M. Solon has made some experiments which show that M. Rayer's denial of the possible redissolution of albumen in an excess of precipitant requires qualification. He found that the coagulum, obtained from an artificial mixture of two drops of serum and one ounce of normal urine, increased in quantity, until fifteen or twenty drops of nitric acid had been added to it, but disappeared completely when the dose of reagent had amounted to thirty drops. But this redissolution, even where the proportion of albumen is very small, requires, at least, twenty-four hours for its accomplishment; hence, in all probability, the difference of opinion of the two observers. We have repeated M. Solon's experiment, and vouch for the correctness of the result announced: the same effect takes place in the coagulable urine of Bright's disease.*

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* We shall employ this phrase throughout the present article to designate the morbid state of the kidneys, made known to the profession by Dr. Bright, because it involves no erroneous or disputable position, and because we do not share Dr. Christison's polite scruples (Preface, p. xiii.) as to the propriety of its adoption. *Granular degeneration of the kidneys* is so utterly incorrect a term, that we cannot understand the latter gentleman's employment of it, more especially, as his doing so tends to keep alive the error into which he presumes, with what justness we shall presently see, Professor Forget had fallen, that of supposing granular deposition an essential part of the renal lesion. M. Solon's neologism, *albuminuria*, is inadmissible as a title for the disease, because, literally interpreted, it is applicable to a symptom only, and this one which occurs in various conditions; but the laxity of that gentleman's use of the word is really sur-

The matter thrown down by nitric acid may at once consist of the proximate principle in question, uric acid and urate of ammonia: microscopic inspection will, in such cases, prevent error, by disclosing the lamellar, corrugated, and peculiar appearance of albumen, crystals of lithic acid, and an amorphous powder, convertible into similar crystals by nitric acid (lithate of ammonia). The proportions of the three ingredients may be ascertained by acetic acid and ebullition. Again, if albuminous urine be red-coloured, from the presence of hæmatosine and globules of the blood, nitric acid, in a great measure, discolours it by precipitating all the foreign principles together; the microscope will detect the globules, either in the urine or imprisoned in the flakes of albumen.

The different aspects of the coagulum depend, in a great measure, on the quantity of albumen present, and this is exceedingly variable. Barely discernible by the usual tests in some cases, in others it has been known to constitute $\frac{27}{1000}$ by weight of the mass of urine; where the proportion is so low as one part in a thousand, ebullition and evaporation should be prolonged for a considerable time, a precaution that has, we fear, frequently been neglected. If it be particularly desirable to learn the precise quantity of albumen present, this may be effectually done by taking the coagulum, obtained by heat, washing it in alcohol, drying and weighing it, and then subtracting the amount from the total weight of the urine employed. Such an accurate estimate as this, however, is not absolutely called for in practice; a simple statement of the quantity of coagulum formed, may be accepted as a satisfactory substitute, and we so perfectly agree with Dr. Christison, as to the scientific and practical benefits which must follow, were practitioners "to employ some common nomenclature for the different degrees of coagulability," that we reproduce the scale he has proposed, in the hope of its being generally adopted, until a better be brought forward.

"1. *Gelatinous by heat.* 2. *Very strongly coagulable,* where a precipitate distinctly separates by heat, and yet occupies, in twenty-four hours, the whole, or nearly the whole, fluid. 3. *Strongly coagulable,* where the precipitate, in twenty-four hours, occupies half the volume of the fluid. 4. *Moderately coagulable,* where it occupies a fourth of the fluid. 5. *Slightly coagulable,* where it occupies an eighth of the fluid. 6. *Feebly coagulable,* where it occupies less than an eighth of the fluid. 7. *Hazy by heat,* where the urine becomes cloudy, but does not form visible flakes a few seconds after being boiled." (p. 44.)

It seems unnecessary to dwell at any length upon the numerous other reagents proposed for the detection of this principle; as they are all confessedly inferior in accuracy, and not superior in facility of employment to those described, it will be enough to enumerate them, and refer the curious to the original volumes for further detail. They are tannin, creasote, alcohol, ferrocyanate of potass and acetic acid, bichloride of

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prising; it is successively made to mean *dropsy caused by disease of the kidneys* (Title-page, &c.); *dropsy with albuminous urine, caused by disease of the kidneys* (passim); *dropsy with albuminous urine, without renal lesion* (p. 109, &c.); and simply *albuminous urine* (passim). In the latter, its true acceptation, we shall ourselves employ it. *Albuminous nephritis,* the name proposed by M. Rayer, may be founded on correct views of the nature of the disease, but we are unwilling to avail ourselves of it, because we are not, at present, entitled to assume, as certain, that an inflammatory condition is invariably the starting point of the disease, though we admit the extreme probability of the fact; besides albuminous urine occurs in different varieties of renal inflammation.

mercury, and alum. A character of albuminous urine, which may sometimes be usefully consulted, is its frothing considerably more than the healthy fluid, when blown into or even shaken; the bubbles produced remain unusually long without bursting.

Albuminous urines differ from each other in a variety of respects. If acid, when voided, and containing but little fat and mucus, they may be perfectly transparent: the fluid may, in other instances, (as is frequently the case, for example, in the early stage of Bright's disease,) be of a deep red tint, from the presence of the colouring matter of the blood, or assume a straw-coloured or pale greenish-yellow tinge, like non-clarified whey, as in the more advanced periods of the same affection. Again, the quantity of fluid discharged from the bladder is very variable: in Bright's disease alone it ranges between a quantity superior to the normal standard of excretion and two or three ounces. Dr. Wells states, but (as he adds, with most commendable respect for accuracy,) "on the authority of patients and nurses," that dropsical patients sometimes void six, eight, or ten pints of serous urine daily. But the most important point of difference in these abnormal urines respects their remaining normal or adventitious constituents. The proportion of urea and salts may be increased, natural or diminished. M. Rayer instances purpura as an affection in which the first condition is observed, and the advanced stage of Bright's disease as furnishing an illustration of the last. With respect to the detection of urea in albuminous urine, the process of Mr. M'Gregor,* for the discovery of such portion of it as may be precipitated in the substance of the coagulum, is sufficiently accurate for all practical purposes; but the residual urine must also be carefully analyzed, for, as MM. Rayer and Guibourt have found, the quantity of urea therein contained may be "more than one hundred times greater than that obtained by the process in question from the coagulum of the same specimen of urine."

In the albuminous urine of acute affections there is an abundant precipitation of lithate of ammonia: in chylous urine the albumen is associated with fatty matter: in others, mixed with pus, the colouring materials of the blood, &c. Hence arise numerous varieties in point of specific gravity; M. Rayer has found it so high as 1,032, in the commencing stage of Bright's disease; Professor Christison so low as 1,004, or probably even 1,001·5, in the advanced period of the same affection.

Dr. Wells, the only person apparently who has examined this point, generally found albuminous urine to possess the saline and bitter taste of that which is healthy, "though sometimes in a less degree, than even its copiousness would account for."

In considering the abnormal character of albuminous impregnation, the question very naturally arises, whether the adventitious principle be vicarious of any of the healthy constituents of the renal secretion. This question has not yet, however, been examined in a general way, with reference to all the various cases in which such impregnation may occur, and seems to have met with attention in regard to Bright's disease alone. The albumen discharged in that disease has been by some regarded as succedaneous of the urea, which is presumed to be proportionally deficient. A careful revision of the few cases yet published, with the

* Medical Gazette, May, 1837.

necessary details on this point, teaches us to doubt the correctness of this notion. It is no doubt true that urea is deficient in pale-coloured albuminous urine of low specific gravity, and that in the deep-coloured variety of high density it is often present to a considerable amount; but it does not thence follow that it exists in the inverse ratio of the abnormal ingredient, and that the contrary is more likely to be the truth appears on the evidence just alluded to. It may be well to quote the statement of Dr. Christison, in reference to this topic; though published so far back as 1829 it is accurately applicable, as he informs us, to all his subsequent experience.

> "In most of the cases I have seen, where the urine was very pale, of very low specific gravity, and deprived of the greater part of its urea, the quantity of albumen was small, never exceeding three parts and a half of dry albumen per thousand, while in the cases where the urea was considerable in quantity, the albumen was also considerable, being in the former so high as ten or eleven parts in the thousand. Besides, the secretion of albumen may be nearly or entirely prevented by proper treatment, without the secretion of urea being restored." (p. 52.)

M. Rayer has shown the advantages derivable from microscopic inspection of the urine under consideration; such examination is indispensably necessary for the recognition of the globules of pus, mucus, or blood, and of the particles of lithic acid and lithate of ammonia, which are frequently thrown down in union with the coagulum.

Albumen has not been admitted by chemists generally to a place among the normal constituents of the urine; and since attention has been particularly directed to this subject, no facts seem to have been elicited proving error in their views. It is certainly true that this failure in its detection may depend not on the total absence of the principle, but either on its minute proportion, or on its assumption of properties distinct from those it possesses when mixed with other fluids, in other words, on the imperfection of the existing methods of analysis: when Liebig, Dumas, and Christison admit that organic chemistry is in its infancy, it is wisdom to doubt. Cautious assertion is here the more requisite, as we find Henry, at least, enumerating this principle as one of the twenty-one constituents of healthy urine; Chevallier, too, is of opinion, as we are informed by M. Piorry,* that it invariably exists therein in very minute proportion. Dr. Osborne, besides, affirms, but this is mere affirmation, that some individuals constantly secrete urine, coagulable by long-continued ebullition, and fancies the peculiarity caused by a habit of drinking sparingly, whereby a concentrated urine is produced.†

In practice, however, we are fully warranted, on the evidence of repeated examinations, made with a view to the settlement of this point, in asserting that the liquid, normally elaborated by the kidneys, contains no albumen. And, on the other hand, it is indubitable that the fluid discharged from the bladder is found, in a considerable variety of local or general derangements of the system, to be impregnated either temporarily or more or less persistently, either slightly or abundantly, with that proximate principle. It is, perhaps, impossible to establish any classification of these derangements which shall embrace under the same head, all those resembling each other in most essential particulars, and separate such as are marked by special characters; but we venture to

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[*] Traité de Diagnostic, &c. i. 36.

[†] On Dropsies, p. 8. 2d Ed.

propose the following, provisionally, as the least objectionable in the existing state of our acquaintance with the subject. A most important distinction between albuminous urines, practically considered, and one which of course exists in nature, is their being thus impregnated during the process of secretion, (which might depend either on a morbid state of the blood, or on a defective exercise of its secretory function on the part of the kidney,) or simply by subsequent admixture; but as it seems hardly possible to decide, in many cases, to which of these categories albuminous urine belongs, we have not adopted it as the basis of our classification. Our fourth class is a full one; some of our readers may probably contend that it should be still fuller.

ALBUMINURIA may be caused by

- A. *An abnormal condition of the blood, dependent on*
  - Scurvy,
  - Purpura,
  - Hemorrhagic eruptive fevers,
  - Absorption of pus?
  - ——albuminous or dropsical effusions?
- B. *Lesions of genito-urinary apparatus.*
  - *a.* Functional.
    - Essential hæmaturia,
    - Diabetes,
    - Secretory excitement of urinary organs and passages, produced by ....
      - Articles of food.
      - Medicinal agents.
    - Active renal hyperæmia.
  - *b.* Organic.
    - 1. Which cause the foreign admixture during the act of secretion.
      - Acute and chronic simple nephritis,
      - Pyelitis,
      - Bright's disease.
    - 2. Which cause impregnation subsequently to the act of secretion.
      - Blood, thrown out in cases of
        - Contusions, wounds,
        - Calculous pyelitis,
        - Cancer of kidney,
        - Fungous tumours,
        - Acute cystitis.
      - Tubercle,
      - Encephaloid,
      - Strumous matter,
      - Pus ... e.g. in cases of prostatic abscess,
      - Muco-pus, in catarrhal inflammation of mucous membrane of urinary passages, especially of the bladder.
- C. *Accidental admixture of healthy genito-urinary albuminous products.*
  - Semen,
  - Prostatic secretion,
  - Catamenial fluid.
- D. *Doubtful cause.*
  - Acute febrile affections,
  - Hysteria?
  - Scarlatina,
    - Primary fever,
    - Succeeding anasarca,
  - Gout?
  - Chronic diseases independent of renal lesion,
  - Chylous urine.

We proceed to make a few remarks upon the majority of these conditions, in so far as they may be productive of albuminuria. We beg to premise that we do not (except where the contrary is expressly stated) vouch for the authenticity or correct observation of the facts on which their claim to be so ranked rests; yet it is just to add that the zealous candour with which the subject has been investigated by the majority of observers leaves but little room for well-grounded scepticism. Further, we desire it to be understood, that we designedly avoid, for the present, any discussion respecting the degree of faith to be attached to an albuminous condition of the urine as diagnostic of Bright's disease. When treating of that affection we shall carefully examine the points, wherein the albuminuria symptomatic of its existence differs, as it is maintained, from that occurring under other local or general conditions of the economy.

A. That certain morbid states of the blood might lead to albuminous impregnation of the urine seems admissible, *à priori*; and experience has established the possibility of their having such influence. M. Rayer states that he has found that the albumen and globules of the blood pass occasionally into the urine in cases of scurvy, purpura, and hemorrhagic fevers, while the fibrine diminishes in the vessels, and the fluid portion becomes infiltrated into the cellular tissue, or exhaled on the surface of the mucous membranes. We expect in this author's next volume the full and explicit information a fact of such import calls for. The statement is not a novel one, however, for Dr. Blackall, in his work on Dropsies, has actually related cases of scorbutus and petechiæ, in which the urine was coagulable; but as the condition of the kidneys was not made a subject of enquiry, his observations, as well as many of the same stamp by other writers, must, at the present day, be acknowledged to be imperfect.

Our notes of interrogation show that we regard as exceedingly doubtful the possible admixture of albumen with the urine as a consequence of the absorption of pus or albuminous secretions. However, the notion that purulent depositions may be carried off through the kidneys has been held by a variety of surgeons, among the rest by Ambrose Paré, Desault, &c.: and recently M. Solon (p. 404) has availed himself of the doctrine, as one respecting the soundness of which no very serious doubts are to be entertained. And with respect to the passage of albuminous secretions, formed elsewhere, into the urine, we find Cotunnius,* in 1764, endeavouring to explain thereby the presence of albumen in the urine of a dropsical patient, whose anasarca rapidly diminished under the use of diuretics. But our incredulity is justified by the testimony of M. Rayer, who has for the last seven years in vain sought for pus in the urine of individuals, in whom the absorption of purulent depositions of various kinds was effected under his own immediate observation. It may not be correct, however, to deny absolutely the possibility of the excretion of pus under these circumstances, for in two cases of metastatic abscess of the lungs, liver, and kidneys, the tubuli and mamillæ of the latter organs were found gorged with a puriform fluid: these facts would be conclusive, were it not possible that the pus was produced by direct inflammation of the renal tissues. The doctrine of purulent absorption is unfortunately in every respect unsettled.

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* De Ischiade Nervosâ Comment. Viennæ, 1770, p. 30.

Some remarkable cases observed in India have been recently related by Dr. J. Mouat, (*Calcutta Quarterly Med. Journal*, July, 1837—see *Br. and For. Med. Rev.*, Vol. VI. p. 251,) as demonstrating that the excretion of purulent deposits from the liver may be accomplished through the kidneys, intestines, and lungs, independently of any direct communication between the hepatic abscess and those organs. We have attentively perused these cases, but without being convinced of the reality of the fact they are adduced to prove. Those in which examination after death took place, are alone, as is manifest, entitled to serious consideration; and here, admitting the urinary deposit to have been truly purulent, as it probably was, though the fact is far from proved, the post-mortem details are materially deficient; the condition of the kidneys, and especially of the urinary passages is most imperfectly described, and the author appears to have mainly trusted for his particulars to the reports of others. His opinions curiously exemplify the vacillation of medical doctrines: by the multitude all manner of mischief is ascribed to the passage of a drop, aye, even of a drop of pus in substance into the circulation, and patients pronounced to be all but lost, in whom the least symptom of such an occurrence is discoverable; on the contrary, Dr. J. Mouat attributes numerous recoveries to the passage of that fluid through the blood and kidneys into the urine, and the preservation of one man's life for eleven months, to his constantly relieving his liver of quantities of pus by this most extraordinary route. Bengal practice will no doubt supply Dr. Mouat with the opportunity of substantiating his assertions, if they be really correct, in such manner as to place them beyond the reach of critical cavils.

The fact noted by Cotunnius proves nothing, for the absence of albumen from the urine was not ascertained before the absorption of the anasarcous fluid; in similar cases, where this precaution was taken by M. Rayer, none of that principle was ever detected in urine, voided concomitantly with the disappearance of dropsical effusion.

B. *a.* In our second class we have placed essential hæmaturia, a disorder which of course causes the excretion of albumen along with or without the other principles of the blood. The presence of the coagulable ingredient in diabetic urine was first ascertained by Cotunnius (Op. cit. p. 31); no doubt of its frequent occurrence therein, though its amount and degree of persistence are by no means known, is expressed; nor is any hint, respecting its dependence on peculiar disease of the kidneys, let fall even by the most zealous followers of Dr. Bright.* According to Thénard, Dupuytren, and Dr. Graves, its appearance is an indication of approaching recovery; M. Rayer has, on the contrary, satisfied himself that when the urine becomes thus impregnated, the immediate occurrence of dropsy is to be apprehended.

It is a well known fact, that the urine of healthy individuals may become albuminous for a short while, for instance, twenty-four hours after direct or indirect excitement of the urinary passages. We are not quite sure of being right in ascribing the action of certain articles of food and medicinal agents to such intermediate irritation of the kidneys;

* See, however, Cyclopædia of Practical Medicine, vol. i., p. 543.

it is perhaps equally tenable that an altered status of the blood is, in these cases, the direct cause of the excretion of albumen with the urine. Further observations are necessary to allow of a decisive opinion on this point; meanwhile let us state the facts. Dr. Christison "has occasionally known a temporary albuminous impregnation, produced in healthy individuals by eating freely cheese, pastry, and such other indigestible articles as are known to have in general the effect of increasing the usual solid ingredients of the urine, and occasioning a large deposit of lithic acid and lithate of ammonia." (p. 36.) M. Solon contends that where such consequences follow, from the cause stated, individual predisposition must be admitted in order to account for them;—this is very possibly the truth.

Again, Dr. Christison has repeatedly seen the same condition of the urine, induced for a time (he does not specify what time) by a cantharides blister, when it excited symptoms of renal irritation.[*] In the only original case related by M. Rayer, as exemplifying the production of nephritis by the action of a blister, no albuminuria existed.

Dr. Mateer[†] has given us the history of a case of dropsy with incoagulable urine, wherein that fluid became distinctly coagulable during the use of infusion of gentian and carbonate of ammonia, the coagulability increasing on the addition of minute quantities of tinctura lyttæ to the medicine. The curative properties of these remedies seemed to be closely connected with the production of albuminuria. As this writer reminds us, Cullen had remarked that blisters "necessarily cause a great quantity of serum to be carried off by the kidneys;" and he, himself, further imagines that all stimulant diuretics cause an albuminous state of the urine, while they carry off dropsical deposits. We find no facts detailed confirmative of this hypothesis, which originated, as we have shown, with Cotugno, and has been proved fallacious by M. Rayer.

We find Dr. Blackall, again, recording his suspicion that the excessive use of another class of medicines, the alkalies and their salts, "disposes the serum of the blood to pass off by the kidneys."[‡] There is some probability that such is really the case. It appears, indeed, to be generally recognized, not as the result of M. Magendie's speculations, but as that of the experience of such men as Huxham, Dr. Copland, &c., that those medicines, when used in excess, materially attenuate the blood in the end; now, if so, the condition of that fluid is artificially rendered the same as in scorbutic subjects, in whom the occurrence of albuminuria seems unquestionable. All this, however, like almost every other point connected with that phenomenon, requires deliberate investigation.

The testimony of writers respecting the action of mercury in this way is contradictory. The evidence of Dr. Wells, on the affirmative side of the question, must be allowed to carry considerable weight with it: this enlightened observer examined the urine of six syphilitic patients before they began to use mercury, and found that it contained no serum in five, and not more in the sixth, than "the smallest quantity that can be detected by heat or nitrous acid." After they had been in a state of salivation upwards of a fortnight, he reexamined their urine, and discovered that that which originally contained a little, now contained more; that in three other cases the foreign principle was manifestly present; while the remaining two individuals were completely free from albuminuria. Dr. Christison, too, has met with several facts "tending to prove the connexion of such a state with mercurial erythism;" a vague statement of this sort, however, is evidently valueless. On the other hand, M. Solon was unable to detect albuminous impregnation in subjects labouring under ptyalism, consequent on mercurial frictions, employed in a variety of diseases; and M. Rayer is of opinion that the urine does not become thus affected during salivation, unless scurvy, purpura, or an intercurrent affection of the urinary apparatus coexist therewith.

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[*] Cruickshank states (Rollo on Diabetes, 2d Ed. p. 448,) that in strangury from cantharides, the urine assumes the appearance of a mass of hydatids.

[†] Edinb. Med. and Surg. Journ., Jan. 1837, p. 79.

[‡] On Dropsies, p. 81.

It appears from all this that the influence of mercury is far from being firmly established.

M. Solon's volume contains some cases seeming to show that hyperæmia of the kidneys, produced by common causes, will produce albuminuria; but it must be confessed that as no post-mortem examination was made in these cases, the exact condition of the organs is a matter of doubt.

b. We for the present pass over this division of our second class; the proofs of their being entitled to hold the place we have given them will be adduced when we speak of the different organic diseases enumerated.

c. The operation of this class of causes of albuminous admixture is easily intelligible.

The urine becomes loaded to a greater or less extent with semen, under a variety of circumstances. Thus it is well known that the secretion of the testes escapes into the urethra in certain cases of paralysis, and in consequence of excessive venereal indulgence, and that when the bladder is evacuated shortly after coition, its contents carry with them a certain quantity of seminal fluid. Again, a reflux of this fluid may take place into the bladder, immediately after its entry into the urethra, during the act of copulation, in cases of stricture of that canal; and M. Rayer has lately attended two persons, labouring under obstinate constipation, who voided pure semen while at stool; that the fluid was seminal, and not prostatic, was proved by the discovery of a multitude of spermatic animalcules.

The presence of these Entozoa is, according to M. Rayer, the only infallible sign of that of semen; for the nebulæ and sediments of spermatic urine are not, by their simple appearance, distinguishable from those produced by impregnation with mucus. The specific gravity of the animalcules being greater than that of the fluid in which they are suspended, they sink to the bottom of the vessel in twenty-four hours or so, and may then be easily recognized with the microscope. They may be detected in the sediment weeks after its deposition, for though urine kills them almost instantaneously, it retards their putrefaction. The absence of Spermatozoa does not, however, in all cases positively prove that the urine contains no semen, for in phthisical and many aged subjects the latter secretion contains but a very small share of animalcules; such at least is M. Rayer's statement. In cases where the urine is rendered turbid by imperfect semen of this kind, we may be guided in ascribing the muddiness to its true cause, as well by the absence of the microscopical characters of mucus, &c., as by these positive signs: acidity, *general* turbidity, which persists, but does not increase under the action of heat and nitric acid, and exhalation of the peculiar odour of semen during evaporation. We have been thus particular in enumerating the characters of seminal impregnation, because we are persuaded that its discovery may now and then become a matter of real practical importance. Dr. Willis somewhere remarks, in the work recently reviewed in this Journal, that such detection may occasionally give a clue to the origin of emaciation and cachectic symptoms, in cases where the practitioner had puzzled himself in vain in searching for organic lesions to explain the condition of his patient.

M. Rayer is acquainted with no method of distinguishing the secretion of the prostate gland when mixed with urine.

D. Cotunnius maintained that "all the vapours exhaled in the various cavities of the body as well as the urine become coagulable during disease." (*Morbi vi.*)[*] Our countryman, Cruickshank, believed that albuminous urine attended "every case of increased action of vessels, more particularly of the inflammatory kind."[†] Nysten announced in 1811 the presence of a large quantity of albumen in the urine of a subject who died rapidly of acute peritonitis, and appears to have conceived albuminous impregnation characteristic of inflammatory urine generally, though he allows it may sometimes depend on a particular condition of the urinary organs.[‡] M. Rayer, also, to pass over a variety of evidence to the same effect, supplied by intermediate observers, states positively that the urine may contain albumen in acute affections for a period of several days, that its appearance almost always coexists with an increase of specific gravity, and of the healthy proportion of lithic acid and lithate of ammonia, and ordinarily indicates congestion of the kidneys, ureters, and bladder. M. Andral, too, declares that he has occasionally met with coagulable urine in the course of acute diseases.

But the most striking observations, appertaining to this matter, have recently been made public by M. Solon: to this gentleman the profession is beholden for the anouncement of a presumed connexion between the appearance of albumen in the urine and the establishment of a favorable crisis in acute febrile, and inflammatory disorders. We shall first lay an abstract of his facts before the reader, and then make a few remarks as to the claim of such urine to be termed a "critical sign." Upwards of one hundred pages of M. Solon's volume are occupied with a disquisition on this subject.

His cases are divisible into two distinct groups: in one of these the urine appears to have become coagulable by heat and nitric acid, precisely in the same manner as in Bright's disease; in the other, precipitable by nitric acid but not coagulable by heat; ebullition, on the contrary, here redissolves the precipitate occasioned by the acid. There can be no doubt that in the first group of cases the coagulation depended on the presence of albumen; and we are told that the foreign principle disappeared gradually with the advancement of convalescence: the action of heat showed that the precipitate in the second was not of albuminous nature, and the microscope in the hands of M. Donné, that it was really composed of lithate of ammonia. The following table gives at one view the results of M. Solon's experience on this matter:

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[*] Op. cit., p. 31.

[†] Rollo on Diabetes, p. 444.

[‡] Recherches de Chim. et de Phys. pathologiques. Paris, 1811, p. 253.

Diseases.		Number of Cases.	Coagulability					
			Not produced by any reagent.		Produced			
			A		by heat. B.		by nitric acid. C.	
			Recovered	Died.	Recovered	Died.	Recovered	Died.
Intermittent fevers		8	1	0	2	0	5	0
Febrile Exanthemata.	Measles	7	2	0	1	0	4	0
	Variola	11	6	0	1	0	4	0
	Scarlatina	3	1	0	1	0	1	0
	Pemphigus	1	0	0	0	0	1	0
Typhoid fevers		23	3	1	3	1	15	0
Bronchitis		1	0	0	0	0	1	0
Pleuropneumonia		24	2	0	2	0	17	3
		78	15	1	10	1	48	3

Hence it appears that the urine of $\frac{1}{11}$ of a mass of patients affected with acute diseases may be expected to become albuminous at some period or other before their recovery. The cause of this phenomenon is not easily demonstrable; M. Solon himself seems inclined to ascribe it to "a modification of secretion, occasioned by a nervous influence;" an hypothesis which cannot be said to boast of much plausibility. But no matter how they are to be accounted for, it is manifest that these facts, if correctly observed, clash with the too prevalent notion that albuminuria is pathognomonic of a certain affection of the kidneys; it is right, however, that the reader should be put in possession of Dr. Christison's mode of attempting to evade their force. We quote his words: "It is fair and reasonable to throw out the query, whether, considering that urine decidedly albuminous is a very rare concomitant of the crisis in these acute diseases, the cases where it does occur may not actually be instances of a tendency to granular deposition, arrested along with the local and general reaction which it accompanies? The affirmative is a perfectly reasonable answer. Singularly enough, in the only two cases where I have observed coagulable urine during the crisis of acute diseases, namely, in two cases of acute pneumonia, the progress of matters unequivocally proved that the patients laboured under granular degeneration of the kidneys." (p. 42.) In this view, however, Dr. Christison has been anticipated, strangely enough, by M. Solon himself, who has tendered it in explanation of Dr. Gregory's case of typhoid fever with coagulable urine, while he has totally overlooked its applicability to his own cases. It was certainly incumbent on Dr. Christison to publish accurate histories of the cases to which he alludes, and so allow the medical world to judge for itself whether their progress was really such as he presumes it to have been. The question is too important a one to be decided by mere assertions.

The second group of cases is unimportant in respect of the presence of albumen; but, combined with the first, it shows that *precipitable* urine may be looked for in very nearly three fourths of all cases of acute febrile disease. It remains to be asked whether these conditions of the urine really constitute a "critical sign" of the resolution of the affections during which they are observed? By referring to the Table (p. 133), it will be seen to contain nineteen exceptional cases in this point of view: 15 in the columns A; 1 in those headed B; 3 in those marked C. M. Solon must be heard in explanation of these exceptions: In respect of the 15 cases of recovery wherein no precipitate occurred, he reminds us that the kidneys are not the sole critical emunctory,—that the skin and intestinal canal should not be forgotten; and that the greatest number of exceptional cases were of smallpox, where the skin may be supposed "to be specially charged with the critical movement." Again, the patient who died in spite of his urine having become albuminous, "was affected with splenitis, and *may have* suffered from absorption of pus, which, in its turn, *might* have caused the passage of albumen into the urine." We have already adverted to the probability of purulent urine being thus induced. Lastly, the phenomenon existed "in a very moderate degree" in the exceptional pneumonic cases. The reader will form his own judgment of the validity of this author's defence; he will probably be guided therein by learning that coagulability occurred in "a transient and, *as it were*, insignificant manner at various periods of the diseases mentioned" (here M. Solon confirms the statements of previous writers); "frequently supervened, and then disappeared, *because* other morbid phenomena declared themselves; but finally appeared again, to announce definitive resolution."

It is not a little remarkable that Cruickshank (Op. cit., p. 448,) believed the *disappearance* of albumen from the urine to point out the same favorable turn in the course of inflammatory affections, which is indicated, according to the French writer, by its *appearance* therein. A third version, possibly, however, coinciding with that of Cruickshank, comes from Dr. Mateer (Loc. cit., p. 92); he conceives that albuminuria occurs towards "the turn of fever; that after the kidneys have for some time secreted albumen, this suddenly disappears, and lithic acid and the lithates are formed in its place, and in this way the *crisis per urinas* is effected." The truth is that albuminuria is an occasional concomitant of acute diseases; but what relation it may bear to what is termed their crisis is utterly unknown; how, indeed, could the fact be otherwise, when the reality of that crisis is still a problem?

Dr. Copland states that he has observed coagulable urine frequently in the acute diseases of children, "where no alteration of the kidneys existed." These facts are unimportant, according to Dr. Osborne, because the urine is quite differently constituted in children and adults. We know not whence Dr. Osborne has derived his information, which is wholly irreconcilable with the results obtained by modern chemists of repute.

The belief in the occasional presence of albumen in the urine of hysterical subjects seems to rest on an analysis made by Peschier in 1826.* It is impossible, however, even to guess what may have been the organic condition of the individual supplying the urine examined: indeed, the title given to the article is the sole evidence of the existence of hysteria; and its author does not appear to have been a medical practitioner. Both Nysten and M. Solon have in vain sought for a particle of albumen in the urine in various nervous affections.

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* Journal de Chim. Médicale, vol. ii. p. 234.

When Dr. Copland, in imitation of Dr. Wells, speaks of albuminous flocculi being deposited during the continuance of the anasarca, occasionally succeeding scarlatina, he probably confounds mucous nebulæ with the coagulable principle; but that the latter is discoverable under those circumstances frequently, if not constantly, no practical observer now disputes. The condition of the kidneys in these cases is, however, not yet decisively settled. Burserius, Fischer, Dr. Blackall, and others have furnished evidence of their presenting more or less marked signs of inflammatory action; but accounts of dissections published before the general promulgation of Dr. Bright's doctrine, need not, for obvious reasons, be examined at any length. Thus limited as to the date of our documents, the earliest communication we find on the subject is that of Mr. Hamilton.* In several cases of death occurring in patients anasarcous after scarlatina, that observer found the kidneys diseased; and, on showing accurate drawings of their lesions to some Edinburgh professors, these gentlemen pronounced them to represent specimens of the early stage of Bright's disease; the descriptions given in the essay justify their decision. Cases are also related here, showing that the urine may be coagulable and the kidneys diseased, before the termination of the primary fever.

MM. Sebastian and Constant and Dr. Mateer give their testimony to the correct observation of Mr. Hamilton; in the volume before us, too, Dr. Christison supplies two cases (x. and xxvi.) which he regards as unequivocal examples of Bright's disease, produced by the renal irritation consequent on scarlatina.

On the other hand Dr. Graves† has, as he avers, seen a case of dropsy after the eruption in question with highly albuminous urine, wherein the kidneys "presented a perfectly healthy structure;" and Dr. Willis (Op. cit., p. 157,) is led to conclude that structural disease of those organs could not have been the cause of the albuminuria almost constantly observed by him to accompany the condition in question. We regret sincerely these authors should have deceived themselves into the belief that vague impressions—and these, in the instance of the latter, almost dubitatively stated—could have any weight in the decision of a question of facts like the present. Had they faithfully recorded the cases to which they refer, they would have done good service to renal pathology; as it is, they have simply excited doubts, without producing a particle of conviction. Unfortunately their error is common to the majority of the medical writers of the day. Open any work you please, and with the rarest exceptions, you will search its pages in vain for well-observed *facts*, and as surely will your equanimity be ruffled by the constant recurrence of egotistical displays of empty *opinions*.

We are not aware whether any investigations have been made into the condition of the urine in the anasarca following, in some instances, the other exanthemata.

It has been asserted that the urine is sometimes albuminous in gout;

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* Edin. Med. and Surg. Journ., Jan. 1835.

† Med. Gazette, Oct. 20, 1838.

no particulars of the organic condition of individuals thus affected are given by the authors of the assertion.

Organic diseases, uncomplicated with renal lesion, undoubtedly form the most important class of cases, distinct from those of Bright's disease, wherein albuminuria may occur,—inasmuch as the existence of that symptom is in them most likely to betray the observer into an erroneous diagnosis. Dr. Darwall was the first to bring forward an example of the coexistence of albuminuria, dropsy, disease of the heart, and "perfect soundness" of the kidneys.* Professor Forget's seventh and eighth cases exemplify a similar combination of circumstances,† and M. Solon has added another to the list. Dr. Christison, however, strives to deprive the facts of the French observers of their importance, by hinting that they were not perfectly competent judges of the morbid anatomy of the kidney, at the time they met with them. On this we have to observe, that M. Solon is to our knowledge a man of most sound general information, and had, besides, been at least a year engaged in the study of renal pathology at the period in question. And, as respects M. Forget, the attestation of the absence of renal lesion is clear and straightforward; and further, given unusual weight by the fact that its author actually expresses vexation at his inability to find a trace of disease in the kidneys. We must warn the reader, too, that it is utterly incorrect to state, as Dr. Christison does, that M. Forget "appears always to have looked for some degree of granular deposition;" on the contrary, he pointedly declares that he adopts the term albuminuria, because such deposition is not a constant phenomenon.

M. Solon narrates the history of a subject who died with anasarca, bronchitis, diseased intestine, and healthy kidneys, and had, during life, discharged albuminous urine. Dr. Morrison, again, has recently given the particulars of a case of aneurism of the ventral aorta, which terminated fatally by rupture and effusion of "at least four quarts" of blood into the abdomen; the kidneys were found in every respect natural, except "from their seeming, like the other abdominal viscera, totally devoid of blood;" yet during the previous *five years* the urine had at various periods been found coagulable by heat and acids. The medical attendant's persuasion, shared in by all who had seen the case, that his patient laboured under special renal disease seems a fair guarantee of his having examined the kidneys with all necessary care.‡

M. A. Toulmouche has swelled the list of such facts by the relation (*Gaz. Médicale*, Fev. 1839,) of a case of phthisis, attended with chronic peritonitis and ulcerations in the intestines, wherein the urine furnished a thick stratum of albumen, when treated in the usual manner, though on the death of the patient the kidneys were found "of small size, pale, and perfectly healthy."

Much has been written on the connexion between dropsies and albuminous urine, especially with a view to proving the latter a dependence on the former. Attempts have even been made by Cruickshank and Dr. Blackall to show that albuminuria exists only in the form of dropsy unconnected with organic disease, and hence to establish a method of diagnosticating the two kinds of serous effusion. It is needless to dwell on the fallacy of this view, the converse of which would be nearer the truth. That coexisting albuminous impregnation of the urine and dropsical effusion stand to each other in the relation of effect and cause may be doubted, for the following reasons: 1. Coagulable urine is voided in cases where no dropsy exists. 2. When dropsy disappears under the influence of diuretics, the urine does not therefore become coagulable. 3. No cases of dropsy occurring in subjects proved to be free from organic disease or a morbid condition of the blood, and in which the urine has been found albuminous, are on record.

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* Cyc. Prac. Med.—Art. "Dropsy."

† Gaz. Méd. vol. v. p. 609.

‡ Dublin Med. Journal, No. xxxvi., 1838.

Such seems to be a tolerably accurate enumeration of the various conditions under which albuminuria has been observed. From the facts related or referred to, the subjoined propositions immediately derive:

1. To infer the existence of a special lesion of the kidneys from the mere presence of albuminuria is utterly incorrect.
2. Consequently, boiling the urine of all the inmates of an hospital, according to the plan of certain observers, in order to determine the frequency of Bright's disease, is liable to lead to false deductions.
3. It will be necessary for future observers to specify the condition of their patients in respect of all the agencies presumed to give rise to a discharge of albumen with the urine, in order to entitle them to refer its appearance, indisputably to any one of those agencies rather than another.
4. The characters of the albuminous precipitate itself, as well as those of the urine containing it, must be much more carefully noted than has hitherto been habitually done.

M. Rayer terminates a very lucid enquiry into the authenticity of cases, reported as examples of the discharge of milky urine, by a number of propositions which we condense as follows: The existence of *naturally milky* urine, at present rests on the mere testimony of authors, never having been proved by rigorous experiment; the only legitimate inference, from cases hitherto published, is that the presence of an abundance of fatty matter in the urine will give that fluid a lactiform appearance: casein and the true milk globule, the only incontestable evidences of the presence of milk, have never yet been recognized in urine, unless in cases where the two fluids had been artificially mixed. Dr. Aldridge's case, as reported by Dr. Graves, and faithfully translated in the pages before us, is inconclusive, according to M. Rayer, of the fact it is adduced to prove; because, though Dr. Aldridge states his having detected casein, he leaves us in the dark as to the method he employed in his search for it, and does not describe its characters with accuracy. Dr. Graves will, we doubt not, see the reasonableness of M. Rayer's demurrer, and probably induce his friend, provided accurate note was made of them at the time of their performance, to give his experiments publicity.

M. Rayer admits that the materials of the bile pass into the urine in various affections of the liver, and in all diseases attended with mechanical obstruction to the flow of the hepatic secretion; to these M. Solon adds the "occurrence of the disordered state of secretion, known by the ancients as the *bilious state*." Nearly forty pages of the latter gentleman's book are occupied with an enquiry into this subject, whereon he appears to fancy himself much more of an original observer than he really is. The whole doctrine was distinctly stated by our most sagacious countryman, Dr. Cruickshank, in his additions to Dr. Rollo's work: "In jaundice," he says, "the state of the disease may be ascertained by examining the urine, in which a very small quantity of bile may be detected by the nitric or muriatic acids," &c. &c.

M. Rayer has recognized the peculiar action of ammonia on pus, whereby it converts that fluid into a ropy, slimy matter, varying in consistence with the quantity of alkali present. He is induced to believe that the ropy matter, detected in certain specimens of urine, such as those voided in chronic cystitis, &c. is not secreted in that state; an opinion which rests on his having repeatedly followed the conversion of pus in acid urine into a ropy, "catarrhal" matter, in proportion as an alkaline reaction was established by the development of ammonia. Our readers will here discover the conformation of Dr. Babington's ingenious researches in the same direction: their accuracy is further attested by Dr. Bright, who "has at this time under his care, a lady, labouring under copious purulent discharge from the kidney, which, on one or two occasions, has been passed in the form of mucus, owing to the administration of alkaline remedies."

If a drop of acid urine, rendered turbid by impregnation with pus, be submitted to microscopic inspection, immediately after emission, and before the foreign fluid has had time to gravitate to the bottom, a vast number of globules are detected. These are regular and spheroidal, measure about one hundredth of a millimeter,* have a well-defined border, a semi-transparent white surface, and a granular appearance, owing to the presence of a number of minute grayish granules in their substance. The changes these globules undergo, by the progress of putrefaction, are well described; but we regret to find the announcement, already made by M. Donné, confirmed by M. Rayer, to the effect that no distinctive character between these and mucous globules is detected with the microscope. If not by their globule, these fluids are, according to our author, otherwise distinguishable; pus or muco-pus contains albumen and fatty matter, mucus does not. The presence or absence of pus or muco-pus in a fluid of doubtful character may be, therefore, easily ascertained by the action of nitric acid, heat, and ether.

From the very comprehensive details that follow on the passage of medicinal agents into the urine, on the nebulæ, sediments, and cremors of that fluid, and on the phenomena of its putrefaction, we have room but for one or two extracts. It appears, as we have already mentioned, from the researches of M. Quevenne, that a formation of *blackish globules* takes place in acid urine, presenting an amorphous sediment shortly after emission, from the third to the twentieth day after that process. They are composed of superlithates, ordinarily of ammonia, measure one hundredth of a millimeter in diameter, and are frequently mixed with crystals of neutral ammoniacal phosphate of magnesia. When urine of this description grows alkaline, a new species of globules makes its appearance: these are white and *organic*, of new formation, and measure only $\frac{1}{1200}$ of a millimeter in diameter; the lithates very rarely assume the globular form in alkaline urine. A third sort of globule has been detected in the sediment of diabetic urine by the same experimenter; this, from his description, appears to be identical with the ordinary *yeast-globule.*

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* The millimeter is equal 0·039370 of an English inch.

In his chapter on the relations presumed to exist between certain morbid conditions of the urine and those of the blood, and other fluids, M. Rayer has given a luminous statement of the whole fund of knowledge as yet accumulated on this most important topic: we shall in due season avail ourselves of its most practical matter.

In examining the body of our author's work, our attention is first arrested by the section on mechanical lesions of the renal organs, under which head are comprehended *wounds, concussions, contusions,* and *lacerations.* The general impression derived from an attentive study of the facts, narrated in this division of the book, is the exceeding difficulty of forming an estimate, immediately on the occurrence of the accident, of the probable results of the injury sustained by the kidneys; cases have gone on in the most favorable manner for weeks even, and at the end of that time terminated fatally in a few days by the renewal of internal hemorrhage. The practitioner acquainted with the nature of previous experience in such cases will, therefore, be cautious in his prognosis to a degree that may appear extravagant to an ill-informed man. On the other hand, recovery has occasionally occurred where the most sanguine might well have despaired; of this abundant proof is given by the remarkable case, published by our countryman, Hennen, wherein complete restoration to health followed the discharge of a piece of cloth by the urethra, eight months after a gun-shot wound in the loins. M. Rayer, indeed, seems of opinion that the severity of these cases does not so much depend on the lesion of the kidney, *per se,* (except when the pelvis, calices, or renal vessels are wounded,) as on the accompanying injury done to neighbouring parts, such as the intestine, peritoneum, &c. This position is founded on a fallacious application of the results of experiments on the lower animals to the human subject:* the cases related in his pages prove it to be untenable, though we admit they show that, with the proviso stated, these injuries are on the whole less dangerous, perhaps, than is currently believed. Those which prove most quickly fatal affect the renal vessels.

Here is a statement which if not new is yet sufficiently important to excuse repetition: "discharge of blood by the urethra, and pain in the region of the kidney, following an injury in the loins, are not infallible signs of a wound of the viscus in question, *unless the hemorrhage follow the injury almost immediately*. Traumatic inflammation of the lumbar muscles and cellular membrane may excite such inflammation of the kidney as shall in its turn cause the pain and hæmaturia alluded to. The diagnosis will be clear, if urine, or a fluid impregnated therewith, escape by the external opening." (p. 249.) On the other hand, in cases of contusion at least, the cortical substance has been superficially wounded without a drop of blood being discharged by the urethra: this is easily comprehensible. Lacerations of this kind, indeed, appear to be quite as curable as simple ecchymoses; an effusion of blood or coagulable lymph between the edges of the solution of continuity prevents the extravasation of urine, and subsequently effects cicatrization. This is well illustrated in our author's first plate.

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* As Professor Marjolin very justly observes, (Dict. de Méd., i. 158,) were we to judge from experiments on the lower animals, wounds of the peritoneum itself must be looked on as almost unattended with danger. But the truth is, that any absurdity may be quasi proved by a clever use of analogical argument. Voltaire in his inimitable tale *Micromégas,* makes one of the *dramatis personæ* assert that the planet Mars is provided with *two moons.* In anticipating arguments against so notoriously false an assertion, he refers the incredulous to those who reason from analogy, as to persons who will prove the thing as clearly as that two and two make four:—he but justly ridicules the pretensions of such sophists. What then shall we say of the cruelties of M. Magendie—of the pertinacious enthusiasm with which he strives to foist on us the results of his complicated tortures of dogs, pigeons, and guinea-pigs as invariably applicable to the phenomena of the human system? What too of the wretched vanity that instigates him to repeat in public over and over again, year after year, experiments which he himself assures us have, Heaven knows how long, been proved to be conclusive of anything and everything he likes? What of the humanizing influence such unjustifiable proceedings must exercise on the youths who witness them?

M. Rayer's experience is confirmative of a most interesting announcement respecting the etiology of calculus made some years past by Mr. Earle:[*] he has found that traumatic lesions of the kidney predispose to calculous affections, and especially to certain forms of gravel. The occasional recurrence of dull local pain in the lumbar region after such injuries, even though it appear only at distant intervals, and last on each occasion for an extremely short time, appears to indicate, with some certainty, this unfortunate tendency. The inference from this is that his medical attendant should never lose sight, if possible, of a patient who has suffered renal injury, nor fail to combat energetically the slightest symptoms which may be traced to the temporary manifestation of an habitually latent inflammation.

There is nothing novel in the mode of treatment recommended for these accidents; this is as it should be, inasmuch as M. Rayer seems to have had no personal experience of them in their early stages. We can, nevertheless, recommend this chapter as one to which, from the excellence of its arrangements and abundance of its borrowed facts, the practitioner may refer, on an emergency, with almost complete certainty of finding the information he requires.

Passing on to inflammatory diseases of the kidneys, our author commences by dividing these affections into three groups, according to the tissue they immediately occupy: each of these groups is subdivided into a certain number of species, distinguishable from each other, either by the cause producing them, or by some particular symptomatic character in the malady. Here is the classification.

1. NEPHRITIS. Inflammation of the cortical or tubular substances.	1. *Simple.* 2. *Produced by morbid poisons.* 3. *Arthritic, i. e. Gouty and Rheumatismal.* 4. *Albuminous.*
2. PYELITIS. Inflammation of the pelvis and calices.	1. *Simple.* 2. *Blennorrhagic.* 3. *Calculous.* 4. *Verminous.*
3. PERINEPHRITIS. Inflammation of the external cellular and fibrous membranes of the kidneys, or of their investing adipose cellular tissue.	
4. PYELONEPHRITIS. A combination of Nos. 1 and 2.	

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[*] Med. Chir. Trans., xi. 211.

That the anatomical distinctions, assumed to exist between renal inflammations by this arrangement, are really to be found in nature, the subsequent pages even of the present volume very fully show; it is most gratifying to discover in this separation of nearly similar disorders, decided evidence of increased accuracy of knowledge of renal disease, and we are willing to wait patiently for the labours of future observers to point out how these different local states, for this M. Rayer has failed to do, are in all cases to be symptomatically distinguished. As to the four species of the first group, Nephritis, these, when fully developed, differ from each other in respect of their symptoms and anatomical characters, while they all agree in commencing by partial or general hyperæmia. Thus we are told, in respect of the organic changes induced by each, that deposition of pus is a frequent termination of the simple variety; that accumulation of plastic lymph or lithic acid, in the cortical substance or mammillæ, is very often remarked in the arthritic species; that gangrene more particularly characterizes the form of the disease produced by "infection;" and that the most ordinary appearances in the albuminous variety are anæmia, consecutive to hyperæmia, increase in size and weight of the affected organs, and milky spots or granulations. Induration and discoloration are common to all the species except the third. To the general correctness of this statement, no serious objection can, we believe, be offered; but as we proceed, it will be found that here, as elsewhere, nature transgresses the limits set her by artificial separations.

The remainder of the present volume is devoted to the history of the subdivision *simple nephritis* alone, illustrated by 104 reported cases, of which 53 are original, and 51 selected from various authors, of very questionable scientific reputation in the majority of instances. To such analysis of this vast chapter (it contains upwards of 300 pages), as our space will permit, we at once proceed.

Mechanical agencies hold a prominent place among the causes of nephritis, as admitted by M. Rayer: of these two distinct kinds may be enumerated, wounds and analogous injuries, and the irritation of foreign bodies, such as calculi, worms, or urine abnormally retained in its excretory passages, and more especially in the pelvis of the kidney. From this it follows that all morbid states, inducing such retention, are really indirect causes of renal inflammation; thus various affections of the ureters, prostate, urethra, and bladder, displacements and diseases of the uterus, ovaries, rectum, &c., nay, even cerebro-spinal maladies, through the paralysis they immediately, and the retention of urine they mediately occasion, rank among the causes of simple nephritis. And it further follows, that advanced age powerfully predisposes to the complaint, for all the morbid conditions of the genito-urinary organs, &c. that we have named are far most frequent at that period of life. Experience corroborates this inference; the occurrence of simple nephritis in children under seven years of age is exceedingly rare. In all these cases, inflammation of the pelvis precedes that of the substance of the kidneys. Cantharides, nitre, and similar agents, our author is of opinion, very rarely produce the disease: a notion carried still further by Professor Chomel, who doubts that a single unquestionable example of such causation is to be found:* this doctrine however, we are firmly convinced, and there are cases in our author's volume justifying this conviction, is by much too exclusive. Cold and damp, and the extension of a contiguous inflammation, terminate M. Rayer's catalogue of causes. On the influence of sex he has nothing of a positive kind to offer: the liability to the indirect causes of the complaint seems *à priori* to be pretty closely equal in the two sexes.

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* Arch. Gén. de Méd., Jan. 1837.

Influences which act at once on both kidneys usually excite inflammation in both simultaneously, but the disease may exist in a very different degree in the two organs. Again, when inflammation of one kidney is produced by mechanical agencies acting directly thereon, a temporary influence on the opposite organ is clearly manifested in the majority of cases by a material diminution or total suppression of the urinary secretion. Further, one organ only inflames in some instances, where the cause (e. g. the irritation of cantharides, cold and damp, &c.) should, to all appearance, act equally on both: in this fact we recognize a further illustration of the unfrequency of coexisting inflammation in symmetrical organs, which observation has established to be one of the general laws of inflammatory action.

We turned with very considerable anxiety to the symptomatology of acute nephritis; but we must confess that the results obtained by our author do not, by the new light they throw on its diagnosis, respond to the high estimate we had formed of their probable importance. Before the publication of this volume, it was fair to ascribe our imperfect acquaintance with the symptoms of nephritis to the fact that no observer had methodically and earnestly entered upon the investigation of this and cognate diseases; we are now constrained to admit that there must be some inherent difficulty attached to their study.

Respecting the rigors and febrile reaction of invasion, we are told nothing of much importance: the former are pronounced to occur in every case without exception, a statement which we are persuaded is incorrect. (Vide M. Chomel's paper.) The characters ascribed to the pain in nephritis are these: it may be superficial or deep seated; limited to a small or spread over a large surface; so intense as to be materially aggravated by the slightest touch, or so obscure that forcible alternate pressure on both kidneys is requisite to show which is affected; increased by movements of the trunk, and occasionally by decumbiture on the affected side and by the heat of the bed; remittent; rarely pulsatile (this is, on the contrary, an ordinary character of the pain in perinephritis,) and irradiating upwards to the diaphragm, or downwards to the bladder, groin, and testicle, or uterus and round ligament in the female. The bladder, strangely enough, is sometimes the principal if not the sole seat of the pain, in cases wherein that viscus is found to be in an almost perfectly healthy state after death, while the kidneys are seen to be profoundly disorganized; this peculiarity was long since noticed by Morgagni. Painful retraction of the testicle sometimes occurs, but is a more frequent attendant on pyelitis and calculous ureteritis.

M. Rayer's first plate includes an engraving of a kidney so enormously enlarged in the course of acute inflammation (it weighed 17 oz.), that a tumour was detected during life, both by application of the hand and by percussion; with similar cases, however, we must very rarely expect to meet. According to our observer's experience, the temperature of the superjacent skin is not increased, unless there be coexistent perinephritis or extra-renal abscess.

The modifications of the urine in this affection affect its quantity, visible qualities, and composition. The secretion is always diminished in quantity, and sometimes wholly suspended: (M. Rayer says of the latter "*especially* when both kidneys are affected;" we find no instance of such suppression, however, in his narrated cases, except where both organs were diseased.) The urine may be excreted rarely, twice or thrice in the twenty-four hours, or there may be distressing micturition. The chief changes in point of composition are stated to be caused by impregnation with blood or albumen, by diminished acidity, neutral reaction or alkalescence, and occasionally by admixture with pus. It is affirmed in the most distinct manner, that the discharge of sanguinolent or albuminous urine is not peculiar to the traumatic variety of nephritis, but that it may also occur when the attack proceeds from general causes or diseases of neighbouring parts. Its appearance is accidental and temporary, and may be completely put a stop to—unless when dependent in part on a coexisting affection of the bladder or urethra—by venesection. Now, in true albuminous nephritis (Bright's disease), the presence of albumen in the urine is a constant condition at all its periods; bloodletting will not cause it to disappear, and the quantity of the foreign principle, as well as of the urine voided, is always greater than in the simple variety of the disease: the coagulable excretions of both are hence sufficiently distinguishable.

These statements call for a remark or two. In the first place, the fact of coagulable urine being excreted in simple nephritis, is of such importance as to require numerous well-observed facts for its substantiation: here the onus probandi rested with M. Rayer, but unless assertions, of which he is no niggard, be taken for proofs, he has declined it almost completely; no case of acute nephritis is related in his volume wherein such a phenomena occurred, and three cases of the chronic form (in one of these the urine was purulent, the prostate and pelvis of the kidney diseased—in another there was cystitis and chronic enteritis,) are all that have been mustered in its exemplification. But while we reprehend this slovenly omission (for we doubt not M. Rayer has met with many more cases which he considers confirmative of the assertion he has made), we confess ourselves disposed to believe that assertion well founded. When the urine is mixed with pus, or tinged with blood, no doubt can be entertained on the matter; and M. Solon has reported at least three cases of acute and chronic simple nephritis attended with albuminuria: the ground on which this observer distinguishes the chronic case from Bright's disease—namely the absence of anasarca—is more debateable, however, than he seems to imagine. Secondly, there is an error in M. Rayer's declaration of the constancy of the presence of albumen in all the stages from first to last of Bright's disease; Dr. Bright himself and Dr. Christison have both noted its occasional total disappearance in that disorder. Thirdly, we dissent altogether from the correctness of taking the comparative abundance of urine discharged in Bright's disease as a ground of diagnosis between the two affections; in the acute stage of that disease, the secretion from the kidneys may be scanty, as well as in simple nephritis.

The urine of acute simple nephritis resembles that of Bright's affection by its diminished acidity and lowered proportion of lithic acid and lithates.

In the former disease, when chronic, alkalescence is more common and more persistent than in the latter; but the decrease in the proportion of the salts is least marked in the former.

The colour of the urine varies according as it contains blood, pus, mucus, &c.; it is usually pale when the disease is at its maximum (and not of the traumatic species), as it is then rarely impregnated with blood, and but slightly charged with lithic acid and colouring matter. M. Rayer makes an important rectification of the prevalent opinion that purulent urine is among the consequences of suppuration in the substance of the kidney: such condition of the fluid is on the contrary the ordinary effect of complication with inflammation of the mucous coat of the excretory passages. He has discovered depositions of pus in the cortical substance after death in cases wherein the absence of pus from the urine had been repeatedly established during life; and he denies that purulent impregnation ever depends on the presence of pus in the kidney, unless there be a direct communication between an abscess and the pelvis of the organ, or an ulcerative inflammation of the mammillæ.

Nothing is said of the density of the urine in the general description of the complaint; but, on referring to the table of specific gravities already adverted to, we find 1011·9 and 1015·6 noted as the mean result of five examinations in two subjects. The fluid was alkaline in these cases.

In the account of the general symptoms and local disorders affecting other organs in the course of the disease, we perceive nothing strikingly novel. Four forms of the malady, differing from each other in the general character of the symptoms, are enumerated; the *acute benignant*, the *inflammatory*, that in which *ischuria and cerebral symptoms* predominate, and a fourth remarkable by the tendency it manifests to *malignity and putrescence*.

M. Rayer prefaces his history of chronic nephritis by affirming that the symptoms of that affection have not yet been correctly laid down; pathologists having according to him fallen into a grievous error in ascribing the habitual excretion of purulent urine and the development of a tumour in the loins—which are in truth symptoms of chronic pyelitis—to chronic inflammation of the substance of the kidneys. If we are to trust his experience, "habitual pain in one or both renal regions, coexisting with diminished acidity, neutral reaction, or more especially alkalescence of the urine, and a sensation of weakness and numbness in the lower extremities" constitute the principal characters of chronic nephritis. But the confidence which this plain statement gives of encountering but little difficulty in the diagnosis of the affection is materially weakened by the subjoined qualification, that we are not justified even in suspecting the existence of the disease in many cases (*latent nephritis*) unless on the evidence of a most minute examination of the urine.

The major position M. Rayer seeks to establish respecting this disease, is that it exercises vast influence on the generation of phosphatic calculi. The urinary sediment in chronic nephritis, when the fluid itself is alkaline, is either amorphous and composed almost wholly of phosphate of lime, or chiefly constituted by crystals of ammoniacal phosphate of magnesia; or as most commonly happens, both these salts are held in suspension together with globules of mucus and a small quantity of lithates. The contents of the bladder are in ordinary cases excreted frequently and in small quantities at a time. Impregnation with blood or albumen is rare in the uncomplicated disease, with mucus frequent, with pus indicative of inflammation of the mucous membrane of the passages.

There is no fever, but the disease is attended with progressive emaciation, and a general cachectic state which, as M. Rayer declares, favours the development, among other morbid conditions, of pulmonary tubercularization. This may be true; but we must confess we shall wait for some more convincing proofs of a point so difficult of demonstration than a mere assertion, before we allow that it may not be false.

The diagnosis of this affection is occasionally confirmed by the excellent effects of local antiphlogistic measures; the renal pain, and the turbidity and alkalinity of the urine not unfrequently disappear altogether for a while after cupping in the loins.

It is plain that one of the most common and important varieties of what has been termed the "phosphatic diathesis" is portrayed in the preceding sketch. It has indeed long been known—and English writers were the first to advocate the notion—that injuries of the loins, and such diseases of the urinary passages as require instrumental treatment, induce the excretion of phosphatic salts with the urine. But these conditions are only its indirect cause,—the chronic nephritis, which they directly excite, has not, so far as we are aware, been yet put forward otherwise than vaguely and incidentally as a condition entailing an accumulation of phosphates in the urine.* On the other hand, it must be distinctly understood, as M. Rayer himself declares, that sensibility under pressure, which is the only symptom localizing the disease in the kidney, may be absent while all the other morbid phenomena of the phosphatic diathesis wear out the constitution of the patient: whether they are in such cases dependent on functional derangement of the kidneys only, or on latent chronic nephritis, can only be safely determined by post-mortem examination, and our author has as yet had no opportunity of appealing to its decision.

The anatomical characters of acute and chronic nephritis are next treated of, each receiving most beautiful and accurate illustration in the accompanying plates. We shall content ourselves for the present with enumerating them with all possible conciseness, as it will save repetition to reserve our more careful examination of them for another article. In the acute form, then, they are: 1. General or partial increase of size, according to the extent of the inflammation. 2. Various forms of injection, superficial and deep seated; visible with the naked eye, or requiring some slight magnifying power for discovery. 3. Red or anæmic induration of the two substances, coexisting with increasing weight and size; both states of coloration may be present in different parts of the same organ—a maculated appearance is the result. 4. Collections of pus, especially in the cortical substance; infiltration with the same fluid; ulceration of the mammillæ. 5. Gangrene (extremely rare). 6. Deposition of discoloured fibrine instead of pus (in the traumatic variety).

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* A fact seeming to illustrate very clearly the influence of chronic nephritis on phosphatic urine is that lithic acid calculi (when they remain for any length of time in the passages) become incrusted with a layer of phosphatic salt, and this in spite of the constitutional tendency to lithic secretion. The irritation of the foreign body excites inflammation of the kidneys, and this in its turn the production of phosphatic urine.

When the disease has reached the chronic stage, the following are its anatomical characteristics: 1. Diminished size of the kidney, if the affection be general; hypertrophy is, however, occasionally observed; increased weight and hardness of tissue. 2. Granulated, rough, or tuberculated exterior. 3. Red injection (possibly the result of an acute attack immediately before death). 4. Anæmia, partial or general, ordinarily attended with augmented density, and in many cases with hyperæmia; 5. A peculiar species of atrophy, of which one of the most remarkable characters consists in the appearance of cicatrices on the outer surface of the organ. 6. Ulceration of, or purulent collections in the mammillæ.

Their investing membranes frequently participate in the inflammation affecting the organs themselves—their vessels rarely.

We cannot tarry with M. Rayer's interesting remarks on diagnosis and prognosis, further than to state, that although, as we have already declared, his results on the former topic do not quite realize our expectations, the reader will derive solid instruction from their careful study. In the section on treatment, as was naturally to be apprehended, there is nothing remarkable for novelty in the general plan advised; but there is a sufficient share of practical importance in some of the particular details. Thus we are warned that in puerperal women nephritis has a marked tendency to terminate in suppuration; and that, notwithstanding the state of fatigue and weakness in which the function just performed may have left the patient, the lancet must be vigorously employed, more especially as the inflammation may have existed for some days when first recognized; the lumbar pain it induces may be readily mistaken for a natural consequence of labour.

M. Rayer believes that the best mode of preventing the occurrence of nephritis as a consequence of retention of urine produced by urinary or cerebral diseases, consists in evacuating the bladder carefully from time to time, and in *not* leaving a catheter constantly in the organ. We are glad to have an opportunity of recording so positive an opinion respecting the impropriety of employing the *sonde à demeure*, especially as it emanates from a practitioner of such repute as M. Rayer. When nephritis is induced by stricture of the urethra or prostatic disease, whereby the performance of catheterism is notably interfered with, leeches will be much more beneficially applied to the perinæum than to the loins.

Our author raises his voice against the exhibition of the mineral acids in cases of chronic nephritis with alkaline urine. We perfectly coincide with him in looking on their exhibition, whether as chemical reagents or as general tonics, as of most doubtful utility: they rarely succeed in altering with any persistency the constitution of the urine;* and the system at large almost invariably suffers from the gastric derangement they are so prone to induce. In these cases M. Rayer has directly and indirectly ascertained the excellent effects of nutritious and succulent diet, especially of the *animal* kind. Like all his predecessors, he recommends opium for mitigating the tortures of micturition; the drug may be administered as an enema, a suppository, or by inunction, when its exhibition by the mouth is contraindicated: the frequent use of the emollient hip-bath is an excellent adjuvant to opiates.

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* Berzelius has published a very instructive case illustrative of their failure in this respect.

The cases of acute and chronic nephritis related from the author's own experience, which occupy the remainder of the volume, constitute in our minds its essentially valuable part. They illustrate the obscure points in the history of the disease; they prove how erroneous is the opinion held by even some of the first pathologists of the day respecting its rarity; they exemplify the action of the various causes of the affection; mark out its relations to other disorders, and prove the modifications its symptoms undergo from the presence or absence of certain physiological conditions, such as pregnancy: each category of cases is enhanced in value by a luminous commentary.

Our article has already extended to such length, that we are unable to do more than thus state the nature of the information contained in the closing section of the work: this we can scarcely regret, however, for nothing but an intimate acquaintance therewith—very different from that obtainable even from a close analysis—will enable the student of renal pathology to estimate justly its important bearing on the practical history of the affection.

Of the fasciculi of magnificent plates accompanying the work, it is impossible to speak in terms too strongly encomiastic; both as delineations of morbid changes, and as works of art, they are truly admirable. To the volume we have reviewed are besides appended six engraved plates representing the microscopical appearances of various constituents of the urine, of albuminous lamellæ, of the globules of pus, mucus, and milk, &c.: these are really very convenient for reference, and illustrate excellently well the utility of this mode of studying organic products.

The appearance of M. Rayer's second volume will be the signal for our resuming the subject of renal diseases.

Addendum. Since these pages were sent to press, we have been very kindly favoured by M. Solon with a communication respecting some further researches which he has instituted on the subject of critical urine. Circumstances have as yet prevented him from publishing the particulars of these additional enquiries: we are now enabled to lay their result before our readers.

"The albuminous urine occasionally, and as it were, accidentally, observed during the progress of acute affections, is *not critical.* The presence of albumen in these cases depends either, as Drs. Gregory and Christison presumed, on a morbid state of the kidneys or bladder, on sanguineous exudation, or on a temporary modification of secretion. On the contrary, the urine which I have designated as simply *precipitable,* in contradistinction to that *coagulable by heat,* really announces by its appearance the solution of acute affections. It results from an examination of about 150 cases, that a critical precipitate is obtainable by nitric acid in seven out of every ten instances. When it presents itself, in the course of an acute disorder, in the form of a transverse disk of flocculent appearance, and four or five lines thick, the practitioner may rest satisfied of the approaching establishment of convalescence. This peculiar condition subsists one, two, or three days; subsequently the urine increases in density and usually becomes colourless; finally it gradually reassumes its normal characters."

Hence it appears that our unwillingness to abide by M. Solon's estimation of the *critical* character of albuminuria, when occurring in acute diseases, has been fully justified by the event of further and unbiassed investigation. That it was unbiassed is proved by the result: the honorable candour evinced by M. Solon in thus unreservedly publishing his own refutation redounds infinitely to his credit.

Dr. Christison may have at one period held the opinion ascribed to him by M. Solon, but the observations on the subject in his present volume (p. 40), acquiescing as they do in the views originally adopted by the French writer, prove him to have since relinquished it.

ART. VII.

1. *De penitiori Dentium Humanorum structurâ observationes.* Auctore M. FRAENKEL. Accedit tabula lapidi incisa.—*Vratislaviæ*, 1835. pp. 20.

Observations upon the internal structure of the Human Teeth. By M. FRAENKEL. With Drawings on Stone.—*Breslau*, 1835.

2. *Meletemata circa Mammalium Dentium evolutionem.* Auctore J. RASCHKOW. Accedit tabula lapidi incisa.—*Vratislaviæ*, 1835. pp. 20.

Researches respecting the development of the Teeth of Mammalia. By J. RASCHKOW. With Drawings on Stone.—*Breslau*, 1835.

3. *Archiv für Anatomie, Physiologie, und Wissenschaftliche Medicin.* Von Dr. J. MÜLLER.—*Jahrgang*, 1835.

Archives of Anatomy, Physiology, and Medical Science. By Dr. J. MÜLLER.—For the year 1835.

4. *Mikroskopiska Undersökningar öfver Tändernes särdeles Tandbenets, struktur.* Af A. RETZIUS.—*Stockholm*, 1837. pp. 88.

Microscopical researches on the Structure of the Teeth, and especially of that of Tooth-bone. By A. RETZIUS.—*Stockholm*, 1837.

5. *On the Structure of the Teeth, the Vascularity of those Organs, and their relation to Bone.* By JOHN TOMES.—A Paper read at the Royal Society, June 21, 1838, and printed in the London Medical Gazette for 1839.

6. *On the Structure of Teeth, and the resemblance of Ivory to Bone, as illustrated by microscopical examination of the teeth of man, and of various existing and extinct animals.* By Professor OWEN.—A Paper read at the Eighth Meeting of the British Association for the Advancement of Science.—(*Athenæum*, September, 1838.)

7. *On the Origin and Development of the Pulps and Sacs of the Human Teeth.* By JOHN GOODSIR, Junior. A Paper read at the Eighth Meeting of the British Association for the Advancement of Science. —(*The Edinburgh Medical and Surgical Journal*, January, 1839.)

Fig. 3.

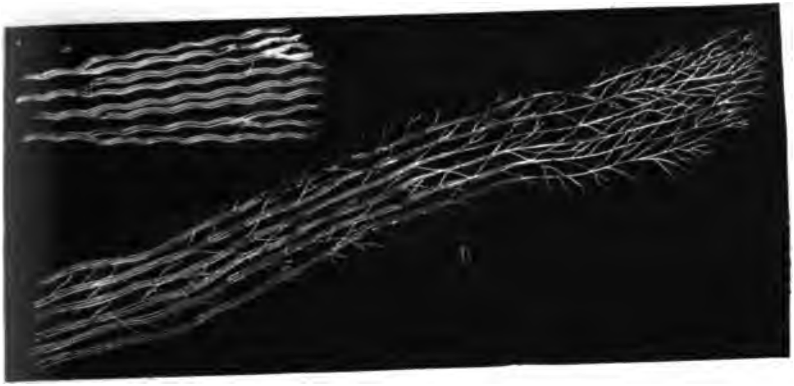


Fig. 4.

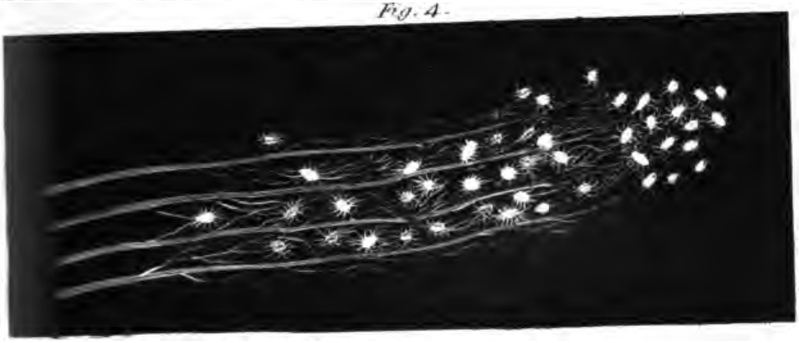


Fig. 5.

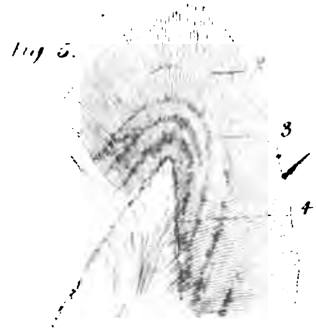


Fig. 1.

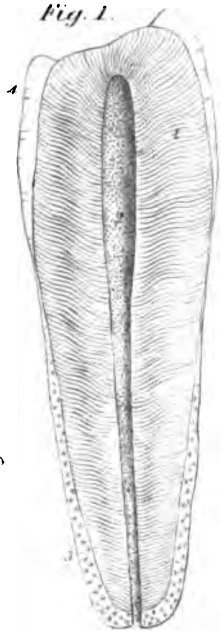


Fig. 2.

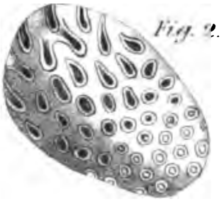


Fig. 9

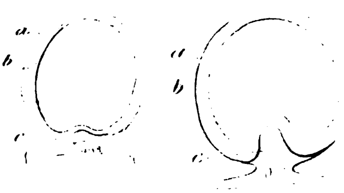


Fig. 6.

Fig. 7.

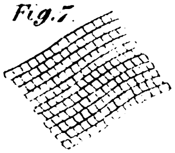


Fig. 10

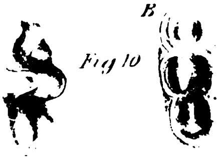
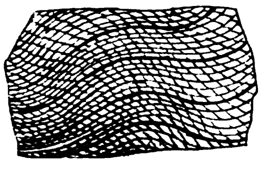


Fig. 8



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[illegible] Medical Times

8. *Elements of Physiology.* By J. Müller, m.d., Professor of Anatomy and Physiology in the University of Berlin, &c. Translated from the German, with Notes, by W. Baly, m.d., Graduate of the University of Berlin. Illustrated with Steel Plates and numerous Wood Engravings. Part IV.—*London*, 1839.

9. *Researches upon the Development, Structure, and Diseases of the Teeth.* By Alexander Nasmyth, f.l.s. f.g.s., Member of the Royal College of Surgeons, &c. Illustrated with numerous Steel Engravings.—*London*, 1839. 8vo, pp. 165.

Investigations respecting the structure and development of the teeth have been carried on during the last few years, by some of the most celebrated anatomists now living; and it is with much satisfaction that we are enabled to place before our readers the highly interesting and valuable results at which they have arrived. In the present article we propose to examine, firstly, the structure, and secondly the development of the teeth.

I. The Structure of the Teeth. In a paper published in the Philosophical Transactions for the year 1678, Leeuwenhoeck announced that, having extracted one of his own teeth, he examined it with lenses, and that he and others "plainly saw that the whole tooth was made up of very small, straight, and transparent pipes;" of these pipes Leeuwenhoeck gave two figures, and he spoke of their existence in the teeth of the cow and of the haddock. In the year 1687, the same author wrote a continuation of the above researches,* in which he further explained the nature of these "pipes," as existing in man and various animals; and he described a molar tooth of the human subject, which was the object of very careful investigation, to contain 4,822,500 pipes or tubes. This discovery of Leeuwenhoeck remained unnoticed during many years. Writers upon the structure of the teeth who succeeded him, and they have been numerous, contented themselves with describing the appearances observed upon examination by means of the naked eye. Purkinje and Retzius, unknown to each other, conducted researches upon the structure of the teeth, with the assistance of the microscope. In the year 1835, the former, through his pupil Fränkel, announced the discovery of the tubular structure of ivory. Nearly at the same time, Retzius, in a series of letters to the Royal Society of Stockholm, published the details of his researches. Müller, in his Archives for 1835, analysed the above researches, and made important additions to them. Mr. Owen has conducted his extensive survey of the same subject into the field of Comparative Anatomy and Geology. Mr. Tomes, another of our countrymen, has also written an original and very interesting paper upon this subject. It is at once a proof of the accuracy of the above observers, and of the perfection which the microscope has attained, that although the use of the highest compound lenses has been found requisite, in order to reveal the more minute structures, all the observers, whose names we have mentioned, agree in every point. We hope that the knowledge of this fact will prove a satisfactory answer to those, who refuse to use the microscope, and to listen to the results derived from its use, "because it is liable to so many fallacies."

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* Antonii a Leeuwenhoeck, Arcana Naturæ Detecta. Vol. iii, 1722. De formatione Dentis Elephantini ac Dentis Suilli: Hominum Dentes esse cavos; Tubulos exiles, dentem conficientes, originem suam habere in cavitate dentis et finire in circumferentiam usque ejusdem. Cavitates dentium nervis, sanguinariis aliisque vasis esse repletas.

After the perusal of Brewster's paper "On the Structure of the Crystalline Lens," published in the Philosophical Transactions for the year 1833, which proved that the peculiar mother-of-pearl colour of that structure depends upon its fibres; it occurred to Retzius, that the same peculiar colour possessed by dental bones might also depend upon the entering into its composition of minute fibres. These minute fibres Retzius readily discovered; and he afterwards proceeded to conduct researches into the structure of the teeth generally.

1. *The Ivory or Bone of the Tooth.* Retzius states that "having made a fine section of a tooth that has been placed in diluted muriatic acid, and having examined it by means of transmitted light, with a lens magnifying as low as 60 diameters, it will be quite apparent to the observer, that it is composed of undulating fibres, in apposition internally with the *cavitas pulpæ*, and externally with the surface of the tooth." If an oblique section be afterwards made, and a magnifying power of 200 diameters be employed, these fibres will appear to be hollow. Retzius found that the above fibres or tubes gave off branches, and that the main trunks opened into the *cavitas pulpæ*. Although these branches are principally evident towards the external surface of the tooth, the canals at their origin may be distinctly seen to present dichotomous divisions. (See fig. 3, A.)

Retzius details the appearances revealed by the microscope in the teeth of man and the lower animals. In man, "the tubes which are contiguous appear to run parallel; upon examining a number, they are found to radiate from the central cavity towards the circumference." These tubes have, more or less, three curves, resembling the Greek letter ξ. These curves vary in different teeth, approaching sometimes in form to the Roman letter S. In well-formed teeth, the curves on each side present a certain symmetry. Independent of these general curves of the entire tube, under a higher magnifying power are observed "numerous shorter curvatures, nearly 200 of which may be counted in the space of a line;" these produce the appearance in each tube of an undulating line. Professor Owen has remarked partial dilatations in the tubes of the human teeth, an appearance which has not unfrequently been noticed by ourselves.

Leeuwenhoeck endeavoured to ascertain how it was possible that tubes, which appear to run parallel to each other, should fill such a different space on the surface of the tooth, and at the internal cavity. He sought in vain for a trace of branches from them. Purkinje discovered, however, that these tubes do give off branches. Retzius says of them "from their commencement in the *cavitas pulpæ*, the tubes appear to be of the same diameter for five sixths of their course;" and this diameter, after repeated admeasurements he states to be $\frac{1}{385}$ of a line. After running five sixths of their course, they are found to diminish considerably in size until they disappear or terminate in small, irregular, rounded, and scattered cells. The branches of these tubes are more easily observable in milk teeth. (See fig. 3, B.) In the permanent teeth, the branches are formed principally towards the extremities of the tubes; in the milk teeth they arise nearly equally from all parts (fig. 3, B). The branches arising from different tubes do not communicate with each other, except perhaps at their extremities.

Are these fibres tubular? Retzius states that the *cavitas pulpæ*, viewed under a sufficiently high magnifying power, presents numerous orifices which he considers to be the mouths of the main tubes (see fig. 1). He also states that, by making sections of the tubes at right angles with their course, the caliber of these tubes may be determined with facility; and that, upon placing a section made in the above manner upon a dark ground, white spots distinctly denote the tubular orifices. Purkinje and Fränkel convinced themselves of the tubular nature of these fibres, from finding, on making various sections of the tooth, that they were always able to demonstrate the parietes and cavity of the cut tube. Müller states that he has seen these fibres or tubes in the horse, injected with red colouring matter. Mr. Tomes has made some conclusive experiments upon this subject. After having reduced a transverse section of a giraffe's tooth, so as to render the tubes visible by the aid of transmitted light, he added to it, while under the microscope, diluted muriatic acid. "Chemical action immediately commenced, gas was disengaged and proceeded from the cut extremities of the tubes." He states that, more than once, he saw "the bubbles of gas in the tubes, and traced them to their extremities from which they escaped."

The Parietes of the Tubes. Müller has made some researches upon this subject, which are of much interest. He states that the diameter of the tubes is only one fifth or one sixth of the spaces between them; the structure in the intervening spaces necessarily forming the greater part of the bulk of the tooth. He says that, upon breaking fine sections of teeth perpendicular to their fibres, he has frequently seen the latter "extend from the margin beyond the tooth-substance, seeming to be perfectly straight and inflexible;" but the earth being removed from slices of teeth, by means of acids, and the latter being torn in a direction contrary to the course of the fibres, "these fibres appear, upon the torn margin, quite flexible and transparent, and they not unfrequently project considerably beyond the margin of the section." Müller thinks that it may be inferred, from the above experiment, that the tubes have an animal basis or membrane, which in the firm tooth is fragile, and probably penetrated by calcareous salts. Upon this subject Retzius says that, under a certain light, "the mouths of the canals appear bounded by a definite shadow;" and their walls, when the light falls upon them, have a different appearance from the surrounding matter, which Fränkel calls the fundamental substance of the tooth. Frequently the circular orifices have a darker and somewhat yellowish appearance. "We may conclude," says Retzius, "from all this, that the above-described tubes are not merely hollowed out of the fundamental dental substance, but that they are tubes properly so called, consisting of a particular substance, differing from that of the tooth."

The contents of the Tubes. Müller was the first who made investigations upon this branch of the subject. He considers the tubes to be filled with organic deposits of calcareous salts, which are soluble in acids. According to him the white colour of the tooth is dependent upon these contents, the intervening substance being more or less transparent. He says, "The white colour of the tooth disappears upon the application of acids; and teeth thus treated do not regain their colour when dried." Observers agree that even if the enamel only be carious, or rather the ivory immediately internal to the enamel, then the tubes passing towards the *cavitas pulpæ* lose their white colour. Müller states that he has seen in carious teeth, independently of the transparent appearance above alluded to, a brittle substance contained in different parts of the tubes; he has, however, also observed the latter in perfectly healthy teeth, and he also speaks of the existence of opaque spots in the course of the tubes. According to Retzius, the contents of the canals consist of an inorganic or earthy substance, which appears white when seen on a dark ground. It appears to consist of "small masses, formed of infinitely delicate particles." The greater or less number and the degree of visibility of these particles appear to depend upon the extent to which the preparation has been penetrated by water, oil, or turpentine. Mr. Tomes agrees with Retzius as to the contents of these tubes. In mammalia he states them to consist of an amorphous mass, the composition of which is the phosphate and carbonate of lime. He concludes that the latter enters into the formation of the above mass, from the results of the experiments which we have before noticed, where he placed a thin section of the tooth of the giraffe in muriatic acid, and observed under the microscope the evolution of gas from the interior of the tubes. Mr. T. states that these tubes in the cartilaginous and osseous fish contain much less of the earthy matter than do those of animals higher in the scale.

The use of the Tubes. Retzius believes that the tubes of teeth, as well as the canals of bone (canals of Havers), are for the circulation of nutritious fluids, which he imagines to be secreted by the capillaries which clothe the surface of the pulp. We quote from Retzius the following interesting observations upon this subject:

> "We have many examples," he says, "that Nature organizes structures which have a close affinity to each other, according to one and the same plan, and hence we have, in different parts, organized formations, which in some are of the greatest importance, whilst in others they are of much less functional significance, or of none whatever. When we assume, what is highly probable, that in bone the peculiar vessels in question give passage to fluids during the entire life of the animal, which fluids contain the solid as well as the liquid materials of the osseous substance, it does not necessarily follow that the same process must be carried on in the teeth during the whole of life; on the contrary, I am inclined to believe that the vessels of dental bone exercise their most perfect action during the first period only of the formation of the tooth. At the same time, the existence of the continual vital process in the tooth, as well as in the crystalline lens, cannot be doubted, which appears, however, to be carried on without any constant exchange of solid matter, and must hence consist in a renovating circulation of fluids."

Mr. Tomes "conceives that the tubes, containing as they do an amorphous substance, could, by capillary absorption, carry on a kind of slow circulation of the more fluid parts of the blood." Professor Owen accounts for the good effects produced by stopping decayed teeth, by "the calcareous salts in such cases pouring out from the extremities of the tubes divided in the operation; then a thin dense layer intervenes between the exposed surface of the ivory and the stopping."

Of the Intertubular Substance. Independently of the salts of lime contained in the cavity of the tubes, and of those which, with the animal basis, form the walls of these tubes, Müller considers that salts of lime also enter into the composition of the intervening substance; and, as we have seen above, that the tubes themselves are only of a size equal to one fifth or one sixth of the space between them, the greater portion of calcareous salts which enter into the composition of the tooth must be contained in the intertubular spaces. Müller considers the lime to be either chemically combined with the cartilage of the tooth, or to be deposited in it in an insoluble manner. The lime-earth of the intertubular spaces may be demonstrated by carefully boiling slices of teeth in potash during many hours. The cartilage is dissolved, and the intervening substance becomes opaque and white. According to Müller, the lime thus obtained appears in the form of dense grains. Between the tubes are observed also the *corpuscles or osseous cells of the tooth*, called by Professor Owen the *calcigerous cells*. Some of the smaller tubes terminate in these cells (see fig. 4). Valentin believes them to be analogous to the corpuscles of bone. Retzius states "that they, as well as the tubes, contain earthy salts." This he proves by causing their white colour to disappear, by placing the preparation in diluted muriatic acid.

The Terminations of the Tubes, according to most observers, take place—first, by entering the corpuscle or cell; secondly, by forming plexuses with one another; thirdly, by being lost in the intertubular substance, either in the interior of the ivory or at its surface of attachment to the enamel and crusta petrosa; fourthly, by communicating with the cells of the crusta petrosa.

2. *The Enamel of the Tooth.* This is also called the vitreous and adamantine substance. Berzelius states that enamel *does not contain any animal matter.* He considers that the delicate tissue which remains after its earthy matter has been dissolved in acid is the residue of a membrane which formed the internal lining to it. He says that "enamel is not at all blackened on the outside, and but very slightly on the inside, by being burned; and that it does not lose more than two per cent. of its weight from combustion." Retzius, with Berzelius, agrees in the existence of a membrane which lines the enamel internally, and which is a band of connexion between it and the external surface of the ivory. Retzius believes that the inorganic fibres of enamel are surrounded by a capsule of organic matter; a view which we shall see is adopted by Purkinje and Raschkow. The researches of Mr. Nasmyth* show the existence of a layer of substance external to the enamel, and intimately connected with it. This layer Mr. N. has demonstrated in the tooth of the human subject, in the calf, and in many other animals; and he considers its presence to be almost universal. It is shown by the immersion of the tooth in diluted muriatic acid during a very short space of time, when it can be removed in the form of a membrane. It is continuous with the layer called the *crusta petrosa* on the surface of the fang, to which it seems to bear the greatest analogy, and, like the latter, contains the characteristic cells or corpuscles. The existence of this layer on the surface of the enamel, as pointed out by Mr. Nasmyth, we have since demonstrated, by exposing teeth to a rather high temperature, when a

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* In a paper read at the Medico-Chirurgical Society.

very thin layer of black substance is recognized on their external surface; it is more easily perceived, and it is thicker in young than in old teeth. Retzius agrees with Purkinje and Raschkow, in believing that the enamel, previous to the eruption of the tooth, consists of delicate prisms; each prism, upon its solution in muriatic acid, leaves behind a small portion of organic matter. As the latter is not found at a later period, Retzius supposes that it is supplanted by the deposition of earthy matter, so as to remain in a quantity so small as to be incapable of demonstration. The enamel fibres are described by Retzius to be $\frac{1}{461}$ of a line in diameter, the surface of which is marked with transverse striæ (see fig. 7), which Retzius believes to be produced by the above-mentioned organic sheath. The fibres of the enamel internally rest upon a delicate membrane interposed between the latter substance and the ivory (see fig. 5). Externally, these fibres present an hexagonal form, which is seen by the aid of a lens, magnifying 300 diameters (see fig. 6). Where the tooth has not been worn away, the extremities of these fibres are rounded. The enamel fibres are supported upon the ivory in different directions. Towards the apex of the tooth the direction is vertical, and the lowest fibres are transverse (see fig. 5). In the lateral part of the tooth there are fibres which are interposed between other fibres, and which themselves do not reach the surface of the ivory. Where the enamel joins the dental bone, it contains at different parts a number of delicate crevices, which appear to have their origins in the separation of the fibres, and produce a dentated appearance, similar to that of the crystalline lens (see fig. 1, iv). Retzius states that this appearance is rendered more evident by the immersion of the enamel in a solution of caustic potash, a circumstance which strengthens his belief in the existence of an organic sheath to the enamel fibres. Independently of the transverse striæ on the surface of each enamel fibre, the enamel presents a lamellated appearance, analogous to that of a calculus (fig. 5, iii); this is also evident in ivory generally, but more particularly in that of the elephant's tusk. The fibres of the enamel, like those of the ivory, are more or less tortuous in their course (see figs. 7 and 8).

3. *The Crusta Petrosa.* It is universally known that ivory and enamel form the principal part of all simple teeth; and previously to the recent investigations in odontology, it was supposed that no other substance was present in the latter. Retzius and Purkinje have, however, proved that a third substance enters into the formation of almost *all teeth.* This substance is the *crusta petrosa* or *cortical substance.* Frederic Cuvier spoke of this structure as existing in the cachalot. It is found on the external surface of the fangs of the teeth of the mammalia. It consists of cartilage and of osseous earth. According to Retzius "this cartilage may be removed from the fang of the tooth in the form of a membrane, its earthy salts having been previously dissolved in acid; its consistence is less than that of the cartilage of ivory. In the human tooth, it is an extremely thin stratum which takes its origin at the neck of the tooth where the enamel terminates; it gradually increases in thickness towards the extremity of the root where it is generally the thickest (see fig. 1). It is thinner in young than in old persons. In proportion as the *cavitas pulpæ* diminishes, this substance is developed until it sometimes becomes thicker than the dental bone itself." The exos-

toses so frequently met with on the fangs of the teeth are of a composition similar to this structure. It is imperfectly formed in the deciduous teeth. Examined by means of the microscope, it presents tubes and osseous cells, bearing the greatest analogy to those of bone and ivory. Professors Retzius and Owen have seen direct communications between the fine branches of the ivory and the tubes and cells of this substance. Mr. Nasmyth states that it always exists on the surface of the enamel; and Purkinje and Fränkel "once traced it a short way upon the surface of the enamel of the tooth of an old man." The two latter observers state that the *crusta petrosa* has a lamellated structure; they have found it lining the *cavitas pulpæ*, and have given it the name of *substantia ostoidea*. Retzius thinks that after the obliteration of the *cavitas pulpæ*, the crusta petrosa is the medium through which nutrient fluids are conveyed to the tooth.

The Cementum. The structure of which we have just spoken is generally considered to exist upon the root or fang of the tooth only, and to terminate at the point where the enamel is secreted, viz. at the neck of the tooth. As has been stated above, Purkinje and Fränkel, however, once saw this substance covering a small part of the enamel of the tooth of an old man, and Mr. Nasmyth describes it as existing upon the surface of the enamel in most quadrupeds; and he has exhibited specimens and drawings of this disposition in the simple and compound teeth of the human subject, and in the simple tooth of the calf, &c.* The cement is a deposition on the surface of the enamel, found in the compound teeth of the herbivorous animals; it has been described by Hunter, Blake, and many other anatomists. Cuvier, in his "Ossemens Fossiles," has given a very minute and interesting account of its development and structure in the grinder of the elephant. It is formed by the capsule, as appears to be the crusta petrosa; it possesses corpuscles and tubes, as does the crusta petrosa; in short, it appears to us that the cementum is simply the crusta petrosa existing on the surface of the enamel; not only, as has been hitherto supposed, in the compound teeth of herbivorous animals, but according to the views of Mr. Nasmyth, in the simple and compound teeth of mammiferous animals. Müller most strangely describes the structure under consideration "as a deposit from the salts of the saliva, and to be essentially the same as what is called tartar in the human subject."

We have given above a condensed account of the views of all recent investigators into the structure of the teeth. We regret that we are prevented by want of space from entering upon the field of Comparative Anatomy, a field in which Professors Retzius and Owen have pointed out many most interesting and valuable facts.

II. THE DEVELOPMENT OF THE TEETH. The researches in this branch of odontology which have lately been published, and to the examination of which we shall at present confine ourselves, have an interest not much inferior to those of which we have already spoken. Much, however, in this department remains to be done; and observers have yet to test the accuracy of the novel views which we are about to relate.

We shall speak—1st, of the Development of the Ivory of the Tooth; 2dly, of the Enamel; 3dly, of the Crusta Petrosa.

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* A paper read at the Medico-Chirurgical Society.

The Development of the Ivory of the Tooth. The ivory of bone is formed by an organ called the pulp of the tooth. This pulp is, at an early stage of development, contained in a cavity called the follicle, from the base of which it grows, and to which it is attached by vessels and nerves. According to Arnold and Goodsir, this follicle is thus produced:—at a very early period of development (between the fifth and seventh week) in the human subject, a groove is observed on the free margin of the alveolar arch; from the floor of this groove the pulps of the teeth grow; after a certain period, septa are formed, which separate the pulps from each other, and divide the groove into a series of follicles, which continue to have a communication with the mucous membrane of the mouth, until a much later period. Arnold has seen the orifices of these follicles quite distinct in a child at birth. The researches of Mr. Goodsir have been conducted on a very extensive scale, and he has published delineations of the development of the follicle in all its stages; at the time that he conducted his dissections, he was not aware of the views of Arnold which appeared in the Salzburg Med. Zeitung for 1831. Purkinje and Raschkow are quite opposed to the above view of the development of the follicle, as described by Arnold and Goodsir. They state that they "have looked with the greatest care, and have made researches in Comparative Anatomy, but have never discovered a trace either of the groove or of the orifice of the follicle." Raschkow states that "the membrane of the follicle consists of soft fibres mixed with much granular matter; at the earlier periods of development, this follicle is not united with the gum; its internal surface is smooth like a serous membrane."

The Pulp or Dental Germ. The pulp is developed in the interior of the above-mentioned follicle, which, when the latter assumes the name of *capsule*, Raschkow considers "the pulp to be a production of the interior of the capsule, inasmuch as the two are inseparably connected, their vessels and nerves having a common origin." He denies that the dental germ is a continuation of the dental nerve. The following is a condensed account of the views of Raschkow on the pulp and its functions. This organ, at its earliest period, consists of globular granulations, in which are not observed either vessels or nerves; afterwards the vessels appear and then the nerves. The granules of nervous matter, in the interior of the pulp, assume the form of nervous cords, after the vessels are completely formed. In no part of the body are the extremities of the nerves so well seen as in the pulp, when it is mature. The nerve upon entering the pulp divides into filaments, the latter into a pencil of rays, in which they terminate. These filaments are accompanied by a cellular web. In the interior of the pulp of the hare, sow, and stag, stony concretions may be observed towards the apex. A membrane covers the dental pulp from its base to its apex; in this membrane the formation of the dental substance always commences. This membrane Raschkow calls "membrana præformativa," and it is described by him as being very dense. Raschkow has observed upon the surface of this membrane, previously to the formation of the dental substance, certain elevated spots. These he considers to be most probably transformed into the undulating ridges on the surface of the dental substance, to which the enamel is attached. According to Raschkow and Purkinje, the dental substance is formed immediately under the preformative membrane, the latter assuming an almost stony hardness; between the dental germ and this ossified membrane, a layer of dental fibres is deposited from without inwards, the parenchyma of the dental germ supplying the materials. It will be seen that the views of the above observers differ much from those of preceding writers. Hitherto it has been supposed that the membrane covering the surface of the pulp, and intimately adhering to it, is the secreting organ of the ivory. Observers have accounted for the peculiar waving course of the tubes in ivory by supposing that the pulp undergoes certain periodic movements.

Development of the Enamel. We have seen that the first stage in the development of the tooth consists in the formation of a follicle or capsule, from the base of which grows an organ—the pulp, the function of which is to secrete the ivory of the tooth. Contemporaneously with the appearance of the pulp is the formation of another structure, a growth from the internal layer of the capsule; it is the formative organ of the enamel. It appears directly opposite to the ivory pulp, was called by Hunter the "enamel pulp," and by Purkinje and Raschkow the "adamantine organ." The researches of the two last-mentioned authors are highly interesting. They state that the "adamantine organ appears as a globular nucleus, having a granular structure, projecting into the cavity of the capsule towards the pulp of the ivory;" and that it eventually is converted into the membrane which secretes the enamel in the following manner: "It throws off towards its internal surface a stratum of fibres, producing the appearance of a silky covering. These fibres are eventually converted into a membrane—the membrane of the enamel. It has no traces of vessels or nerves. If examined with the microscope it will be found to consist of hexagonal bodies of almost equal size. Each of these fibres must be considered as a *gland*, whose function is to secrete a single enamel fibre, corresponding with itself. Each *gland*, simultaneously with the earliest formation of the dental substance, deposits the primitive part of each adamantine or enamel fibre, one upon another, so that every one of these fibres, when carefully examined by the microscope, displays the order of its parts arranged in strata in a transverse direction." The enamel is first deposited upon the hardened preformative membrane mentioned above, and the progress of its formation corresponds exactly to that of the development of the ivory. At an early stage of the production of these enamel fibres, the organic substance is a lymph which enters between each individual fibre, and appears to soften its entire substance; this organic substance appears to be formed by the parenchyma of the adamantine membrane between the above-named *glands*." Purkinje and Raschkow suppose that this animal substance, by means of a chemico-organic process, enters into close connexion with the earthy matter, and thus forms the animal basis of the enamel, which is evident, by means of the microscope, after the latter has been placed in acid. These observers account for the production of the peculiar waving fibres of the enamel by the occurrence of certain movements in the adamantine membrane. The adamantine membrane is permanent in the incisors of the rodentia. Previous to the formation of the adamantine organ (see fig. 10, A and B), it is supposed that the rudiments of this structure exist in the form of a serous sac (see fig. 9, A and B).

Development of the Crusta Petrosa and Cementum. Upon this branch of the subject much remains to be done. No new facts have been adduced since the time of Cuvier. The crusta petrosa and cementum are formed subsequently to the completion of the ivory and enamel; but whether the organ which produces them is a modification of those which formed the latter, or one for their especial development, anatomists have not yet determined.

The result of the investigations above detailed is that the structure of the tooth is very analogous to that of bone. We shall refrain from making any observations upon this analogy until we have laid before our readers a view of the recent discoveries upon the structure of the latter tissue (and which we hope to do in an early number). Although we possess so full an account of the internal composition of the teeth, we are very deficient in researches into the development of each individual part of the tooth, as well as of the organ generally: we have noticed above that Purkinje and Raschkow entertain opinions directly opposite to those of Arnold and Goodsir; in fact, we believe, that upon no subject is there a greater diversity of opinion than upon the nature and functions of the structures in connexion with the development, growth, and organization of the dental apparatus. In support of this assertion, we refer to the first part of the work by Mr. Nasmyth. The cause of the strange discrepancies of opinion therein detailed, appears to arise from the too partial examination of the subject before us, which has been conducted by various observers.

Mr. Nasmyth promises to prosecute an extended series of researches into every branch of odontology; in doing so we feel sure that he will reconcile many of the present conflicting opinions, and we hope to receive from his hands, what is much wanted in medical literature, a Systematic Treatise on the Development, Structure, and Diseases of the Teeth.

To conclude: to those who wish to pursue the subject which we have so briefly brought under notice, we recommend the first part of the work of Mr. Nasmyth, as containing an entire translation of the papers of Retzius, which are illustrated by many beautiful and original plates; also a complete view of the researches of those whose names we have introduced in the present article; and, lastly, a comprehensive historical survey of all works on odontology.

Explanation of the Plate.

Fig. 1. Section of a human molaris divided in the direction of the axis of its pulp cavity, magnified four diameters. 1. The ivory showing the direction and larger curves of the dental tubes. 2. The cavity of the pulp showing the openings of the dental tubes. 3. The cortical substance surrounding the fang as high up as the border of the enamel. 4. The enamel.

Fig. 2. A small portion of a section of a similar tooth to the above, made perpendicular to the course of the dental tubes, showing the openings of these tubes and their parieties, distinct from the uniting substance. The tubes to the left hand are cut obliquely.

Fig. 3, A. Innermost portion of the dental tubes, from the incisor of a child two years of age, showing their dichotomous division. B. External portion of the tubes, showing their ramifications.

Fig. 4. External portion of the dental tubes from the incisor of a full-grown horse, showing the numerous cells in which the dental tubes terminate.

Fig. 5. Upper portion of a longitudinal section of an incompletely formed incisor taken from the follicle. 1. The ivory and dental tubes. 2. The enamel. 3. Parallel curves of the enamel, giving rise to the appearance of concentric lines when a lower magnifying power is used. 4. Small depressions and points on the surface of the ivory upon which the fibres of the enamel rest.

Fig. 6. A portion of the surface of the enamel, showing the hexagonal ends of the fibres.

Fig. 7. Fibres of the enamel seen laterally with a magnifying power of 350 diameters, showing the transverse striæ upon them. (The above figures are from Retzius.)

Fig. 8. A section of enamel parallel to the outer surface of the tooth. (Fränkel.)

Fig. 9, A. The dental follicle in its early state. The adamantine organ *b*, as yet, a closed sac: *a* denotes the capsule: *c* the dental germ. B. The dental follicle in a more advanced stage.

Fig. 10, A. Adamantine organ from the surface of the grinder of a fœtal calf of nine weeks. B. The free surface of the above tooth. (The last four figures are after Raschkow.)

ART. VIII.

1. *The Works of* JOHN HUNTER, F.R.S.; *with Notes by* JAMES F. PALMER. In four Volumes. Vol. III. *A Treatise on the Blood, Inflammation, and Gunshot Wounds.—London*, 1837. 8vo, pp. 586.
2. *A Treatise on Inflammation.* By JAMES MACARTNEY, M.D. F.R.S. F.L.S. M.R.I.A. &c. &c.—*London*, 1838. 4to, pp. 214.
3. *Illustrations of the Elementary Forms of Disease.* By ROBERT CARSWELL, M.D., Professor of Pathological Anatomy in University College, London, &c. Fasciculus, XII. *Inflammation.—London*, 1838. Folio, pp. 8.
4. *Teoria della Flogosi, di* GIOVANNI RASORI.—*Livorno*, 1837. 8vo, pp. 260.

Theory of Inflammation. By J. RASORI.—*Leghorn*, 1837.

IF any pathologist were asked what he regarded as the most complete department of his science, that most reduced to general principles, and consequently furnishing the most certain rules for the therapeutic art, he would probably name, without much hesitation, the subject of inflammation. And yet a very little attention must convince the discerning practitioner how imperfect is even this best-understood department of pathology; and how liable are its precepts to fail of success, even when the apparent simplicity of the case would lead him to place the most implicit reliance upon them. Take, for example, the local treatment of an acute sthenic inflammation; how purely empirical is it! One surgeon will apply cold, another warmth; one will use astringent or strong stimulants, whilst another finds more satisfaction in emollients and sedatives. And, after all, it will probably be by the feelings of the patient, and the remedial efficacy of the means, that the continuance of any particular plan will be decided on. If we turn to the pages of authors on the theory of inflammation, we shall no longer feel surprised at the uncertainty of the therapeutic rules with which they supply us. Few subjects have been more keenly debated; few debates have ended in a less satisfactory conclusion. To what cause can this result be attributed? We have ourselves no doubt that it is chiefly owing to the neglect of the study of the natural or physiological condition of the functions whose pathological states have occupied almost undivided attention. We cannot hesitate in the belief that if half the acuteness of observation and of the sagacity in the interpretation of phenomena, which have been bestowed upon the subject of inflammation, had been devoted to the establishment of truth in regard to the functions of circulation and nutrition, our pathology would be now possessed of the means of acquiring far more consistency in theory, and more certainty in practice. If we do not hold with Broussais, that inflammation is the foundation of every derangement to which the organized fabric is liable, still it is unquestionably the most important of all elementary forms of disease, constituting a very large proportion of the maladies which affect the human frame, and participating in almost every other.

Those who decry the pursuit of physiology as unprofitable, who think that all the student's attention should be devoted to the study of the morbid conditions of the body, and who imagine that they can rectify the errors of the organic mechanism, as well (if not all the better) in their ignorance of its regular operations, may here be brought to confess the uncertainty of any doctrines founded upon such observation alone, by being required to explain the rationale of the success of two opposite modes of local treatment, possessing the empirical character we have just described. We shall not do our readers the injustice of seriously arguing with them in favour of a position so self-evident as that a good pathology can only be founded on a good physiology; but before entering upon the consideration of the subject which we are to review in this article, we think it desirable to give a brief exposition of the present state of our knowledge in regard to these healthy actions, a peculiar derangement of which constitutes the diseased condition termed INFLAMMATION. We are the more desirous of so doing as we believe that there are few physiological questions respecting which there prevails more either of positive error or vague misapprehension. Without aiming, then, to bring forwards anything of a very original character, we shall endeavour to determine what may be regarded as sufficiently certain and definite to stand as the foundation for pathological generalizations. In so doing we shall only be following, as will presently appear, in the same track with John Hunter.

In the first place, then, we must enquire what are the forces by which the blood is moved, and what are the physical and vital properties of the vessels through which it passes. A great deal of useless speculation on the first of these questions might have been saved, had physiologists brought to the investigation something more than that vague knowledge of physical science which has led some among them, even at no distant period, into most absurd hypotheses. The motor forces assigned by the leading physiologists of the present day may be regarded as originating in, 1, the heart; 2, the arteries; 3, the capillaries. Regarding the first, we need here say but little. No one has any doubt that, in warm-blooded animals, its contraction is the principal agent in the propulsion of the blood to the periphery of the system; and that it operates in a manner strictly mechanical. It is evident, also, that any variations in its operation must influence the whole system alike, except so far as these are antagonized by local peculiarities in the conformation of the blood-vessels. But there is by no means the same certainty that the movement of the blood in the veins is solely due to the *vis a tergo* communicated from the heart, through the capillary system; and still less does it seem probable that this cause can operate where the blood has to be propelled through a double or triple set of capillaries by one impulse. In fishes, for instance, the blood is first transmitted through the respiratory capillaries, for the purpose of aeration; the confluent vessels which collect the blood from these terminate in the general systemic trunk or aorta, in which no pulsation is seen, and by this it is distributed to the systemic capillaries. The blood which returns from the posterior part of the body and from the viscera passes through another set of capillaries, those of the liver and kidneys, before it returns to the heart, the muscular power of which would seem far too weak to overcome such resistance. Indeed, the non-pulsation of the intermediate trunks, the aorta and vena porta, shows that the heart's impulse is not sensibly communicated to them; and the same may be said of the vena porta in birds and mammalia, which, as Hunter has justly observed, should be regarded rather in the light of an artery, strongly resembling, as it does, the aorta of fishes.

As John Hunter was the first who gave due attention to the properties of the arteries, we shall follow him in some detail through his chapter on the vascular system, of which he takes a very comprehensive survey. The following list of the titles of the sections will afford a general view of its contents: 1. General observations on Muscular Contraction and Elasticity. 2. General observations on the elongation of relaxed Muscles. 3. On the structure of Arteries. 4. Of the Vasa Arteriarum. 5. Of the Heart. 6. General observations on the Blood-vessels. 8. Of the division or branching of Arteries. 9. Of the action of the Arteries, and the Velocity of the Blood's Motion. 10. Of the Veins. These titles will show the comprehensive scope of the contents of this most valuable chapter. That many errors of opinion and some of fact have found their way into it, few will deny; but, notwithstanding all that has since been written on the subject (of most of which Mr. Palmer has supplied us in his notes with an excellent condensed view), no one can be regarded as master of it who has not made himself well acquainted with Hunter's experiments and reasonings. To some of the more important questions here discussed, we shall now direct our reader's attention.

After making some general observations on muscular contraction and elasticity, and on the elongation of relaxed muscles, he proceeds to treat of the structure of the arteries. These he regards as made up of elastic and muscular fibres. The elastic substance is most abundant in the large vessels and in those nearest the heart, gradually diminishing as the distance from this organ increases. From his own observations and experiments, he was led to think that the smaller arteries, or "what have been called capillary vessels," were entirely devoid of an elastic coat, and made up principally of muscular fibres. From the same experiments he also discovered that the larger arteries possessed little muscular power, but that, as they receded from the heart, the muscular power was gradually increased and the elastic diminished. Hence he imagined that there might exist a set of vessels totally devoid of elasticity, although this he conceived to be only in the very extreme parts. He has left no account of the minute and capillary vessels, an omission which, we are sorry, has been but imperfectly supplied in the present edition by a short commentary. Hunter carefully distinguished the elastic from the contractile, or, as he names it, muscular powers of arteries, and has assigned to each their relative importance and use. Elasticity is best adapted "for taking off the immediate force of the heart," and, therefore, predominates in the larger vessels more immediately connected with this organ. The muscular power of an artery, according to him, "renders a smaller force of the heart sufficient for the purposes of circulation; for the heart need only to act with such force as will be sufficient to carry the blood through the larger arteries, and then the muscular power of the arteries takes it up, and, as it were, removes the load of blood while the heart is dilating." Arteries, therefore, afford the most striking instance of an animal substance furnished with two powers existing in the same part, the one to resist mechanical impulses, the other to produce action where action is principally required. Hunter considered that the heart was unable to carry on the circulation without the aid of the arteries; and it was for this reason that the latter, as they decreased in size, were endowed with a greater contractile power. On the question of the agency of the arteries in maintaining the circulation of the blood, we shall extract the following remarks made by Mr. Palmer, which will show the care and ability with which he has executed this portion of his task.

"The arteries were regarded by Hunter as a sort of supplementary heart, scarcely less instrumental in the propulsion of the blood than the heart itself, although it is probable that in this estimate he was as much mistaken on the one hand as Dr. Arnott has lately been on the other, in denying their agency altogether (*Elements of Physics*, 5th edition, p. 358). I shall not, however, enter here into the controverted question respecting the muscularity of the arteries, which appears to me to be a dispute about words rather than facts; for that the arteries possess a power of varying their caliber quite independently of the force *a tergo* is proved by the most indisputable facts. Admitting, therefore, the muscularity of the arteries as a fact, the question arises, in what manner is the circulation affected by it? It is objected to the author's explanation of the action of the arteries, 1st, that if the arteries were extensively elastic, the intermittent stream in the arteries would soon be converted into a continuous one: 2d, that no vermicular or progressive motion in the arteries has yet been observed; 3d, that no diametrical enlargement is visible in the living body; and 4th, that it is impossible to explain the almost perfect instantaneity of the pulse in all parts of the body, on the supposition that the tubes through which it is propagated are in a relaxed state. It is supposed, on the contrary, that the contractile fibres of the vessels operate at the same moment with the heart, so as to induce a rigidity of the whole arterial system, in order that the heart may produce its effect through all the vessels, almost as it would through tubes of metal (*Arnott*, ut supra). Now the first of these objections does not apply, because such extensive elasticity as is supposed to exist does not occur, for even in the dead body the pulse may be imitated; neither does the second objection apply much better, because the very assumption that the pulse depends on the transmission of a wave of blood, presupposes a certain interval of time, and consequently a progressive dilatation. The third objection has been adverted to already, (see note, p. 216;) to which I may add that Poiseuille found, by direct and careful experiment, that the artery apparently dilated with each systole of the heart, that it reacted with a force superior to the impressing impulse, and that the arterial tension was nearly the same in all parts of the system at the same time. The fourth and last objection equally falls to the ground, when it is recollected that no such rigidity, as is supposed to be necessary for the propagation of the pulse, actually occurs, for the isochronism of the pulse is equally conspicuous in all states of the arterial tension, and is scarcely less remarkable in the dead body." (Vol. iii. p. 229.)

There is little doubt, we think, that Hunter was right in ascribing to arteries a property somewhat analogous to that of muscular irritability, and recent enquiries into their intimate structure would seem to justify this doctrine. It is true that the authority of Berzelius should lead us to regard their middle coat as destitute of fibrine, which has usually been considered as the characteristic ingredient of muscular fibre: but we fully agree with Dr. Allen Thomson in the opinion that "although we must admit the importance of such observations, as establishing chemical distinction between muscular substance and the texture of the middle coat of the arteries, they do not warrant the conclusion too hastily deduced from them by some, that this coat cannot be irritable, or does not possess any of the same properties as muscle, the existence or non-existence of which must be ascertained principally by physiological evidence."* Although the structural difference has, by most observers, been regarded as equally decisive, it would appear to have been so on insufficient grounds. The comparison has been made with the fasciculi of the voluntary muscles, in which *fibres* are seen made up of *fibrillæ*, inclosed in a sort of sheath, and bound together by annular or spiral bands, which have given the appearance of striæ. In the muscular fibre of the alimentary tube, however, as in that of the lower animals, the fibrillæ are separate, unmarked by striæ, and irregularly interlaced instead of lying parallel; and in all these respects they closely correspond with the fibrous coat of the arteries.† Between these tissues, again, there is more physiological resemblance than either possesses to the fibre which composes the voluntary muscles. Both of the former are acted on by direct stimuli, and not at all, or at least very imperfectly, through their nerves; whilst the contraction of the latter excited by direct stimuli is very trifling, and can only be effectually induced by an irritation transmitted through the nerves. We shall not detail the experiments which prove that a property of this kind exists in arterial tissue; they are to our minds quite satisfactory; but the question then arises, what influence it has in causing or modifying the circulation of blood through the vessels. It is universally conceded that to their *elasticity* only is due the gradual equalization of its flow, and the diminution of the pulse-wave, as we proceed from the centre towards the circumference. We do not think that there is evidence of any action like that of the pulsating

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* Cyclop. of Anat. i. p. 665.

† Skey in Phil. Trans. 1837; and Mandl, Anatomie Microscopique.

dorsal vessel of insects, in which a kind of peristaltic or vermicular contraction propels the blood from behind forwards,—the vessel being, in fact, a series of hearts, one for each segment, which come into play consecutively. Still it does not appear that mechanical distention stimulates the vessels to contract again with more than the force which dilated them; and this, acting through the whole arterial system at once, may, as Hunter supposed, be an important auxiliary to the propelling power of the heart.

But we cannot agree with many physiologists in attributing to this secondary influence all that is required to make up for the deficiency in the heart's action. A more important source of movement, and one which has been very generally overlooked or undervalued, originates in the capillary system; but before enlarging upon this, we must advert to what we conceive to be the principal office of the muscular coat (for so we feel justified in terming it) of the arteries, and especially of the smaller branches where it is most developed. We fully accord with Mr. James* (to whose treatise Dr. Macartney seems to us to be under more obligation than he has acknowledged,) in thinking that the elastic coat of the arteries enables them to "enlarge or diminish their caliber, to *regulate* the quantity of blood conducted through them to any part or the whole, according to the demand that may be made, under circumstances constantly varying." This appears to be Dr. Macartney's opinion, as far as we can gather from his phraseology, which sometimes exhibits an almost Hunterian obscurity. "The small arteries never yield to mechanic force, but always change their character from a new disposition, arising out of a new state of feeling." (p. 126.)† Every physiologist knows that arterial trunks vary much in diameter under different natural conditions of the organs which they supply; and this difference will be found to accord with variations in the activity of the processes of nutrition, secretion, &c. that are characteristic of the capillaries into which they respectively divide. The idea that the dilatation of arteries is ever occasioned by distention *a tergo* (except under very peculiar circumstances), or that their contraction is due to the withdrawal of internal pressure, has recently been ably controverted by Dr. Graves,‡ to whose valuable papers on the subject we would refer such of our readers as wish to see the question treated at large.

We shall advance, however, a step further, (in which we believe that we are again in accordance with Dr. Macartney, although he does not state the doctrine very explicitly,) and maintain that this alteration in the caliber of the arteries, in proportion to the amount of blood which they have to convey, is governed by the ganglionic nerves distributed so universally upon their tubes. We infer this from the analogy in structure and function between the elastic coats of the arteries and the muscular tunic of the alimentary canal. The vermicular motion of the latter is unquestionably influenced by mental emotions, and by sympathy with other processes of the system, and these influences can scarcely be conveyed in any other way than through the ganglionic nerves that supply them, whose function appears to be to connect the purely *animal* with the *organic* functions. Just so we conceive it to be with the arteries. That they are capable, in some parts of the body at least, of being excited to special changes through the nervous system can scarcely be doubted when we consider the phenomena of blushing and some cases of *erection*, in which sudden and spontaneous dilatation of the smaller arteries is known to occur; and the facts which we shall hereafter adduce leave small ground for hesitation, as to their being sympathetically influenced through the same channel in diseased as well as healthy conditions.

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* Observations on Inflammation, (p. 9), 1832.

† We shall take occasion further on to discuss the propriety of Dr. M.'s use of the terms *feeling*, *sensibility*, &c. to imply changes in which *consciousness* does not participate.

‡ Medical Gazette, No. 553, &c.

We alluded in our last Volume (p. 181) to the general arguments which, in our opinion, substantiate the doctrine that the changes which the vital fluid undergoes in its passage through the capillaries, whether these changes be of a nutritive or a secretory character, have an important influence on its movement through them. Several of these are well stated by Mr. Palmer in his Summary of this part of Hunter's Treatise (p. 231-2); and to this we may refer our readers, premising, however, that we do not understand them as proving any *mechanical* action of these vessels, which appear as inert tubes under the microscope,—but rather as leading to the belief that a new set of *attractions and repulsions* (whether of a physical or vital character) are created between the particles of the blood and the walls of the passages through which they move, by the action which, in the normal condition, takes place between them. "The inference which may fairly be drawn from the whole of these facts," concludes Mr. P., "seems to be that the capillaries possess a *distributive* power over the blood, so as at least to regulate the local circulation independently of the central organ, in obedience to the necessities of each part." We deem ourselves further justified in laying down as a general proposition that—*the circulation of blood through the capillary vessels of any part will have a rapidity proportional to the activity of the normal changes which the blood undergoes in them; that it will be accelerated if, from any cause, these processes take place with unusual energy (in which case the vitality of the part may be regarded as exalted); that it will be retarded if these processes are in a depressed condition; and that it may be entirely checked by their total suspension.*

It is not a little curious that those physiologists who deny that the capillaries have any kind of influence on the motion of the blood through them, are driven, by the force of the facts to which we have alluded, to make certain exceptions, in which nearly all is conceded that is demanded by their opponents. Thus, we find Müller stating that the movement of the blood is entirely dependent on the action of the heart, and presently afterwards alleging the existence of an attraction or affinity between the blood and the solids which it supplies, in the "turgescence, turgor vitalis, or orgasm," which is observed to take place in certain parts of the body, independently of the heart. He cannot conceive how such an attraction can become one of the regular moving forces, however, since it would cause congestion, by rendering the blood stationary in the capillaries, "unless," he adds, "it be again admitted that this attraction of the capillaries for the blood is exerted only while the blood retains its arterial character, and ceases when it has become venous." Now, that this hypothesis is the true one, is, we think, supported by a pretty strong body of evidence. It is well known that, when there is any local excitement to the processes of nutrition or secretion, an increased flow of blood towards the part speedily takes place; and not only this, but an increased circulation of blood *through* the part occurs, quite independently of any altered action in the heart. This we find to be the case at puberty, as to the whole generative system; during pregnancy, with regard to the uterus; during lactation, in the mammary glands; probably with regard to the brain, also, when its functions are unusually, but normally, excited; and during the formation of new parts in the embryo. In vegetables, also, it is acknowledged that an afflux of fluids takes place towards parts in which active nutritive processes are being performed, even though the latter be artificially stimulated at an unusual time; and if the superfluous portion of these fluids were not returned or exhaled, the tissues would soon exhibit the unhealthy dropsical appearance which they assume when exhalation is checked by want of light.

We find the converse of this action occurring where there is any diminution or obstruction to these processes. If the diminution of the circulation through any part be one of the changes natural to the economy, we find that congestion is prevented by a progressive contraction in the arteries supplying it, as in the case of the uterus after parturition. But if the processes of secretion or nutrition be interrupted by extraneous causes, congestion is the immediate result; for the capillary circulation is *thereby* rendered inactive, and the blood accumulates in the affected vessels. We cannot have a more striking example of such an occurrence than in asphyxia, the true pathology of which has a most important bearing on this and other important physiological questions.

We have dwelt much upon the question of the independent action of the capillaries, because it is one of peculiar practical importance, especially in regard to the proximate cause of congestion and inflammation. It affords us at once the key to the comprehension of a great majority of cases of local disease, in which the former is a leading symptom. We do not mean to say that congestion of the different vessels is in *all* instances the *result* of inactivity in the nutritive or secretory processes in the part affected; but we are persuaded that it will be found to be so in a great majority of cases. If so, the obvious curative indication is to excite the capillary circulation by stimulating these processes. How successful such a plan is where we have the means of adopting it, we need hardly point out in detail. It will be sufficient to allude to the relief immediately effected by artificial respiration in cases of congestion of the pulmonary arteries from obstruction to the access of air; and to the influence of mercury in reducing the congested state of the liver which is so often found to accompany a deficient secretion of bile. The deficient secretion is often regarded as the *result* of the congestion; but the analogy of the lungs is, we think, quite satisfactory upon that point.

The doctrine which we have been upholding, in regard to the influence of the capillary changes in the blood on its circulation, serves also to explain the mode in which the action of the ganglionic nerves affects this function. There can be no doubt that the nutritive processes are regulated and harmonized by the operation of this system; and that the *organic sympathies* are chiefly communicated through it. It is easy to see, therefore, that, by acting like other stimulants or sedatives on the nutritive and secretory functions, innervation may greatly affect the motion of the blood; but that it has any other channel of influence has never been proved. Sudden and violent impressions on the nervous centres, such as powerful electric shocks, extensive mechanical destruction of nervous tissue, especially by crushing, and certain sedative agents, are found to arrest the capillary circulation almost instantaneously through every part of the body. These act, it is well known, upon the general vitality of the system, and seem to destroy that of the blood, as well as of the solids; so that there is a double reason for the remarkable effect in question. And local destruction of vitality by spontaneous gangrene has been found to check the flow of blood towards the parts, although it was proved, by subsequent examination, that no mechanical impediment existed. For ourselves, we are unable to perceive a single valid objection against the doctrine we have been supporting. It harmonizes completely with all the phenomena which a careful comparison of organized structures of different kinds brings to bear upon it, as well as with those supplied by pathological observations. It may be objected to it that it is deficient in precision, and that the idea of the reaction between the blood and the surrounding solids having anything to do in the propulsion of that fluid is a mere hypothesis; but we would reply, that we know just as much of the mode in which it operates as we do of the excitement of muscular contractility by the application of a stimulus; and that the action of the heart itself is quite as inexplicable as that which we have ascribed to the capillaries.

We shall now request our readers to follow us in another branch of the enquiry—that which relates to the vital properties of the blood; and here we shall again take Hunter for our guide, prefacing the discussion, however, with a few remarks on his treatise, entitled "General Principles of the Blood." This gives a comprehensive view of the mass of the blood, as composed of different parts,—of coagulation and its effects,—the serum,—the red globules,—the quantity of the blood, and course of its circulation,—the living principle of the blood,—and some unconnected experiments respecting this fluid. At the time Mr. Hunter lived, animal chemistry was in such a rude and imperfect state, that he was little disposed to place reliance on the aid which it afforded, and hence he endeavoured principally to ascertain the properties of the blood from other sources. Much additional light has been shed on all these points over which chemistry presides; so that, while Hunter may be said to have minutely and accurately sketched the natural history of the blood, none of the results furnished by modern analysis appear in the original pages. This, however, has been amply remedied in the present edition by the excellent notes which are appended, and which not merely correct the errors into which the author was betrayed, but, by setting before the reader, in a plain and lucid manner, the more recent chemical discoveries, carry down the subject to the knowledge of the present day. It is only, indeed, within the last few years, comparatively speaking, that many of the most interesting questions regarding the blood have been brought forward and discussed with vigour; we allude to the presence of peculiar substances in the mass, or in the secretions immediately formed from it, the changes which the former is observed to undergo under various circumstances, and the relation which these bear as to cause and effect in the phenomena of disease. Of all these things Hunter was nearly ignorant; for, although the humoral pathology had been known for centuries, it had, during his days, sunk before the more specious and tangible system of the solidists. It is doubtful what share he would have taken in this discussion, as there is almost nothing in his writings to lead us to form an accurate conclusion.

The point, above all, for which Hunter was distinguished, and on which he separated himself from most other physiologists of his time, was his doctrine of the *life of the blood.* It ought to be remembered, however, that although he was the warm advocate of this theory, he was not, by any means, its original projector: we find the same idea entertained by Harvey, who considered the blood as the *primum vivens* and *ultimum moriens* in every animal; and it is not improbable that he caught the hint from some less definite expressions made by Aristotle. We may trace it still further back in the writings of the inspired lawgiver of the Jews: "Only be sure that thou eat not the blood; *for the blood is the life,* and thou mayest not eat the life with the flesh."* The notion of attaching vitality to a fluid appears, at first view, not a little extraordinary, and accordingly drew down upon Hunter severe ridicule from his opponents.

> "To conceive that blood is endowed with life while circulating is perhaps carrying the imagination as far as it well can go; but the difficulty arises solely from its being a fluid, the mind not being accustomed to the idea of a living fluid. . . . . When all the circumstances attending this fluid are fully considered, the idea that it has life within itself may not appear so difficult to comprehend; and indeed, when once conceived, I do not see how it is possible we should think it to be otherwise, when we consider that every part is formed from the blood, that we grow out of it, and if it has not life previous to this operation, it must then acquire it in the act of forming; for we all give our assent to the existence of life in the parts when once formed. Our ideas of life have been so much connected with organic bodies, and principally those endowed with visible action, that it requires a new bend to the mind to make it conceive that these circumstances are not inseparable." (*Palmer's Hunter,* iii. p. 104-5.)

Hunter asserted that the blood was not only alive in itself, but the support of life in every part of the body; for if it had not this living principle, it would be, in respect of the body, an extraneous substance. It is not alleged, indeed, by any of its supporters, that this doctrine is capable of being demonstrated: but only that it is impossible for us otherwise to explain many of the changes which the blood undergoes. We have not space, nor is it worth while, to follow Hunter through his speculative argument; for, although sufficient evidence may be adduced to show that blood is affected by circumstances which have no influence on other fluids, nothing like *proof* of its possessing a distinct and independent vitality of its own was brought forward by him. "The argument for the vitality of the blood," as Mr. Palmer justly observes, at the end of a very judicious summary which he gives of Hunter's main facts and reasonings on the subject, "is strictly a cumulative one, and must be viewed in its totality, in order to estimate its real force. Taken altogether, the preceding facts raise a strong presumption in its favour, not to say a high probability." The two main points on which the doctrine with Hunter rests, are the circumstances affecting the coagulation of the blood, and the phenomena attending union by the first intention. The former he considered one of the greatest proofs that this fluid possesses life, for he conceived coagulation to be "an operation of life." Few physiologists, either British or continental, continue, we believe, to hold this part of Hunter's doctrine; but we cannot help thinking that it has been abandoned upon very slight cause. The evidence in its favour, involving many considerations which did not occur to Hunter, has recently been most ably summed up by Dr. Alison;* and we do not think it necessary, therefore, to enter into any detail on the subject at present. We may observe, however, on the nature of the facts brought in its support, that they are such as show an analogy between the effects of various agents on this process and those of the same on operations confessedly vital, which, in both cases, frequently differ from what might be supposed to be their physical influence. If the fibrine of the blood be regarded as possessed of vitality under any circumstances, we cannot see any difficulty in supposing its properties to be such that it shall remain fluid when in movement and in contact with the coats of living vessels, and that it shall concrete into a solid when withdrawn from these influences; this concretion being still, however, a result of the properties with which it became endowed when its elements were united in its peculiar proportions.

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* Deut. xii., 23.

Those who deny vitality to the blood on account of its fluidity, or, in other words, maintain that its properties are only those of inorganic matter, seem to forget that other fluids unquestionably exhibit vital properties. "We must not believe," it has been philosophically remarked by Andral, "that the manifestation of life only occurs where there is an organization such as we find it in the higher orders of animals, such as we are therefore in the habit of representing it in our own minds, and such as we conceive it to be in all cases." The proportion of fluids to solids in many of the inferior organisms is much less than in the blood. A large *medusa*, for example, which, when taken out of the water, weighs fifty ounces, is reduced, by drying, to a few grains; and the same might be said of some of the aquatic cellular plants; but it may be objected that these possess a definite organized structure. What, then, will be said of the semen of animals, or of the fovilla of the pollen of plants, which so nearly resemble the blood in character? Will it be maintained that any mixture of chemical products can imitate the effects of these? Or, to take a still more analogous case, the semi-fluid granular mass which unites to form the germinal membrane of the ovum; the changes which this undergoes are so completely analogous to those of the blood in process of organization, that they are evidently governed by the same laws; and will any one assert that these are laws of physics rather than of vitality? By such comparisons, it appears to us, we may not only refute the objection urged against the doctrine of the vitality of the blood, on the score of its fluidity, but obtain an affirmative argument of no mean value. To the series of proofs derived from the changes which the blood undergoes whilst still in its vessels, or extravasated among living tissues, we shall presently recur,—concluding this part of the subject with the following judicious remarks, which we extract from Dr. G. Burrows' Croonian Lectures on the Pathology of the Blood:[*] "It is perfectly true that, if the blood be regarded as a mass moving through the larger vessels, and be compared with any organized tissue, or with the parenchyma of any organ, there is no doubt a great difference between the physical qualities of the living fluid and solid. This dissimilarity, however, no longer exists, when a comparison is made between the blood in the capillaries of an organ and the surrounding structure which it nourishes and forms; in this part of each organ the blood and the tissue are intimately blended together; [as intimately, we may add, as the solids and fluids in the tissues of the acalephæ, in which little circulation takes place.] In every structure there is a point where the blood must be regarded as becoming extravasated—where it mingles and combines with and renovates the surrounding tissue—where, in fact, it becomes organized, and where its vitality is no longer a matter of doubt."

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* Supplement to Outlines of Physiology, pp. 31-3.

The strongest evidence that the coagulation of the blood, when drawn from the living body, is the result of the vital properties which it still retains, appears to us to be derived from the fact that a process essentially the same is the preliminary to the organization of the blood within its natural cavities. It is scarcely to be believed that, if this fluid be at any time possessed of vitality, its *death* should be the connecting link between its liquid and its evidently organized condition. That a similar coagulation occurs in plastic lymph, as a preliminary to its organization, has long been known; but this coagulation has been regarded by many as of a different character from that of the blood, from its being supposed that the red globules played an important part in the latter, and that coagulable lymph was a product of inflammation, essentially different from anything which previously existed in the body. Now, however, it is known that coagulable lymph and the liquor sanguinis are almost identical, and that the red particles are passive, or nearly so, in the coagulation of the blood. There is, therefore, a strong argument from analogy for considering this change in the blood in the same light as in the lymph. But, it may be argued, we do not find organization following it in the blood, as in plastic lymph, it being now generally allowed that blood effused into wounds or cavities is never converted into new tissue. Recent observations have shown, however, that, under certain conditions, blood *is* organized after coagulation in its natural cavities; and this process appears due to the influence of the vital properties of the tissues with which it is in contact. We shall not, at present, adduce the proofs of this position, but shall be satisfied with referring again to the very valuable Croonian Lectures of Dr. Burrows, where our readers will find them well displayed, and with extracting his summary of facts relating to the changes of a vital character which the blood undergoes in the vessels. "Thus we have instances, both in the heart and arteries, of the coagulation of the blood during life; of the various changes taking place in that coagulum, from a state of simple, unorganized, discoloured fibrine to an organized solid, often eliminating within itself fluids found in other organized parts, such as serum or pus; at other times the fibrine is converted into cartilaginous or osseous structure, or into matter resembling medullary sarcoma, which may or may not coexist in other organs of the body." The same changes are found to take place in blood stagnated in the veins; they result from any circumstances which impede the circulation, either by depressing its moving powers or by mechanical obstruction; and they are also produced by inflammation of the coats of the veins, which occasions the coagulation of the blood stagnated in them. The kind of structure into which the coagulum is ultimately converted appears to be much influenced by the nature of that with which it is in contact. Thus Andral mentions a case in which an osseous cyst, which had apparently originated in a fibrinous clot, was found beneath the valves of the pulmonary artery; and a large quantity of osseous matter was deposited in the lining and walls of the right ventricle. Such facts were noticed by Hunter, who embodied them in a general law; that when the blood has once coagulated in the living body, and become a solid, it "then changes into this or that particular kind of substance, according to the stimulus of surrounding parts."

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[*] Med. Gazette, vol. xviii.

It is remarkable that after the doctrine of Hunter, as to the organizability of coagula of blood effused into the tissues, had been abandoned by most succeeding physiologists and pathologists, it should be again revived with such additional evidence as to demand our reception of it as a fact. Such appears to us the case; for the late investigations of Andral, Carswell, and others leave no doubt that small coagula of blood thus effused may undergo most if not all of those changes which have been just stated to occur in blood coagulating within its own vessels; acquiring a distinct structure, and forming adhesions to the surrounding parts, and occasionally undergoing various morbid transformations. It is evident that our elementary notions of disease must be very erroneous if facts of this kind are left out of view; and we have given this brief sketch of the present state of our knowledge regarding the vital properties of the blood, because we deem with Hunter that on no other foundation can we erect any true theory of inflammation.

Before proceeding to enquire what inflammation *is*, we shall find it useful to determine what it *is not*. There are two conditions of the vascular apparatus of any organ which are liable, on a superficial view, to be mistaken for it; and these, although frequently confounded, are as different from one another as from the state with which we would contrast them. These are *determination of blood* (as it is commonly but erroneously called) and *congestion*. In the first there is an increased afflux of blood to the organ, resulting from increased activity in the vital processes to which it there ministers; and this increased activity may be either a part of the regular series of operations performed by the organism (as in the enlargement of the uterus, mammary gland, &c.); or it may be of an abnormal character, in which case it is generally accompanied by deficient action in other parts. How common is it to meet with this activity of function in the brain, accompanied by flushing of the face, throbbing of the carotid and temporal arteries, and the other symptoms which are popularly known to attend "determination of blood to the head," whilst the extremities are liable to coldness and lividity, marking torpor of the circulation in them? Now, it is sufficiently evident that the amount of blood at any time contained in an organ thus excited, whether normally or the contrary, will be greater than in the same whilst performing its regular actions only; yet this state is not one either of congestion or inflammation. An increased quantity of blood is attracted to it, but there is no stagnation of it in the vessels; no action of an abnormal *character* takes place between the fluid and the tissues which it supplies, the difference from the natural actions being only in *degree;* and the blood, propelled through the capillaries with an accelerated motion, is freely returned by the venous system. Although this condition *is not* inflammation, we shall presently see that it may border very closely upon the first stage of that morbid action; and every one knows that organs which are most liable to such states of excitement are also most prone to take on the inflammatory process.

In *Congestion* (using this term, however, in a more restricted sense than some writers seem to intend,) there is an absolute retardation of the flow of blood through the organ, together with an accumulation, either in its afferent vessels alone, or in both systems, according to circumstances. This retardation and accumulation may result from several causes. A more frequent *cause* of it than is usually suspected is, we are confident, a depression of the vitality of the organ, and a consequent inactivity of those processes which are essential to the maintenance of the rapidity of the circulation through it. We find an excellent exemplification in the state of asphyxia, in which the suspension of one of the conditions of those processes, the access of oxygen, causes congestion of the afferent vessels, from the accumulation in them of blood which is not able to pass through their capillaries. And we are certain that this principle may be more generally applied than it is in the diagnosis and treatment of disease, with very great advantage; for when the inertness of the nutritive and secretory processes is regarded as the *cause* rather than the *effect* of the congestion, our treatment will be applied rather to stimulate them than to relieve the congestion by the abstraction of blood. Now we shall hereafter find that this kind of congestion bears nearly the same relation with the second stage of inflammation that determination of blood does to the first; the principal difference being, as in that instance, that in inflammation there is a change in the *character* as well as in the *degree* of the natural actions. Here again, however, we find the two states often approximating very closely; since a morbific cause is peculiarly liable to operate in producing inflammation on an organ whose vital actions are already disordered. Of the other causes of congestion, arising principally from mechanical obstruction to the passage of the blood through the vessels, owing to its stagnation in other organs, compression of the vessels, &c., we shall not here dwell, since they have little to do with the theory of inflammation; we shall revert to them, however, when following Dr. Carswell through his account of the sensible changes produced in the tissues by the two conditions respectively.

The chapter in Hunter's Treatise devoted to the Fundamental Principles of Inflammation is, without any exception, the most laboured and intricate in the whole volume; and we shall, therefore, not attempt to give any analysis of it. But it is, at the same time, replete with interesting matter, and contains the rich outpourings of a vast and enlightened mind; a fountain of knowledge at which the pathologist may drink deeply. What a world of thought sometimes lies in a single sentence? We can only notice, in passing, the division appropriated to the consideration of the progress of inflammation in different structures, and the influence which structures have in modifying its character. Here has Hunter developed, in his own masterly style, the changes to which inflammatory action gives rise in the different tissues, and which have since his day occupied so conspicuous a place in the writings of succeeding pathologists; and here, also, may be distinctly found the germs of those brilliant views afterwards brought forth by Bichat, but without any acknowledgment of the source from which they were originally derived.

Hunter considered that inflammation was only a disturbed state of parts which required a new but salutary mode of action to restore them to that state wherein a natural mode of action is alone necessary; and that, from such a view of the subject, inflammation was not to be reckoned a disease, but as a salutary operation consequent either to some violence or some disease. We need scarcely stop to remark that such a notion is, on the very face of it, far too exclusive to be correct. Every deviation from the natural state cannot be regarded as salutary; and although inflammation is no doubt set up by nature in certain cases to effect a certain end, it is, at other times, the most dangerous and fatal disease which could arise. We almost wonder how Hunter could have been betrayed into such an error; and, indeed, we find him, with that disregard of minute consistency which characterizes minds of his ardent and enquiring character, acknowledging it in the very same page. It is not, however, in this chapter, but in that which comprehends Adhesive Inflammation, that Hunter discusses the state of the vessels in inflammation, the local and constitutional signs of the disorder, the effects in the system according to the structure of the part affected, and the remedies necessary for its removal. One of the most interesting points of enquiry, and which has received a large share of attention, and been made a subject of keen discussion, is the actual state of the vessels of an inflamed part. Considering the amount of labour and time spent in this investigation, we are naturally curious to know, even at this distant period, in what conclusions such an acute observer as Hunter rested. It does not appear then, from anything that we can detect, that he had ever examined the process of inflammation experimentally, or as it is seen going forwards in the transparent parts of an animal texture, and he seems to have rested content with theorising on the subject. From this it followed that, while some of his notions were correct, he was at the same time drawn into inconsistent statements, evidently experiencing the greatest difficulty in making up his mind on this abstruse question. No information can be drawn from such reasoning as he offers, founded as it was upon data so insufficient; for wherever speculation usurps the place of experiment and observation, we only hunt after the shadow and lose the substance.

Only one point was prominently insisted on by Hunter—the increased size of the vessels, coupled with increased power of action; but he does not define what he implies by the latter: "It must appear," he says, "that a much larger quantity of blood passes through parts when inflamed than when in a natural state, which is according to the common rules of the animal economy; for whenever a part has more to do than simply to support itself, the blood is there collected in larger quantity .... What the action is, or in what it differs from the common action of the vessels, is not easily ascertained." He seems to have had some difficulty in reconciling increased action of the capillaries, which, it might be supposed, would produce their contraction, with the fact of their dilatation. He gives a theoretical explanation of this discrepancy, and endeavours to establish his doctrine of increased action by reference to a great variety of phenomena, many of which, however, have no more than an analogical relation with it, and many more may be explained on other principles. Of these Mr. Palmer has given an excellent condensed view in his note to the chapter in question.

But although we differ from Hunter so completely as to the nature of the change of action in the capillary vessels, we cannot help thinking that, in referring the proximate cause of inflammation to that change of action, and in considering the modification in the size of the vessels and in the rapidity of the circulation of the blood through them as secondary phenomena, he was approaching the true point of investigation much more nearly than some of his successors, who have considered these last changes (which, as we shall presently see, are but effects of others,) in the light of proximate causes. "Inflammation," says Mr. Palmer, in the spirit of Hunter, "is an act which involves all the functions of a part." The blood circulating through it, as well as its own tissues, are altered in structure and properties. "More or fewer of these changes invariably attend inflammation, which cannot therefore be supposed to consist in a simple alteration of the circulation, but principally of those vital affinities subsisting between the vessels and their contents, which are the peculiar attributes of life." (p. 328.)

The theory of inflammation propounded by Dr. Macartney appears to us even more unsatisfactory than that of Hunter. Neglecting all consideration of the blood, and of the vital processes to which its motion through the capillaries is at once subservient and in part due, he has looked for the proximate cause of this condition only in the state of the vessels, which we cannot but regard as either a *result* of the primary alteration, or, at most, only a *collateral* change. It is but fair, however, that he should speak for himself, and we shall therefore give his opinions in his own words. In the first place, speaking of the *modus operandi* of the local causes of inflammation, he says:

> "It is obvious that they all act by making such impressions on the sensibility of parts as dispose the arteries to assume the inflammatory state ..... That even local injury excites inflammation, by operating on the sensibility, is proved, by its effect being proportioned to all the circumstances connected with organic sensibility, and not measured by the degree of mechanic destruction. As one illustrative example we may mention that a small punctured wound of the tendinous parts of the hand or foot would naturally induce more inflammation than blowing off the whole hand or foot by an explosion of gunpowder. The most striking evidence we have of the impression on the sensibility being the sole cause of inflammation, is found in the fact of all the naturally close cavities inflaming from being opened, and thence placed in a *new state of sensation* to them ..... It is the organic consciousness or sense

of exposure, or rather of imperfection, which causes inflammation in cavities that are naturally close." (pp. 111-2.)

This passage will afford us the key to Dr. Macartney's very peculiar phraseology, upon which we must take leave to make some strictures. It is entirely based on the doctrines of Bichat, which have been abandoned, in this department at least, by all the most intelligent physiologists of the present day. We refer particularly to that of "organic sensibility" being dependent upon the ganglionic system of nerves, which is to it what the cerebro-spinal system is to the animal sensibility. We shall hereafter see that the whole of Dr. M.'s theory of inflammation is erected upon the hypotheses (for such we must consider it), that the "organic sensibility" of the nerves of the part is first acted on; and that is the cause of all the other changes. In accordance with this he is led to the use of terms which we cannot but regard as worse than useless; since they not only complicate what is in itself sufficiently simple, but lead the passive recipient of them to imagine that something is explained, when in reality it is only involved more deeply. What is gained, we would ask, by attributing the contraction of the bladder on its contents (so that, whatever amount of fluid it may hold, it is always full,) to the "organic consciousness of vacuity?" (p. 112): or the disposition in the arteries to contract when blood is withdrawn to the "sense of danger felt" in them? Truly, in regard to the intelligibility of such expressions, we think that they are far below the celebrated "stimulus of necessity" of John Hunter. The latter had the advantage of not appearing to account for what seemed unaccountable; but plainly confessing the author's ignorance of the cause as any other than a law of the economy; whilst Dr. M.'s explanations, which appear to us about as valuable as those of the physiologists who regard everything as *vital* which they cannot explain in other ways, have the same injurious effect in tending to check enquiry, by holding up that as accounted for which is in reality only expressed in different and, to our minds, most objectionable language. It seems to us an abandonment of all precision to use the terms *sensibility* and, still more, *consciousness* in any but a psychical import. The more abstract language we employ, the more necessary it is that we should keep within the bounds of rigorous definition. And we are quite sure that the metaphysics of physiology will never assume a truly scientific form until its language is much more precise than at present. Most French physiologists of the present day have abandoned the term "sensibility" as applied to the simple vital properties of any part, which is all that it can thus express; and though some use the term *sensation* in the same import that in Britain is attached to *impression*, that is, a change in the condition of the afferent nerves, which, propagated to the sensorium, produces consciousness of the presence of an external object, it has only reference to the cerebro-spinal system. If, then, we regard this as a retrograde movement on the part of Dr. Macartney, still more must be reprehended his use of the term *consciousness* to imply a state in which *the mind* is not concerned. To do so is to break down all barriers between physics and metaphysics, the corporeal and the psychical. Even his prototype, Bichat, did not venture so far as this, but reserved the term to imply a state of *mind* in which the individual, *le moi*, is (we cannot phrase it in any other way) *conscious* of his existence. And will Dr. M. tell us that this condition can exist in the ganglionic system or the organs connected with it? It is very true that he qualifies it by the prefix "organic;" but why use it at all, since nothing is gained by it? We venture to prophesy that should Dr. M.'s opinions be made the subject of controversy a hundred years hence, they will stand a fair chance of being as much misinterpreted as those of Whytt have been in recent times; Whytt's "soul," as the originator of the "vital and involuntary motions," being precisely the analogue of Dr. M.'s "consciousness." We can imagine some learned critic of the next century following the same track, in the exposition of his opinions, as we have lately trodden, in the enquiry after those of Whytt; and toilsomely seeking, in the writings of the almost forgotten Bichat, and, peradventure, in our pages, for the solution of the mystery.

We must now return from this digression to criticise, not Dr. Macartney's language, on which we have sufficiently commented, but his doctrines. The first part of his chapter, on the Proximate Cause of Inflammation, is devoted to an enquiry into the structure and properties of the arterial tubes; and as his opinions on this subject lie at the foundation of his explanation, we shall quote such portions of his exposition as will serve to convey a fair idea of them, referring to the work itself for the arguments in their behalf.

> "It is admitted by all physiologists, that in proportion as the elastic coat of the arteries declines, these vessels possess the power of enlarging and contracting the size of their tubes. . . . . That there is such a thing as a positive and active extension and dilatation in the arteries, and other tissues similarly endowed, I think, cannot be denied, since we have evidence of dilatation taking place, under circumstances, and to an extent, that could not be produced by a mere cessation of the contractile action; and, as we see in all cases where vessels are dilated, or surfaces are extended without mechanic force, the causes are some excitement or irritation, or a necessity for the performance of some new or more vigorous functions. We have, therefore, the right to assume, that the dilatation or extension of parts is their most active condition, and that their contraction is the negative or passive state, it being that into which tissues fall when they are free from stimulation, or when their functions cease, or when they are deprived of nervous influence, or when the causes which produced dilatation are withdrawn. . . . . In arteries this *tonic action*, if not limited to the inner tunic, is most remarkable in those branches which are composed only of this tunic and common cellular substance. Arteries are observed to exhibit three states: 1st, the *middle* or *ordinary* one, which is consistent with natural circulation, growth, and secretion; 2d, the *excited state*, in which they are dilated, and thereby admit an unusual quantity of blood; and 3d, the *passive state*, in which they receive a smaller quantity of blood than usual, and sometimes none at all." (pp. 116-8.)

In these statements there are several controvertible points. In the first place it has been shown, by the recent observations of Schwann, that the capillaries have a fibrous coat at least as well developed, in proportion to their caliber, as that of the larger arteries. Dr. M. is, therefore, in error in stating that the tonic power of arteries increases with the decline of their elastic tunic; and we maintain, on the other hand, that all their changes in diameter, as well in the capillaries as in the larger trunks, are under its control. But further, we deny *in toto* Dr. M.'s position that a dilated condition of these tubes is an evidence of the activity of their functions. In the facts which he adduces in support of the doctrine, he appears to us to manifest a strange confusion of ideas. Thus he brings forward the dilatation of the arterial trunks, and the thickening of their coats, in cases of determination of blood to particular parts, as an evidence that the dilatation of the capillaries is an active condition. The two phenomena are essentially distinct. The dilatation of the arterial trunks, occurring wherever there is an increased demand for blood, has only reference to their function as canals for its conveyance; and it would be very possible to attribute it solely to the increased nutrition of their coats which takes place under such circumstances. The dilatation of the capillaries, on the other hand, has reference to their functions as nutrient and secreting vessels; and it by no means follows that *these* functions are more active when their caliber is increased, so as to allow a larger stream of blood to pass through them. In fact, we shall find that the exact contrary is the case. The same objection will apply to all the other arguments which Dr. M. has adduced in proof of his position. We by no means deny that, what he terms *vital dilatation*, that is, the enlargement of the caliber of a canal, independent of mechanic force, does take place in the living body; but we deny that it is always to be regarded as the result of a state of increased activity in the tissues concerned, and more especially in the instance of the capillary vessels. We shall now follow Dr. Macartney to his explanation on the *proximate cause* of inflammation, forewarning our readers to make due allowance for his peculiar phraseology.

> "When an injury has been committed on any part of the body, the natural sensibility of that part is roused to a consciousness of what has been suffered, either instantaneously or in a very short time. This local feeling or impression, or whatever it may be called, is distinct from the sensible pain of a wound, and may exist in the greatest degree where little or perhaps no pain is felt. At other times it may be coincident with common pain, although when perceived it is distinguishable from it. In general the organic consciousness is distinct from that of the individual, unless the sense of injury be communicated to the whole nervous system, and then the person feels a degree of agitation and alarm which he cannot understand or account for. On some occasions we observe the immediate effect of the impression shown by the vessels of the part, as when the circular blush around the puncture at the moment of vaccination, gives intimation of the sensibility of the part, to the admission of an animal poison: and sometimes the individual is conscious of a peculiarity of sensation from the contact of other morbid poisons, when the organization has not been injured, but has merely received the impression. As we have sufficient grounds for assuming that an organic sense of injury or danger, modified according to the causes which produce it, necessarily precedes inflammation, it is reasonable to conclude, that this is the proximate or essential cause of inflammation, and the opinion seems to be confirmed by the history already given of the constitutional and local causes; as also by the contemplation of the kind of sensibility and vital properties which belong to the tonic tissues, of which the arterial is one." (p. 130.)

Now this seems to us to be only another and more complicated manner of stating, what every one knew before, that certain impressions would produce the condition termed inflammation. It is the *modus operandi* of these impressions that we have to seek for; and the only step, if it be one in reality, which Dr. Macartney has made, is in attributing their action to their influence on the ganglionic nerves, a doctrine by no means novel. The term *proximate cause* does not seem to us rightly used by him to designate what is at most but the channel through which impressions act on the organic system. It is the change in that system *primarily* resulting from their action, and giving rise to the other more sensible alterations, that properly deserves the title; and on this Dr. M. does not seem to us to have thrown any light. He has, indeed, obscured it rather than otherwise; for we cannot but think that his mode of reasoning on the nature of inflammation is essentially defective, in considering only the state of the vessels, and in leaving out of view the changes to which they are subservient. But is it true that the impressions which produce inflammation necessarily act through the nervous system? We think not. To prove this it must first be shown that the normal changes which constitute the organic functions, all of which are due to the influence of external agents on the organism, depend upon its influence; a doctrine of which we have heretofore shown the instability. If a normal stimulus can produce a healthy change or action, without the intervention of nervous agency, it is perfectly evident that an abnormal stimulus may produce a morbid change independently of it. The remark of Mr. Palmer upon this question strikes us as peculiarly judicious. "The office of the nerves in inflammation appears to hold precisely the same relation to this action that it does to the other organic functions of the body. It is regulative but not essential." (p. 329, *note.*) It is to be remembered that Dr. Macartney's theory involves only the ganglionic nerves, and it is somewhat difficult, therefore, to disprove it; but it is for him to bring forwards the evidence in its favour, of which we can find but little. It is, of course, no objection to his theory, that parts of which the cerebro-spinal nerves are paralyzed are still liable to inflammation; and even more so than in the perfect state, since their nutrition is less active than it should be, and their susceptibility of injury from external agents therefore increased.

> "In these cases of paralysis," says Dr. M., "the nerves of the arteries which are chiefly derived from the sympathetic system, have their functions unimpaired; and. therefore, these vessels are influenced by irritations, of which the individual takes no cognizance. I cannot doubt that if the arteries could also, by any experiment, be deprived of their own nerves, or if the connexion of these with the sympathetic were interrupted, none of the phenomena of inflammation would arise, whatever injury might be inflicted." (p. 134.)

We, on the other hand, do very much doubt Dr. M.'s doctrine, and we shall find that it is based on a very insecure foundation. Our readers will recollect that, in his introductory chapter, he maintained that inflammation is liable to occur in different beings, in proportion to the development of their nervous system, and that it does not take place in those which are destitute of that system, namely in vegetables. We took occasion, when adverting to that subject, (Vol. VII. p. 429,) to correct this error, and to show that a state corresponding with inflammation may be produced in plants; and we think this quite a sufficient disproof of the doctrine, that the nervous system is *essentially* concerned in it. There can be little question that by this system are produced, in great measure at least, those sympathetic effects which take place in the system at large from any local disease; and we find these more strongly marked, as Dr. M. has very justly stated, in proportion to the development and complex distribution of this system. But we are by no means warranted in thence inferring that the primary changes are themselves dependent upon it.

A much more satisfactory view of the nature of inflammation, because more practical and comprehensive, is given us by Dr. Carswell; though we think it still open to some of the objections which we have urged against Dr. Macartney's. Although he regards it as in part a disease of the function of innervation, he fully recognizes the change in the vital properties of the blood, and in its actions with the affected tissue, as primarily and essentially concerned. His account of the phenomena, which may be regarded as of constant occurrence, and as in fact constituting the disease, is so good that we shall quote it almost in full. After speaking of the unsatisfactory results of the numerous experiments on the state of the capillary circulation in inflammation, if these be indiscriminately contrasted, he thus proceeds:

> "If, however, we carefully examine in detail the numerous experiments which have been made on this subject by Thomson, Wilson Philip, Hastings, Kaltenbrunner, Wedemayer, and Gendrin, and separate the effects of chemical or other agents which are known to modify the tissues and the blood in a manner very different from that of a mere stimulus, from those produced by this latter cause, we shall find that the results have almost always been similar in kind under similar circumstances, and in perfect accordance with the known laws of the vital manifestations of sensibility and contractility—properties, the sensible modifications of which, if not the most important, are the first that occur in the series of changes which constitute the state of inflammation. Equally important, because constant changes are also those which take place in the temperature of an inflamed part, and in the vital and physical conditions of the blood, and, as immediate consequences of these, in the functions of secretion, absorption, and nutrition." (p. 2.)

We are ourselves disposed to believe, for the reasons already hinted at, that the organic functions are those *primarily* affected by such stimuli as produce an inflammatory action; and that the alteration of the *sensibility* (by which we mean, with Dr. C., *conscious* sensibility,) is consequent upon that change. The lesion of sensibility is that which may first manifest itself; but it does not thence follow that it is the cause of the other phenomena. We shall proceed, however, with Dr. Carswell:

> "I shall consider inflammation as a disease, the essential phenomena of which present themselves in two successive stages or periods, each of which is referrible to opposite conditions of the physiological properties or functions of the affected part. Although these opposite conditions are sufficiently characterized by the changes which take place in the temperature of the inflamed part, and in the functions of circulation, secretion, and absorption, they are more especially so in those which occur in the sensibility and contractility. The first effect of mechanical irritation is an increase of the sensibility, soon amounting to pain, even in those tissues which, under ordinary circumstances, possess this property in a very low degree; and subsequently, or at the same moment, contraction, or an increased development of the contractile power, if not of the capillaries, of the arteries with which they immediately communicate. After a certain time, varying with the violence of the exciting cause, the sensibility diminishes or ceases to be manifested, for pain is no longer the consequence of the same cause. The contractility undergoes a similar change; the minute arteries cease to contract when stimulated, and they, as well as the capillaries and minute veins, are permanently dilated and distended with blood. That these two opposite states of the sensibility and contractility exist in inflammation, and occur in the order in which I have stated, might, independently of the evidence of the facts themselves, have been believed and understood, for they are but an exag-

> geration of the law of vital action, or of the physiological conditions of all parts endowed with these properties; a state of activity and repose, of increase and diminution, being the opposite and invariable consecutive conditions of both. It is hardly necessary to observe that these properties, both pathologically and physiologically considered, admit of these opposite conditions in so far only as the exciting cause acts in the strict sense of a stimulus; for if the agent to the influence of which they are subjected is of such a kind as to destroy, or, as is commonly said, to exhaust, instead of increase, there is at once produced a diminution, cessation, or extinction of both, and consequently of their visible signs, such a state cannot, therefore, be regarded as a state of inflammation, for the first or active conditions of the disease have not existed. It is, however, a state which is frequently produced in experiments made to ascertain the effects of stimuli on the capillary circulation in inflammation, and then becomes an interesting illustration of the fact just stated; for although congestion takes place in the part thus debilitated, it is soon followed by the active phenomena of inflammation, or those which characterize its first stage, and a more rapid and extensive formation than usual of those of the second." (pp. 2, 3.)

These remarks are, we think, peculiarly valuable; presenting, as they seem to do, the true explanation of the contradictory phenomena which have so much perplexed pathological observers. We fully accord with Dr. C. in regarding the first effect of a simple stimulus, as an exaltation of the vital actions (whatever may be their character) of the tissue to which it is applied; and Dr. Macartney seems to have lost sight of the fact that this exaltation is manifested in a contraction of the arteries distributed in the part, which does not yield to dilatation until a certain interval has elapsed which varies under different conditions. This exaltation or exaggeration of the regular vital actions of the part is manifested, as we shall presently see, in other ways also; and it is obvious that it differs but little from the condition which we have already discussed under the name of "determination of blood." The effect of certain stimuli in immediately depressing these actions, and thus producing congestion, will recall to the minds of our readers what we then said of the character and causes of that state.

With regard to the changes which are witnessed in the movement of the blood simultaneously with those already described, Dr. C. states the well-known fact that, "in the first stage of inflammation, the circulation is accelerated, and a greater quantity of blood than natural passes into the capillaries; in the second stage, the circulation becomes impeded, and the blood which distends the capillaries ceases at last to circulate." (p. 3.) Dr. C. allows to the altered diameter of the capillaries some influence on the rapidity of the stream of blood through them; but considers as of the greatest importance the relation of the two states to the primary lesions, which implies, he justly remarks, "opposite states of the agents which effect or regulate the transmission of the blood in the capillary vessels." We shall return to this point after enumerating the other most important changes which take place in this disease, which we shall do in Dr. C.'s language.

> "During the first stage, the vital properties of the blood undergo a manifest increase. A greater quantity of fibrine is formed, the plastic property of which is increased; for, besides its rapid organization under favorable circumstances, it retains, when separated from the other constituents of the blood, its fluidity for a longer period, and contracts more firmly than in the natural state. The unwonted vigour of the circulation in general, the increased temperature of the whole body, and the resistance opposed to the means employed in inflammation to weaken the vital

powers, may also be regarded as connected with this state of the blood, or as inordinate effects of its vital agency through the medium of the nervous system. In this stage too, the temperature is variously and greatly increased; the function of secretion is also for a time augmented; in glandular organs, however, only at the commencement; in serous tissues, for a much longer period, and to a much greater degree. Absorption, if not increased, manifests its activity by the speedy effects of poisons locally employed in this stage.—During the second stage of inflammation, changes, the reverse of those just described, occur in rapid succession. The blood ceases to circulate, coagulates, and assumes a dark colour; the temperature sinks; and secretion, absorption, and nutrition are finally interrupted. If those conditions of the affected part are maintained for a certain time, new products are formed, or other diseased states are produced, as softening, suppuration, ulceration, and mortification, which are therefore denominated *terminations* of inflammation, and constitute separate objects of investigation." (p. 4.)

Dr. Carswell subsequently points out that these opposite conditions may be recognized also in the character of the effusions from the inflamed tissues.

"The different degrees of fluidity, viscidity, and coagulability of the secretions of inflamed tissues are derived from the presence of the serum, albumen, and fibrine of the blood in various proportions. But the most important circumstances connected with these changes in the secretions of inflamed tissues is the formation or separation from the blood of two fluids of an opposite kind, viz. coagulable lymph and pus; the former possessing vital properties, assumes, as it is said, *spontaneously*, but no doubt in virtue of these properties, the solid form, becomes organized, and fulfils the all-bountiful purposes which nature has assigned to it in the economy of life,—the reparation of those injuries so frequently the consequences of the diseases of which it is the product; the latter, possessed of no plastic properties, being, as it were, the residue of the former, and of the molecular structure of organs, by a disorganizing or destructive process, is essentially inert, and like all substances incapable of being assimilated, is separated or removed from the body. The presence of these two products, therefore, marks not only the existence, but serves to distinguish the two most important periods of inflammation. Nor does this apply to the local conditions only of the disease; it is equally conspicuous in the general phenomena which accompany each period, or, as we have said before, each stage of inflammation, by the increased energy or vital power manifested in the first, and the diminution or perversion of the same in the second, and by the nature of the remedies required in each." (p. 12.)

From the consideration of these and other facts, we are led to the belief that the *first* effect of a stimulus calculated to produce inflammation, is on the general organic properties of the part, and especially on its power of deriving from the blood the materials of its assimilating or secreting processes; and that the influence it exerts on the caliber of the vessels, and on the motion of the blood through them, is altogether *secondary* to this. Considering it fully proved that the capillary cirlation is greatly affected by the energy of healthy reaction between the fluid and the tissues it permeates, we cannot doubt that any morbid alteration of this will greatly influence the movement. Accordingly we find that the effect of a stimulus which increases, for a time, the physiological or normal actions of any part, is to accelerate the capilary circulation, whilst the caliber of the vessels is diminished. The latter alteration can scarcely be due to the direct application of the stimulus; but is rather, we think, to be referred to the influence of the ganglionic nerves, which unquestionably are largely concerned in the subsequent processes. To the same influence we should be disposed to refer the simultaneous dilatation of the trunks leading to the part, so as to admit an increased supply of blood, which dilatation, as we have already pointed out, is probably due to the increased nutrition of their elastic coat.

The changes which take place in the blood itself may be accounted for in two ways. We may believe, with much reason, that the physical and vital properties of the whole mass are altered by its passage through the vessels of the affected part, whose parietes are capable of thus influencing it. This idea would correspond with the facts disclosed at a later stage of the process by microscopical examination, as to the evident changes which take place in the blood during its passage through an inflamed part; and it would seem to derive some confirmation from the circumstance that the buffy coat seldom shows itself on blood drawn very shortly after the commencement of inflammation; but that a period of about twenty-four hours seems usually necessary, in man at least, for its development, during which the whole of the circulating fluid will have been repeatedly subject to this influence. On the other hand, it may be argued with much plausibility, that the increase in the quantity and vital properties of the fibrine is due to a sympathetic alteration in the process of sanguification communicated through the ganglionic system: and this view would appear supported by the fact that the buffy coat is commonly found on blood drawn during pregnancy,—a state in which the active assimilating processes which are taking place in the fœtus, may be supposed to require a corresponding amount of the organizable part of the blood, and in which no influence of the maternal vessels on the blood can be supposed to produce the change; as also by the detection of other analogous changes (the formation of pus-globules, for example,) in the blood, when no local cause can be assigned for them. Perhaps we shall not be wrong in assuming that both these causes may be in operation, with varying ratio, in almost every instance.

Whilst the tendency to organization in the blood is thus increasing, the vital properties of the inflamed tissues are undergoing a contrary change. Their power of separating certain elements of the blood, whether for their own nutrition, or for secretion, is impaired; being, it would seem, not merely diminished, but perverted. Hence, the organizable portion of the blood, not being appropriated, as it should be, is effused either into the substance of the tissue or from its surface, as the case may be; and there it may undergo the process of organization by its own inherent properties, requiring only the neighbourhood of a living tissue, to bring it into communication with the general structure, and to supply it with continued nutrition. On this part of the subject we cannot here enlarge, although it is among the most interesting on which we might dwell. We must stop, however, to notice the curious fact brought to light by Dr. Carswell, that the effusion of tubercular matter closely resembles that of coagulable lymph which does not possess sufficient vitality to become organized. And in this connexion, we may introduce some remarks made by Dr. Macartney upon the scrofulous diathesis, which we think very discriminating, and worthy of attention.

"In scrofulous persons, the vascular system is weak, the vessels are small, the blood is deficient in quantity, and is, I think, imperfectly organized; it wants the full power of generating coagulable lymph. . . . . The brain is pale, and does not possess the usual quantity of red blood; and yet there is a mental character belonging to the scrofulous habit, which more strikingly indicates the peculiar state of the constitution than all the other signs. Scrofulous children, in general, exhibit no mental energy, but a gentleness and amiability of disposition, a refinement and judgment in matters of taste, and a purity of moral feeling, which is sometimes so remarkable, as to place them in these points far beyond the scale, and even beyond the conception of the mass of mankind." (p 82.)

This description strikes us as being more correct than many we have seen. Mental superiority is often dwelt upon as a characteristic of the scrofulous habit; but we agree with Dr. M., that it is rather in the *moral* than the *intellectual* faculties, that the difference is presented.

To return, however, to inflammation. When the change in the vital properties of the affected tissue has proceeded to a considerable length, it affects in a corresponding manner that of the blood which circulates through it; and instead of the organizable lymph, we have an unorganizable product, pus, of much lower degree of vitality, separated from the fluid. The observations of Gendrin on the conversion of the globules of blood into those of pus, by undergoing decolorization and other changes, have obtained currency amongst pathologists; but the late researches of Mandl,* on this point, seem to prove that this account is not correct. He found that when blood was mixed with pus, the red particles of the former underwent disintegrating changes, but did not come to resemble pus-globules; whilst, on the other hand, the latter closely resemble the irregular globules which are formed in the liquor sanguinis, when separated from the red particles, and coagulating imperfectly.

Although the full exposition which we have thought it right to give of our views on the essential nature of inflammation has already extended to such a length, we cannot dismiss this department of our subject without adverting to the highly valuable remarks of Dr. Carswell on the changes which are produced in the tissues themselves by the existence of this morbid action. It would be quite superfluous in us to attempt to criticise such statements from one whose authority on all questions of Pathological Anatomy is so high; and we shall, therefore, content ourselves with such an abstract of his observations as may be interesting and useful to those who have not access to this most valuable work. Excluding the *physiological* characters of inflammation—those deduced from observation of the *actions* of the part,—the physical or *anatomical* characters consist in modifications of its natural colour and vascularity, and of its consistence and bulk. Of these, the most conspicuous are increased redness and vascularity; but as these may occur in other states besides that of inflammation, it becomes necessary to seek for some diagnostic indications whereby this may be recognized. The *primary* or most general forms of inflammatory redness may be described as the *ramiform*, *capilliform*, *uniform*, *punctiform*, and *maculiform*.

"The first and second of these, as the terms imply, have their seat in the small arteries and veins, and in the capillaries respectively; the third and fourth are produced by capillary injection, the uniform red colour which accompanies the former being the consequence of an accumulation of blood in the entire capillary system of a part; the minute dotted or punctiform redness of the latter arising in the peculiar structure of the part, as in the villosities of mucous membranes, when the seat of inflammation, apart from the mucous tissue itself. The maculiform has also its seat in the capillary vessels, is sometimes produced by the accumulation of blood being greater in some points than others, but much more frequently by rupture of these vessels and extravasation, and hence may be called the *hemorrhagic* form of inflammation. Exclusive of the influence of various modifying causes, but more especially of the quantity and quality of the blood, the several degrees of inflammation are expressed by each of these forms in the order in which they are enumerated, the ramiform indicating the least, the maculiform the greatest degree." (p. 5.)

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* Anatomie Microscopique, liv. ii.

Uniform redness is best seen in dense vascular tissues, such as the skin, where it always occurs as the first physical sign of inflammation: in inflammation of mucous membranes, however, it is always preceded by the capilliform or punctiform injection; and in these, also, the ramiform is remarkably conspicuous, whilst the maculiform or hemorrhagic is more frequent than in any other tissue. "All these forms, indeed, varying in degree, are met with combined in the mucous membranes, more especially of the digestive organs; the capilliform and punctiform being the first that appear, and the most characteristic of inflammation." In all the serous membranes, uniform redness is produced in the same manner; but ramiform injection never occurs in them,—really having its seat, where it appears to be present, in the subjacent cellular tissue. Indeed, adds Dr. C., "it has always appeared to me that even the capilliform injection of these membranes is in great measure produced by the penetration of the blood into their softened substance by the *vis à tergo*. I have certainly never been able to discover blood-vessels in the arachnoid, in inflammation of the pia mater, except where it lies in contact with this membrane." The same source of fallacy must be guarded against in other cases, since the red colour which any membranous tissue presents may be owing to blood accumulated in the subjacent cellular substance, the hue of which is transmitted through it, according to its degree of transparency. In parenchymatous organs, inflammatory redness is strongly marked; but from their complex structure, and the great quantity of blood with which they become impregnated during life and after death, from various causes, it is often extremely difficult to say how far the redness depends on this circumstance or on capillary injection. In organs naturally of a pale colour, redness and the causes of it are sufficiently obvious; it depends on the uniform capillary injection of the first or second stage of inflammation, and is succeeded by the punctiform or hemorrhagic.

The *secondary* forms which inflammatory redness presents, constitute the distinctive local characters of many diseases, especially in the cutaneous and mucous tissues. On these, however, we shall not at present dwell.

Other changes of colour are noticed besides *redness*, especially in the chronic or asthenic states of inflammation. From a more or less complete stagnation of the blood in the capillaries, and from certain changes which it undergoes there, especially when neither suppuration nor effusion of coagulable lymph are taking place, various shades of purple, brown, and black, are produced. When the discoloration is punctiform, as when seated in the villosities of the alimentary canal, it produces a gray or slaty hue, called by French pathologists *couleur ardoisée*.

Most or all of these appearances, however, may be artificially produced in many organs by local circumstances operating either during life or after death, without the existence of inflammation; and it becomes a matter of great importance to be able to distinguish the injection of the vessels produced by gravitation or mechanical obstacle to the return of the venous blood, and the coloration of the tissues by imbibition, from the effects of disease which they so closely resemble.

"The differential characters of inflammation and local congestion are founded on certain differences in the physical characters of each, and in the circumstances under which they respectively occur. Although similar forms of redness and vascularity are produced in both, it is perhaps only in mucous membranes that a difficulty arises in distinguishing the one from the other. So long as the redness and vascularity are confined to the capillary vessels, or have their seat in the villous or follicular structure, there can be no doubt as to their inflammatory nature. It is only when they become more general and present the ramiform character that any difficulty arises. This, however, is removed by an examination of the neighbouring veins, which, in mechanical congestion, will be found dilated, tortuous, or even varicose, according to the degree and duration of the obstacle by which it has been caused. The congestion of the veins may likewise be traced to its cause. In inflammation, the local congestion commences in the capillaries, afterwards extends to the small veins, but never to large branches; in mechanical congestion, the blood accumulates first in the trunks, which are always conspicuous, and afterwards in the branches and capillaries. It is only when mechanical congestion is combined with inflammation, that the anatomical diagnosis becomes difficult or impossible." (*Carswell*, p. 9.)

No distinction of this kind, however, will hold good with regard to the vascularity produced by the *depending* position of parts during life or after death, which perfectly resembles that of inflammation, when occurring in the same parts. Our determination must be grounded, therefore, upon the presence or absence of this cause.

The redness observed in tissues in contact with the blood after death, produced by *imbibition*, and also that occasioned by transudation when decomposition has taken place, are easily distinguished, according to Dr. C., from that which is the consequence of inflammation.

"It can never be confounded with that of inflammation, when it occurs along the course of the larger subcutaneous veins; but it has frequently been so in the lining membrane of the large arteries especially, and of the veins. Whatever may be the circumstances which favour the red colour of imbibition, it never bears a near resemblance to that of inflammation. It is a mere die of a uniform, almost scarlet, red colour, generally limited to the lining membrane, without any other perceptible change of the coats of the vessel; whereas redness from inflammation is of a dull, rather pink, tint, extending more or less to the other coats, accompanied by a fine capillary injection of the subjacent cellular tissue, and marked congestion of the vasa vasorum; the lining membrane is softened and opaque or easily removed; the cohesion of the other tunics is diminished; they are also thickened or swollen, and infiltrated with serosity or coagulable lymph." (p. 10.)

The *permanence* of the redness and vascularity of inflammation after death is regarded by Dr. C. as a character by which alone, and under doubtful circumstances, we may ascertain the existence of this disease, and distinguish it from the local congestions with which it may be confounded.

"The redness and vascularity of mechanical congestion, of position or gravitation, and of imbibition, differ, as we have seen, essentially from those of inflammation, in the mode of their production and other local circumstances. But they also differ essentially in other respects. Thus, all these kinds of redness and vascularity are produced and maintained by appreciable causes, which operate external to the vessels, without the blood or the vessels themselves undergoing any change, by which the vital properties of either are so modified as to render the redness or vascularity permanent after death. By means of ablution, pressure, or scraping, both of these physical characters disappear in a short time. In inflammation it is far otherwise; the employment of the same means never removing, or effecting only a slight diminution of either. Injections, too, which penetrate the capillaries in the former kinds of congestion, cannot be made to reach them in that which proceeds from inflammation. It is necessary, however, to observe, that this state of the capillaries, and the redness which accompanies it, and which constitutes what I mean by the *permanency* of these physical characters of inflammation, are more or less decided by the degree and stage of the disease. In the highest degree or second stage, the permanency of both is most marked; in the first stage it is much less, the redness diminishing or disappearing entirely after death, in slight inflammations of the skin of short duration. This difference in the permanency of the redness and vascularity of inflammation in its two stages may be explained by the facts already noticed, viz., the absence of coagulation of the blood in the first and its occurrence in the second stage, in which also the fibrine unites with and penetrates the capillaries, thereby retaining the colouring matter of the blood, and producing occlusion of the vessels. Although, however, I have said that *redness* disappears after death, in slight inflammation of the skin, some degree of increased *vascularity* remains, as is seen by comparing the diseased with a healthy part. And, besides, it is of great importance to know that, even in such slight cases of inflammation, the cutis undergoes changes which render the existence and extent of the disease very conspicuous after the disparition of the redness, and from one to two days after death. The affected parts only of the skin assume a purplish tint, and become infiltrated with bloody serosity, and the epidermis is detached from these parts much sooner than from those which were not affected by the inflammation. This post-mortem congestion, and a more rapid tendency to decomposition than in ordinary circumstances, are conditions which ought not to escape the notice of the pathologist, when it occurs in internal organs. They are sometimes the only morbid appearances which are met with in fatal cases of scarlatina, and especially in rubeola, and indicate the extent not only of the inflammation, but of the depressing influence which it must have exercised on the vital function of these organs." (*Ib.* pp. 10, 11.)

The practical importance of these remarks must be our apology for not having abridged them. Every man of experience must have met with cases in which the difficulty of distinguishing the marks of inflammation from the results of other agencies has been both perplexing and important; and this is more especially the case in medico-legal investigations. There can be no doubt that in past times the existence of the marks of inflammation has often been too hastily credited; and that the erroneous inferences, founded upon such belief, have led even to the sacrifice of innocent lives. Perhaps in later times the tendency has been to err in the other extreme, by not admitting the previous existence of inflammation, unless some of its peculiar products are discoverable. Any diagnostic marks which can aid us in drawing a correct middle line are evidently, therefore, of great importance; and, without saying that Dr. Carswell has done all that could be wished in establishing these, we are satisfied from his remarks, that the difficulty is by no means incapable of being overcome.

Our determination may be assisted, as already hinted, by careful observation of the state of consistence of the affected part. Acute inflammation almost invariably produces *diminution of cohesion* between its organic elements; and this change, which of course arises from the depravation of the nutritive processes, is apparent in many cases of inflammation where redness and vascularity have disappeared, or mark but faintly the degree of alteration which the disease has effected in the process of nutrition. The solidification of spongy organs during acute inflammation, as when the lungs are hepatized, might appear a fact contrary to this general doctrine; but this solidification is only due to the retention of the effused fluid in the tissues; and although they feel harder than natural when compressed, the diminution of cohesion which has taken place between their anatomical elements is rendered conspicuous by the facility with which they are penetrated, broken down, and crushed. "The necessity and importance of recognizing this physical character of inflammation must therefore be obvious, as it not only enables us to detect the existence and appreciate the degree of this pathological state, but also to recognize the first and most simple organic alteration to which it gives rise." An opposite condition, a state of *induration*, is a frequent consequence of inflammation when chronic, or, at least, often accompanies it. It differs from the solidification in acute inflammation in this, that there is at the same time increased cohesion of the anatomical elements of the affected part. It is obviously the consequence of the organization of the coagulable lymph effused in the first or second stages of acute inflammation, either into the interstitial cellular structure of organs, or into the tissues themselves, in the state of softening just described.

The increase of *bulk* which accompanies acute inflammation, and which depends upon the accumulation of blood in the part and the retention of effused fluids, is an alteration which acquires importance rather from the effects to which it gives rise than from its value as a physical character of acute inflammation.

Every one knows the old definition of inflammation—*rubor, tumor, cum calore et dolore;* and, having discussed the two former, we should advert to the latter, had we anything either novel or peculiarly important to say regarding them. It is not part of Dr. Carswell's plan to reat of them, and Dr. Macartney is more than usually mystical upon he subject. He quotes the experiments of Brodie, as if their fallacy ad never been exposed, to show that animal heat does not depend upon espiration; and considers that we are to look upon animal heat as 'one of the phenomena of sensibility." We do not see how this can be egarded as a step in the enquiry. There can be no doubt that its evo- ution is greatly influenced by the nervous system; but, as we conceive, nly through the medium of those molecular changes to which it is nmediately due, and which are without doubt controlled to a certain xtent by the agency of that system. Two facts mentioned by Dr. M. re new to us, and of considerable interest. He states that if the prin- ipal nerve of an extremity be divided, the member becomes instantly hree, four, or five degrees hotter than it was before; and that division f the spinal marrow produces an immediate elevation of the tempera- ure of the posterior limbs. "A case," he adds, "has been related to e of death by injury to the spinal marrow, near the head, in which the eat of the body rose as many as twelve degrees (if I recollect right) bove the natural standard." (p. 13.) This effect he considers due to e liberation of the organic actions from the restraints under which they are carried on as long as the influence of the individuality of the nervous system is maintained. We, on the other hand, should be disposed to regard it as due to the temporary excitement of the molecular changes by the irritation produced by the section of the nerve, and propagated to its extremities: just as a similar irritation will, in many cases, produce muscular movements. If Dr. M.'s doctrine were true, the temperature of a paralyzed limb would be *permanently* hotter than that of the sound one, which everybody knows not to be the case. We are not, therefore, disposed to accord with him in the belief that the increased heat of inflamed parts is to be ascribed "more to their state of local or organic sensibility, than to the condition of their arteries, as regards circulation and secretion," (p. 15;) since we are fully convinced that it is in the altered state of the nutritive processes that we are to look for one cause of the high temperature—the increased quantity of blood sent to the part being another. It is a well-known fact that when a portion of the surface of the body is inflamed, the *respiration* (so to speak) of that portion—in other words, its conversion of the oxygen of the air in contact with it into carbonic acid,—is increased; and this, harmonizing with what we know from other sources of the relation between the disengagement of carbon from the body and the maintenance of its temperature, seems to us nearly decisive on the question.

When speaking of the *pain* of inflamed parts, we find Dr. M. using the term *sensibility* in its ordinary sense; a change of meaning which, we fear, will be rather puzzling to many of his readers. He very properly points out that the pressure of distended vessels, the tumefaction of the tissues, &c. aggravate the pain, but do not produce it; since the same degree of pressure or tension would not cause pain in parts not inflamed. We are disposed to think with him that the increased quantity of blood sent to the part will of itself augment the *impressibility* of the nervous matter, since we know how intimately its functions are connected with the activity of the circulation of the vital fluid through it.

In regard to the *treatment* of inflammation laid down by Dr. Macartney, we do not find much that calls for particular remark from us, except the *rationale* which he assigns for the effects of the various remedies which he recommends. The following is his classification of these:

> "1st. Remedies which diminish the force of the heart, and give the disposition generally to the small arteries to go into the contracted state. 2d. Means that effect a diminished size of the arteries, or reduce the sensibility in the inflamed part. 3d. Medicines that augment or reproduce the natural secretions, and thereby abate the circulation, or lessen the effusions made into inflamed parts. 4th. Counter-irritants, secretions, or impressions made in different parts from those which are inflamed. 5th. Lotions or fluids which exert sedative and astringent power. 6th. Means for affecting in an agreeable manner the sensations of inflamed parts. 7th. Causes which produce an easy or satisfied state of feeling, on the sentient surfaces, or in the individual." (p. 143.)

Under the *first* head Dr. M. of course ranks general bleeding, the intention of which "is to produce that kind of impression which is followed by a weaker action of the heart, and a more contracted state of all the smaller arteries in the body." This is doubtless true; but it is by no means the whole truth. Here and elsewhere Dr. M. has entirely passed over the changes which the vital properties of the blood undergo in this disease, and which are modified by remedial means perhaps even more than the action of the heart and the state of the vessels. The latter, indeed, being but secondary to the local changes of which we have formerly spoken, cannot be the essential object of treatment, except when it is found impossible to reach the affected part but by an impression made upon the whole system. This impression is, we think, rather communicated by the nervous system, and by a change in the circulating fluid itself, than by any alteration in the action of the heart and arterial tubes. When blood is suddenly abstracted, "the nervous system," to use Dr. M.'s rather peculiar language, "takes alarm at the rapid departure of the vital fluid; the heart beats feebly, and the arteries also sharing in the sense of danger, are disposed to contract, even beyond the degree which would be necessary to accommodate their tubes to the reduced volume of the fluid in circulation." The effect of loss of blood upon the properties of the circulating fluid itself is essentially that of lowering its vitality, as evidenced by its more rapid and at the same time less firm coagulation; and the want of harmony between the properties of the fluid and those of the solids with which it reacts is thus diminished: The influence of nauseating medicines is exercised, according to Dr. M., in the same manner as that of abstraction of blood, operating only in diminishing the force of the circulation. This, we must repeat, is but a very limited view of their efficacy. They lower the whole tone of the system, and diminish the vital activity of every part. Hence it is in the first stage of inflammation that they are most useful. The *contra-stimulant* plan of administering tartar-emetic is thus summarily dismissed: "It has become fashionable of late to give and repeat large doses of this remedy, on what grounds I cannot understand, as small doses are sufficient to produce all the effects we wish for, provided they be repeated with sufficient frequency."

Of the remedies of the *second* class, which effect a diminished size of the arteries, or reduce the sensibility of the inflamed part, *topical bleeding* is among the chief. We quite agree with Dr. M. that the rationale commonly assigned to its action—that of its directly and mechanically diminishing the quantity of blood circulating through the inflamed part, is a very fallacious one; and yet we cannot see that he has done much towards its elucidation by telling us that it operates by creating "the organic consciousness of vacuity, which always produces the tendency to contraction." Considering, as we do, that the dilated state of the capillaries is but secondary to the condition of the assimilative processes, we must look for the efficacy of this and other local remedies in its influence upon the latter. How they act it may not be easy to determine; but how much more do we know of the action of any external agents upon the system? Among the local applications to inflamed parts, those which *abstract temperature* are ranked by Dr. M. as of the highest consequence. We have already seen that, by the regulation of temperature, the supervention of an inflammatory state may be often prevented, and that the reparation of injuries may thus be effected in a manner more resembling that of simple natural growth. By the same means a considerable degree of control may be exercised over the inflammatory state of parts accessible to their application, when this has already commenced; but they must be used with judgment to be successful.

> "The reduction of the heat of any part of the body in an *extreme degree* renders the existence of inflammation impossible in that part during the time; but as cold is a direct sedative to all vital actions, there is a temporary suspension also of the process of reparation. The remedial operation of a *moderate degree of cold* is in most cases to be preferred. It diminishes in place of extinguishing sensibility and vascular action, and under its operation the reparative processes can be carried on." (p. 157.)

It is in repressing the increased action with which the *first* stage of inflammation commences that we should *à priori* look for the greatest efficacy of *cold*, since it is well known to be a sedative to vital action in general; and we think that experience fully bears out this principle. Having formerly noticed pretty fully the advantages of cooling applications, and the method of using them recommended by M. Josse, we shall not again travel over the same ground. It may be advantageous, however, to quote Dr. M.'s mode of employing *irrigation*, as a very simple and easily-managed one.

> "The most easy and manageable way of employing irrigation is to place the limb of the patient in a trough, and having laid some lint on the inflamed part, to let the water be conducted by means of a strip of woollen cloth, from a vessel holding the water or other fluid, which may be placed on a chair or table standing beside the bed. One end of the strip is to be inserted into this vessel; the other, which should be cut into a pointed shape, laid on the lint. The water will then proceed in the manner of a syphon continually from the vessel, not by drops falling from a height, the sensation of which is disagreeable. The water is carried off by a tube proceeding from the end of the trough into a vessel placed at the end of the bed. I have found that a strip of cloth of some breadth where it is inserted into the water, and ending in a point where it touches the lint, answers the purpose of a syphon much better than the filaments of candle-wick which some surgeons have employed. The patient with this apparatus is able to vary his position, which is a great comfort to him. It is obvious that irrigation can only be used with convenience to the extremities. The water may have any degree of temperature that is desired; and if it should be wished to employ iced water, the vessel holding it may be placed at distance from the patient's bed, or even outside the room, and conveyed by an elastic tube on which there is a cock, to regulate its admission into a smaller vessel, situate near the bed." (p. 159.)

Dr. Macartney lays considerable stress, and we doubt not with justice, on the propriety of applying and withdrawing cold *gradually* when an intense degree is used. This can only be proper in cases of very severe injury, where the inflammation cannot be restrained by any other means. Where the inflammation is not very violent, a moderate degree, such as that obtained by irrigation, is generally suitable, especially in summer and in warm climates. "A very simple rule," he adds, "may be safely followed with regard to the use of cold applications: which is, to consult the feelings of the patient. Wherever they alleviate the pain, they do good; and wherever they have not this effect, they are improper." It is certainly very provoking to find, as the practitioner so often does, that the application on whose efficacy he most relied must give place to one of an opposite character, from its uncongeniality to the feelings of the patient; and, while we fully agree with Dr. M. that these may be taken as our safest guide, we would also point out that the very circumstance of our being obliged to rely upon them shows that we have yet much to learn upon the subject, and that we must not neglect the consideration of any of the phenomena of diseased action, however insignificant they may appear. Having, in a former article, adverted to Dr. M.'s claims as the reviver of the remedial employment of cold water, we need not discuss these again. We hope that the publication of these statements regarding its efficacy, which his oral prelections have already diffused pretty extensively, will contribute to the more extended employment of a means of cure, the extraordinary efficacy of which in some of the most dangerous circumstances is only paralleled by its extraordinary simplicity. With the following quotation we shall conclude what we have to say of this interesting subject:

> "In a letter from Dr. Bourgen, a very intelligent pupil of mine, now settled in Demerara, received the 23d of June, 1837, he states that a medical man in extensive practice there uses water-dressing after amputation and other operations; and that these wounds healed as well as in the best-treated cases in cold climates; and that in fourteen amputations he had performed, he had not lost a single patient by tetanus." (p. 193.)

We do not find anything under the *third*, *fourth*, and *fifth* heads, which calls for especial remark, except that Dr. M. has found the *constant* application of the lead-lotion an infallible remedy for *tinea capitis*, curing even cases of several years' duration in as many days. The means by which *an agreeable state of feeling* is produced are those on which he lays most stress for the abatement of the "sense of injury" consequent upon dangerous accidents, and for the encouragement of the reparative processes, which will effect a cure when sources of irritation are removed. The use of *fomentations* is popularly known to have this effect to a certain degree; but Dr. M. has found the employment of *steam* at moderate temperatures to be far more efficacious, so as almost to deserve the character of a new remedy. Of this we have already spoken when treating of the reparative processes in our former article; and we must again refer to Dr. M.'s work for the details of its employment. The application of *water-dressing* is also ranked by him under this head; and we are glad to perceive that it is gaining ground among English surgeons, possessing, as it does in most instances, so many advantages over the common poultice. Until, however, their physiological notions are so far reformed that they can conceive that a wound may heal more advantageously without granulation and suppuration than with them, we cannot hope to see it employed as extensively as it deserves.

As to the classification of these remedies in Dr. M.'s arrangement, we must take leave to say that it appears to us even less correct than that of others which we have already criticised. The "state of feeling" of any part, whether its "sensibility" be of the conscious or unconscious kind, must depend upon the nature of the actions going on in it; and though it may indicate the normal or abnormal character of those actions, we cannot conceive how remedial applications can affect it primarily, or otherwise than through them. Remedies of this class, therefore, we should rather consider as having an immediate tendency, like others, to restore the natural actions of the part, and *thence* to produce "an agreeable state of feeling."

Dr. Macartney lays much stress, and with great propriety, upon "freedom from the sense of restraint, pressure, and friction, an easy and elevated position, and avoidance of all motion," as remedies for inflammation. Though all surgeons and patients would assent to the propriety of the above circumstances being procured, both surgeons and patients are too often practically remiss in their attention to this part of the treatment. Knowing, as we do, the extent to which a state closely resembling inflammation in many of its characters may be induced by the mechanical conditions only of the part, we cannot wonder that when this morbid action is established, it should be aggravated and maintained by such conditions, if they are allowed to remain. We must again remark, however, that the sensations produced by them are only the indications of their existence; and that it is not, therefore, the *feelings* which are to be treated, as Dr. M. seems to suppose, but the conditions which give rise to them.

With one more observation we shall conclude our review of Dr. Macartney's work. He notices at the end of it the experiments of Dr. Guyot, on the treatment of wounds by dry, heated air, a plan whose results are obviously conformable to his own doctrine of the modelling process. Some experiments on the treatment of amputations in this manner have been lately published, from which it would appear that, if the cure be not accomplished more rapidly, it takes place with less constitutional disturbance than in the ordinary manner. From the experiments of Dr. Guyot, joined to those of Dr. Macartney, it would seem well established that the temperature of the animal is the degree most favorable to the simple reparative processes by which breaches of substance are filled up by natural growth without inflammatory action. Whether dry air or steam is the best application for this purpose is a point well worthy of investigation; and we would strongly recommend the trial of both to such of our readers as have opportunities of putting them in practice. We shall now dismiss Dr. M.'s treatise, which, with all its faults, we regard as a valuable contribution to pathological science and to the healing art.

We had intended concluding this article with a pretty full analysis of the *Teoria della Flogosi* of Signor Rasori; but as our allotted space has been already occupied by subjects which we deem more important, we shall notice at present only a few points of special interest. Our general appreciation of the value of the work has already been given;* and as we have adverted in the same place to the analogies and differences between the leading doctrines of its author and those of Brown, it will be sufficient to remind our readers that he recognizes *two* diatheses as the cause of diseases; and that, while he views *debility*, requiring the administration of wine, opium, and other stimulants, as very frequently concerned, he also regards the *diathesis of stimulus* as the cause of all inflammatory diseases, and as requiring the most vigorous antiphlogistic treatment, combined with the use of those remedies which he denominates *contra-stimulant*, from their supposed power of controlling or repressing excitement without giving rise to any increase in the evacuations.

The views of Rasori, on the subject of the buffy coat, will not, we suspect, meet with the sanction of many observers in this country; yet, perhaps, they may show that further enquiry is still requisite to determine its true conditions. He affirms that its presence is to be considered in all cases a decisive proof of the existence of inflammation, and believes that its quantity is in all instances accurately indicative of the degree of inflammation. To the validity of this test he admits of no exceptions. The appearance occasionally exhibited, by the blood drawn from pregnant women, cannot, he asserts, be mistaken, by any experienced practitioner, for the true inflammatory coat, as it consists at the most in a thin, soft, muciform layer, scarcely differing in colour from serum, and unaccompanied by any of that firmness or cupping characteristic of a true buff. In all cases where a true coriaceous covering appears, he asserts, that inflammation must exist somewhere, however latent it may be. He seems, however, entirely to overlook the fact that it is possible to superinduce the true buffy coat, even on healthy blood, by the influence of repeated venesection. Another instance of similar hasty generalization is his assertion that, whenever there is an absence of the characteristic marks of inflammation after death, no inflammation could have existed during life, even for a short time. We apprehend that this dogmatical statement could not well stand the test of certain cases of rapidly-fatal erysipelatous affections either of the skin or mucous membranes.

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* Vol. vii. p. 426.

His idea of the essential nature of inflammation is so obscure that we do not pretend to understand it. He maintains that the character of an inflamed part consists in congestion of the *venous* capillaries; but that this results from a general increase of action in the arterial system, the source of which is to be found entirely in the heart. His idea, if we apprehend it aright, seems to be that all inflammation depends on a generally increased action of the heart and arteries, the local effect or increase of vascularity being determined upon some spot in those portions of the body most liable to inflammation, where the arterial capillaries happen to be somewhat more crowded than in the adjacent parts, and thus bear rather more than the average proportion to the venous capillaries in which they terminate. The blood being now introduced into the latter with more than the usual force, and in more than the ordinary quantity, in consequence of the above-mentioned increase of arterial action, they become, as a matter of course, morbidly dilated. Thus an universal "diathesis of stimulus," to use Rasori's own phrase, would seem to be presupposed, as an indispensable preliminary to the local effect. On this point, however, his disciples are very far from implicitly following their master. Tommasini, in particular, has taken great pains to make it known as his belief that very many general diseases, fevers, &c., have, on the contrary, their origin in the propagation of some local morbid excitement. The insufficiency of Rasori's hypothesis discovers itself, we think beyond dispute, when applied to inflammations originating in an obviously local cause, as, for instance, mechanical or chemical irritants.

Inflammation, he asserts, never gives rise to organized products. As for the vessels occasionally seen in the morbid exudations of lymph on serous membranes, they are by no means new formations, but merely some of the preexisting vessels of the membrane displaced, and pushed from their original nidus by the secretion of fibrine beneath them! He denies also, that ossification is ever the result of inflammation, in respect to which, as well as the previous assertions, we need hardly remind our readers that he stands in direct opposition to some of the highest authorities in morbid anatomy.

He animadverts strongly on the application of the term *weakness* to that state of vessels which exists in the chronic stage of inflammation;

and asserts that such of the so-called astringents as are known by experience to be useful in this condition, as, for example, in the latter stages of conjunctivitis, really owe their efficacy to their contra-stimulant virtue. As for opium, he bestows on it his unmeasured condemnation in all such cases; for, according to his theory, it is, and can only be under any circumstances, a stimulant, and nothing but a stimulant. Thus, like a thoroughgoing theorist, he makes all things, even facts themselves, bend to his hypotheses.

Amongst the greatest peculiarities of his system is the opinion, which he looks on as indubitable, that groups of symptoms, in all respects similar, as far as we can perceive, may arise in connexion with the opposite diathesis, a circumstance which will, of course, give rise, in many instances, to no small embarrassment as to the kind of treatment proper to be pursued. He seems in such dubious cases to renounce all further attempts at diagnosis founded on rational or physical signs, and to have recourse at once to experiment. Thus, if in an affection apparently of an inflammatory character, antiphlogistics have already been pushed to what he thinks the extreme length of safety, without benefit, he considers it a sufficient proof that the true diathesis of the malady has been mistaken; he reverses his plan, and puts the stimulant practice into full operation, beginning with moderate doses, and gradually but promptly raising them to heroic ones. Opium, ether, and wine are thus employed with as free a hand as Brown himself could have desired, and with perfect confidence of success. He gives, in an appendix, two series of cases, the first consisting of diseases treated as inflammatory by a free employment of antiphlogistic measures, till the patients were brought to the very brink of the grave, and yet eventually saved on the diagnosis being happily reformed, and an opposite line of treatment (wine and opium) liberally adopted; the second comprising cases in which the erroneous diagnosis being persisted in to the end, and copious bloodlettings and other lowering methods employed throughout, a fatal termination was the result; and here, on examination, the dead body exhibited no marks of true inflammation, simply because none had ever existed. Our practical readers will be at no loss to discover a different *rationale* for the success of the alteration in the mode of treatment in the first class of cases; since every one knows that when diseases, unquestionably inflammatory in character, have been kept in check by rigorous antiphlogistic measures, a period is very likely to supervene, in which the exhaustion of the system requires support, whilst the subsidence of the inflammation permits its administration. It is no proof of the incorrectness of the diagnosis that such should be the case, though experience has too often shown that there are members of our profession unworthy enough to take advantage of such a circumstance to raise their own reputation at the expense of that of others. The second class of Rasori's cases does not seem to us to prove anything, except upon the supposition, to the fallacy of which we have already adverted, that inflammation, existing during life, always leaves evidence of its operation in the dead body; and although errors in diagnosis will, in the present state of our knowledge, be sometimes made by the most accomplished physician, they are far from justifying Rasori's assertion, that the same group of symptoms may present themselves in totally opposite states of the system.

We shall now bring this subject for the present to a close, hoping that, however imperfect our outline may have been, it will at least serve to exhibit the more important results of modern observation, to develope the bearing of physiological doctrines, and to indicate the questions which most require elucidation. The activity with which almost every branch of enquiry into the operations of life is now being carried on leaves us no doubt that we shall, at no very distant period, have reason to recur to this topic; indeed, we hope ere long to embody some important observations on one department of it, the formation of pus, in a connected view of the progress of our knowledge regarding the physical conditions of vital action.

Art. IX.

Der Schrägschnitt, eine neue Amputationsmethode, nebst Erörterungen anderer, die Amputationen betreffender Gegenstände. Von Ernest Blasius, m.d., Professor der Chirurgie zu Halle, &c.—*Berlin*, 1838. 4to, pp. 70.

The Oblique Section.—A new Method of Amputation, to which is added, an Enquiry into other Circumstances respecting Amputation in general. By Ernest Blasius, Doctor of Medicine and Surgery, &c. &c. &c., with Six Plates.—*Berlin*, 1838. 4to, pp. 70.

In the essay before us, which extends through seventy pages of quarto, the learned Doctor has, with much labour and research, presented to his readers all the different methods of amputation which have been recommended and practised in the times of modern surgery; he has compared and contrasted them in all their bearings, by enumerating their respective advantages, and placing against the latter the objections to which they are obnoxious. The result of his investigations and experience has led him to adopt the "oblique section," as combining all the advantages on the one hand, while it eschews the evils on the other. In an operation which, however modified, must always consist in cutting through the soft parts and sawing off the bone, in such a manner as to leave a covered stump, nothing essentially new in principle can be expected; and the chief novelty of our author's method will be found in the peculiar shape of his instrument and the manner in which it is used. Both the one and the other are described with more than German minuteness; and we shall endeavour to make them intelligible to our readers by *woodcuts*, and by extracting from the book that portion which immediately bears on the operation and the precise mode of performing it, first apologizing to the Doctor for having occasionally departed from the text in order to curtail its prolixity and rectify its involutions. We fear, however, that most of our English surgeons would shrink from the quaint-looking knife which is here described and delineated, requiring the use of both hands for its guidance; and we cannot but imagine that its introduction into a London operating theatre would excite as much surprise as the sight of a regiment of soldiers armed with the six-foot double-handed broad swords, with which our ancestors were wont to deal their doughty blows.

"The characteristic feature of the oblique section," says Dr. Blasius, "is this. The soft parts are divided by two incisions, both of which have a double slanting direction, inasmuch as they are oblique to the transverse as well as to the long axis of the limb. These incisions are carried round the bone in such a manner that, when the latter has been sawn through, it will be found to occupy the deepest part of the wound, while the section of the limb, when the operation is completed, will be found to represent an oval figure, one extremity of which is situated near to the part where the saw has been applied, while the other terminates at a considerable distance beyond it. The stump, before it is closed up, resembles a funnel, from which the whole of the base, and the greater part of one side, has been removed by an oblique section; or perhaps it may still more aptly be compared to a shallow paper-cone, with its apex encircling the bone; and in the same way as we shut up a paper-cone, by folding the lappet over the top, so does the surgeon close his stump, by bringing up the lower angle of the flap, and adapting it to the corresponding fissure at the upper part of the wound." (p. 7.)

For the performance of this operation, Dr. B. uses a strong knife, five inches eight lines long in the blade, four inches long in the handle, which latter is ten lines in width and six in thickness. The blade measures eight lines across near the handle, and is single edged to within about two inches to its extremity; the remaining part is double edged, and the back is carried out so as to increase the width to thirteen lines. The blade terminates in a convex point, while the cutting edge at the back ceases suddenly, and presents a broad concave surface, adapted to afford a secure rest for the finger.

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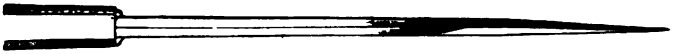

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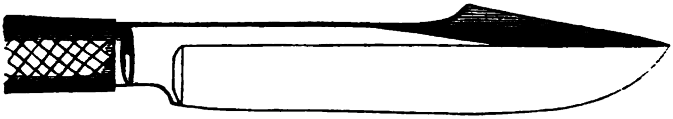
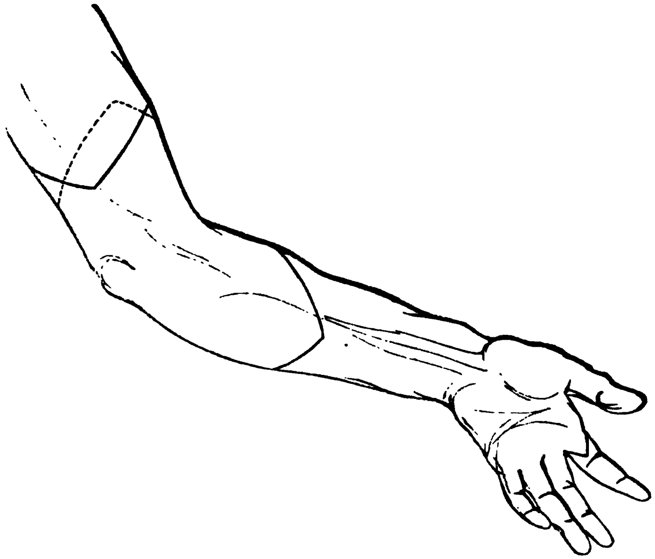


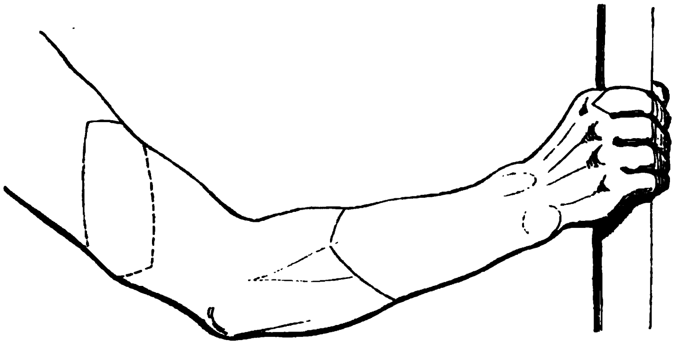
1. Back view of the knife. 2. Side view of the knife.

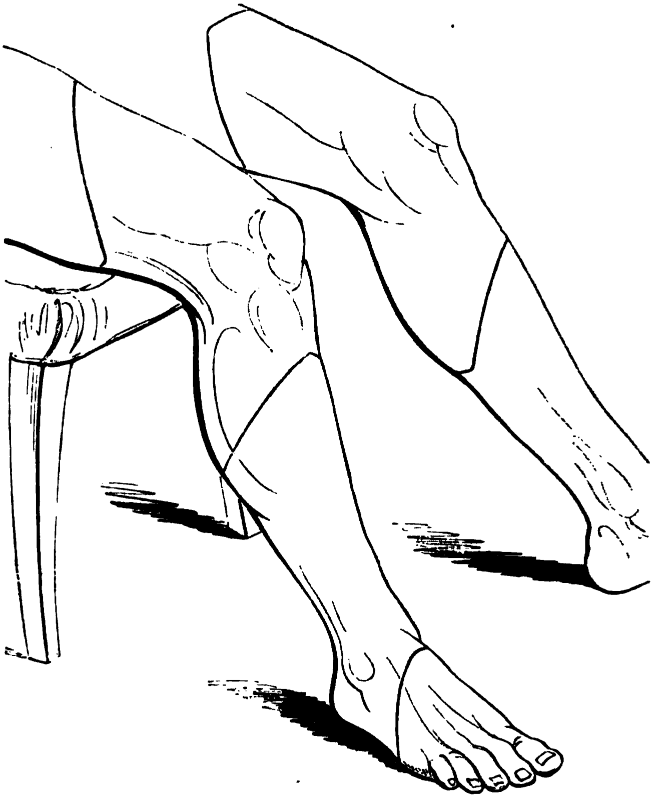
'A knife of the above dimensions may be used for removing any of the larger limbs which do not exceed the average size. For amputations below the knee in children, a smaller knife of similar shape is employed; and in certain cases, where the soft parts are few and the limbs very small, a convex scalpel will answer the purpose. It may be advisable, before commencing the operation, to trace out on the limb the track of the incisions, or at any rate to mark with accuracy the points, above and below, in which the two incisions will meet, and which constitute the two extremities of the oval surface, represented by the wound after the removal of the limb. It is not essential, for the subsequent closing up of the wound, that the two extremities of the oval should be exactly opposite, or, in other words, that an imaginary line, drawn between them, should pierce the centre of the limb; and, in fact, when there is a preponderance of soft parts on one side, it may frequently be desirable to make the incisions of different lengths, so that the longest may extend along the thickest side of the limb. By this arrangement, the edges of the flap will be more readily adjusted and brought into contact when the wound is closed. Too great a disparity, however, in the length of the cuts, throws difficulties in the way of their completion. The distance between the two extremities of the oval section, measuring along the length of the limb, must amount to from one half to two thirds of its transverse diameter, at the part where the removal takes place, the variation depending upon the greater or less degree of yielding, of which the skin and muscles may be susceptible. (Thus, if the incisions commence on the fore and terminate on the back part of the limb, the terminations would be opposite to a point distant from the commencement, from half to two thirds the diameter of the limb at the part where the bone is sawn through.) The upper extremity of the oval should lie some-

> what below the spot where the bone is cut through, indeed, just so much below, that from it the spot just mentioned may be reached with the point of the knife, when the latter is introduced obliquely, according to certain rules which will be laid down. The distance, therefore, between the two will vary from one line to half an inch, according to the thickness of the intervening soft parts between the points where the knife is first introduced and the bone. In most cases it will be found convenient to place the upper extremity of the section between the two greatest diameters, which can be drawn through the limb, from before to behind, and from side to side. Rules will be given to guide the direction of the incisions as shall render them best adapted for the performance of the different amputations, always excepting those cases where an unfavorable state of the soft parts may control the choice of the operator; and it is one great advantage of this method, that the section may be carried from before to behind, from side to side, and, in fact, may be performed through every conceivable diameter of the limb; so that an entire circumference of healthy soft parts are by no means required, but wherever disease or injury has left them in sufficient quantity, then they may be rendered available for the purpose of closing the wound."

The following outlines, taken from Dr. Blasius's figures, will facilitate the comprehension of the verbal narrative which, it must be confessed, is not very perspicuous.







"After the usual precautions for suspending hemorrhage have been taken, the operation is performed in the following manner: Two assistants are required, whose business it is to draw upwards the integuments and soft parts, and at the same time to press them firmly around the bone, just above the part where the section is to be made. The operator, standing on the right side of the limb, takes the knife in his right hand, his thumb resting in the cavity cut out to receive it at the heel of the blade, and carries the instrument under the limb, so as to reach with its point the upper angle or commencement of the section previously marked out. Holding the knife so that both the length and breadth of its blade shall be directed obliquely towards the limb, he lays his left fore-finger on the rest which is cut out on the back of the blade, and by the pressure of his finger alone pushes the point down to the bone. He then carries the edge along the indicated oblique line, keeping the convex point as closely as possible in contact with the bone, and on reaching the lower angle or point where the flap is to terminate, directs the knife so as to bring it into the second line of incision on the side of the limb opposite to that on which he has just cut. While he is thus changing the direction of the knife, a manœuvre accomplished without the least difficulty, he alters his grasp of the handle, by bringing the end of his fore-finger into the rest at the heel of the blade previously occupied by his thumb, and at the same time places his left thumb on the rest at the back of the blade. The point is again pushed to the bone by the pressure of the left thumb, and the knife is carried upwards, by observing the same rules which regulated the first incision, until it reaches the point where the operation commenced. As soon as this is accomplished, the operator seizes the lower angle of the flap with his left hand, draws it back, and by a few strokes of his knife separates the remaining soft parts from the bone, thus laying bare the latter as high as where the saw is to be applied. In limbs containing two bones a catlin must be used to separate the deep-seated muscles; but the operation is equally applicable to the removal of the thigh, the leg, the upper arm, or the fore-arm. A linen retractor is applied to hold back the flap, while the periosteum is divided and the bone sawn through in the usual manner. The wound is closed by bringing up the flap so as to unite the upper and lower angles of the section. The lower half of the oval figure is in fact folded upon the upper, so that while the surfaces of the two are brought into close apposition, their margins will be found to correspond with each other." (pp. 8-12.)

As we have ourselves found it extremely difficult to follow the doctor's descriptions (which we have partly divested of their obscurity by adopting a very free translation of the text), we beg leave to state to our readers, in a few words, what we consider to be the principle of this operation. Its object is simply to cut through a limb in an oblique plane, the incision entering the limb at a point above on the one side, and leaving it at a point below on the other side. A limb bears more or less resemblance to a cylinder; and if the knife were carried obliquely through the bone as well as the soft parts, the line of incision would be simple and straight. For obvious reasons, the bone neither can nor ought to be included in the same line of section as the soft parts; the latter are, therefore, cut in an oblique line all round the former, which is then sawn through at some distance above the commencement of the external wound. The soft parts which are thus left beyond the bone, and which form the flap, consist of integuments and muscles; but in order that the former may project beyond the latter, and more especially for the purpose of preventing the evil arising from a quantity of superficial muscle being contained in the flap, the knife, while performing its section round the bone, is held obliquely, and not at right angles, to the limb. The end of the limb will thus be found to present all that would be left of a hollow cone, after its apex had been truncated by a transverse section, and its base, together with the greater portion of one side, removed by an oblique section. The end of the bone lies in the bottom of the wound, forming the truncated extremity of the cone.

There is nothing essentially new in this operation. In principle, if not in detail, it has often been had recourse to, in cases where, either from injury to the soft parts or from disease, a deficiency of integument, necessary for circular or lateral flaps, has compelled the operator to seek a covering for his stump from whatever source he could obtain it. Compulsion rather than choice has hitherto led to its adoption; but by some surgeons it has been preferred to the more ordinary sections; and time alone can show whether the recommendation of Dr. Blasius, founded on considerable experience of its efficacy, may not render the oblique section the more common mode of amputation. The well-known method of saving a flap from the calf of the leg, so frequently had ecourse to where a circular flap cannot be obtained, differs but slightly, f we except the detail of the operation, from the oblique section so laborately described by our author.

Our own experience and observation has convinced us that the successful issue of an amputation depends mainly on the observance of two or three rules which are applicable to all methods of performing the operation. 1. It is essential to have sufficient integumental flap (which may or may not include a portion of the superficial muscles adhering to it,) to cover the stump. The size of this flap should be adapted as accurately as possible to the surface it is intended to cover; if too large, it will bag and hang loosely, and be constantly shifting its position; if too scanty, it must be strained into apposition. In either case no adhesion will be obtained. 2. The section necessary for obtaining a flap should be made by as few cuts and these executed in as clean and decisive a manner as possible; the knife should never be employed twice to effect what may be obtained by one application. Where the flap is intended to include muscle as well as integument, the knife should always, if possible, be carried to the bone in one sweep. Unless this character of decision is given to the section, the same line of animal fibre, be it muscle, cellular membrane, nerve, or vessel, necessarily becomes divided in more than one point, and the surface is jagged and irregular, instead of being smooth and clean. 3. When the flap has been completed, the deep-seated muscles should be carefully and skilfully separated from the bone up to a certain distance,—in fact, to such an extent as shall furnish a quantity of muscular fibre, thus separated, sufficient to cover the end of the bone, to form a sort of cushion before it, and separate it from the integumental flap. This constitutes in our view a most important step in the operation—one often neglected, because it requires time and mars the *eclat* which attends the rapid removal of a limb; but the neglect is the more reprehensible, inasmuch as the sin of omission incurred by the surgeon is sure to be visited on the patient, and in too many cases the thirty seconds saved in the operation by the former are afterwards paid for by the latter at the price of thirty days of protracted suffering and confinement. We think that the importance of having a cushion of deep-seated muscle to cover the bone, although constantly urged, has not been sufficiently attended to in practice, perhaps because its precise utility has not been properly understood or explained. Indeed, a muscular covering to a stump has been altogether deprecated, as being liable to subsequent contraction and exposure of the bone. To a certain extent this objection obtains, as far as the superficial or long muscles are concerned, but has certainly no reference to the deep-seated muscles which derive their origin from the bone which they immediately surround. When the superficial or long muscles (for the terms are nearly synonymous) are divided, if they contract, they are drawn towards their remaining fixed point, and, consequently, recede from the stump. Not so the deep-seated muscular fibre, which, on being separated from the bone, is at once deprived of both its fixed points, and therefore no longer retains its power of altering its position during contraction. It is immediately converted into a mass of loose muscular tissue, having no attachment whatever, save its cellular and vascular connexion to the surrounding soft parts; and the contraction of its fibres being no longer capable of influencing its position, it falls before the end of the bone and remains for ever in that situation: it therefore forms a permanent cushion composed of a material the very best that could be chosen for covering the bone.

The remaining portion of Dr. Blasius's essay is partly occupied by a comparison of his own operation with all the other methods of removing limbs which have ever been devised or executed. Then follows a minutely detailed account of the way in which each particular limb of the body should be amputated, illustrated by plates, in which the various lines of incision are marked out on the arms and legs. The whole concludes with copious remarks on amputations in general, the prognosis to be formed or the result which may be anticipated when we have recourse to the operation for different forms of disease, the after-treatment, more particularly with reference to the management of the stump, &c.

ART. X.

1. *Statistical Report on the Sickness, Mortality, and Invaliding among the Troops in the West Indies. Prepared from the Records of the Army Medical Department and War-office Returns.* Presented to both Houses of Parliament by Command of Her Majesty.—*London,* 1838. Pp. 103, and Appendix, pp. 40.
2. *Statistical Reports of the Sickness, Mortality, and Invaliding among the Troops in the United Kingdom, the Mediterranean, and British America. Prepared from the Records of the Army Medical Department and War-office Returns.* Presented to both Houses of Parliament by Command of Her Majesty.—*London,* 1839. Pp. 34, 92, and 57.
3. *A Letter to the Right Honorable the Secretary-at-War, on Sickness and Mortality in the West Indies, being a Review of Captain Tulloch's Statistical Report.* By Sir ANDREW HALLIDAY, M.D. F.R.S.E, Deputy Inspector-General of Army Hospitals.—*London,* 1839. Pp. 63.

THE first Statistical Report of Major Tulloch was undertaken for the purpose of fulfilling an enquiry suggested by the secretary-at-war into the extent and causes of the sickness and mortality among the troops in the West Indies, with a view to measures likely to diminish the great loss of life annually experienced in these colonies. It consists mainly and essentially of a digest of the portion relating to the West Indies of the Historical Record of the Medical Transactions of the British Army, which the country owes to the method and industry of the present enlightened director-general of the army medical department, Sir James M'Grigor. The reports in the Medical Board Office have been collated with the War-office returns, and thus a source of inaccuracy, from death so sudden that the patient had not been dieted in hospital, has been obviated, and likewise the average number of men constantly sick has been ascertained. From these conjoint quarters, Major Tulloch, assisted in the first instance by Mr. Marshall, and on the departure of this able statist from London, by assistant-surgeon Balfour, of the medical staff at Chatham, has compiled a statistical report, which, whether we regard the perspicuity, copiousness, and accuracy of the details, or the prudence of the deductions from them, is not surpassed by anything in the records of our art, and which we regard as equally creditable to the compilers, to the science and arrangement of the department whence the principal information was derived, and to the benevolent and patriotic motives of the secretary-at-war, Lord Howick, by whose direction the work was undertaken. It comprises the substance of the reports from the different West-Indian stations during a period of twenty years, viz. from 1817 to 1836 inclusive. The second Report, or that relating to sickness and mortality among the troops in the United Kingdom, is less comprehensive, being necessarily confined to the seven years subsequent to January, 1830; the annual regimental returns furnished to the war-office, whence the sudden deaths, and those which took place on furlough must be gathered, having only then been established. The Reports, however, from the Mediterranean and British America, comprehend the same period of time as that relative to the West Indies, and all are from the same hands, and display the same scientific method and accuracy which are so manifest in the West Indian Report.

The letter of Sir A. Halliday is intended to be at once a critique of Captain Tulloch's Report, and a Supplement to it,—supplementary of that portion of information relative to sickness and mortality in the West Indies which the Report does not profess to give, that, to wit, "which cannot be discovered by the test of facts and figures, and cannot be made the subject of calculation." Though we consider the tone of this pamphlet as often needlessly peevish, yet, knowing it to be the work of a man of talent and experience, we shall embody in our review much of the information it imparts, protesting, however, *in limine*, against the author's professed reliance on opinions relative to the origin of disease not derived from what we must take leave to consider the only sound basis of reasoning on the subject—facts and figures. It is certainly quite supposable that any medical man might have been more lavish of deductions from statistical details coupled with geological and other facts than Major Tulloch has shown himself to be; and it is the fact that Sir A. Halliday has in this pamphlet drawn conclusions from the amount of sickness and mortality in the West Indies, connected with facts which fell under his observation there, which are left untouched in the former gentleman's Report; but on examining these conclusions, we find that figures lie at the foundation of all of them, and that Sir A. Halliday has spoken in a disparaging tone of arithmetic as a ground of reasoning, which his success in employing it has shown to be unmerited. Major Tulloch's Reports being chiefly in a tabular form, and such portions of them as are employed as a commentary on the tables being written as succinctly as possible, a complete analysis of it would be little less than a transcript, and we shall consequently abstain from an attempt to condense further what already exists in the extreme of condensation. We consider, however, that we shall be doing justice to these documents and our readers by endeavouring to gather from them a portion of their more general facts, with certain of the author's deductions, which always bear the stamp of great prudence and circumspection. So far as it appears that inferences, not touched upon by Major Tulloch, are legitimately deducible from this vast repository of facts, of the nature of general truths or principles having a relation to disease wheresoever and in what class soever it prevails; and especially should they refer, either in the way of correction or confirmation, to opinions so generally received as to be considered as recognized medical doctrines, we shall not hesitate to deduce them.

The first Report presents the multitudinous statistical details comprised in it, arranged as they occur, in

1, The Windward and Leeward Command,
2, The Jamaica Command,
3, The Bahamas, and
4, The Honduras Command.

The details of the sickness and mortality in each command are preceded by a concise but, as it appears to us, very clear and accurate account of the geology and meteorology of the district in which they occurred, whilst, besides this general view, the nature of the soil and the general character of the various localities in which the troops are immediately stationed, are described.

In the colonies comprised in the Windward and Leeward Island Command, the mean temperature is about 80½°, and its extreme range is only 13°, whilst in some islands it is only 4° throughout the year. The range of the barometer is not more than from about a quarter to half an inch throughout the year, and the quantity of rain that falls is at least three times as great as in Britain, being estimated at from sixty to seventy inches annually. In this command the admissions into hospital from among the white troops amounted to 1,903 per thousand; so that, on an average, every man must have been under medical treatment once in every six months and a half, while the deaths in the same class, *in hospital*, amounted to 78·5 per thousand. Some corrections of this estimate, required by the occurrence of sudden and accidental deaths, and a discrepancy between the medical and war-office returns of the number of troops in the command, raise the proportion of deaths to 93½ per thousand; or, in other words, the eleventh part of the force has died annually. The mortality among troops in the United Kingdom being fifteen per thousand annually, it follows that the mortality is six times as great in the former situation as in the latter, though the extent of sickness, as shown by the hospital returns, has only been twice as great, an evidence of the much greater severity of the disease in the one situation than in the other.

In Jamaica, from the variety of plain and mountainous surface, almost every diversity of climate may be found. At Kingston, situated on the plain, the maximum of heat is 85°, the mean 80½°, and the minimum 76½°, on an average of all the months throughout the year; whilst at Maroon Town, the highest station occupied by our troops, the maximum is 80½°, the medium 74½°, and the minimum 68½°, likewise on an average of months throughout the year. The quantity of rain which falls annually is fifty inches. In this island the mortality (after all due corrections) is found to be 143 per thousand; or a seventh part of the force has died annually. The four most healthy years were 1823, 1828, 1829, and 1836, when the deaths averaged only 67 per thousand, whilst the most unhealthy were, 1819, 1822, 1825, and 1827, when the deaths averaged 259 per thousand annually. The average being thus during the most healthy period four or five times as great as among troops in Britain, and in unhealthy years nearly sixteen times as great. As an instance of the extreme insalubrity of Jamaica, Major Tulloch mentions that, in the seventeenth century, of 800 troops who arrived in the island, two thirds died in a fortnight.

In Bahamas the maximum temperature (taken, as in the former instance, on an average of months throughout the year,) is 83½°, the mean 78½°, and the minimum 74°. A considerable quantity of rain falls, but the reporter is not acquainted with the exact measurement of it. The admissions into hospital have been 1,430 per thousand annually, and the deaths 200 per thousand, or a fifth of the whole force. This extraordinary mortality, above thirteen times as great as among troops in the United Kingdom, Major Tulloch is inclined to ascribe rather to the unhealthy site of Fort Charlotte Barrack, where the troops are stationed, than to the general insalubrity of the climate, finding that, of the white population of the town of Nassau, only 66 per thousand die annually, being only thrice the rate which prevails in Great Britain.

In Honduras, the average maximum of the thermometer is 81½°, medium 79¼°, and minimum 77°. It rains during five months of the year, and the quantity that falls is very considerable, but it has not been specifically measured. The average annual ratio of admissions has been 1,209 per thousand and 103 deaths, a ratio intermediate between the Windward and Leeward Command and Jamaica. Major Tulloch, however, accords with the current opinion, that this station is more favorable to the health of Europeans than most in the West Indies, finding that the returns of mortality have been swollen by an extremely fatal fever which prevailed in the settlement in 1826.

The main source of this frightful mortality among the white troops throughout these colonies is one form or other of fever, especially the remittent, and that species of disease which is sometimes included in this class, and sometimes mentioned apart, according to the peculiar views of medical men, *yellow fever;* for whilst intermittent and certain other fevers contribute largely to the admissions into hospital, the mortality from them is small. We take the relative mortality from these fevers in the two larger commands to illustrate this point. In the Windward and Leeward Command, whilst intermittents of all types are fatal to one in 159 attacked, remittents are so in the proportion of one in nine, and yellow fever in that of one in one and two thirds. In Jamaica, whilst intermittents are fatal to one in 165, and synochus only to one in 448, remittents destroy one in eight, and yellow fever one in one and a third. When it is considered that the number admitted under the head of remittents is very large in the returns, they constituting nearly one third of the admissions from fever in the Windward and Leeward station, and considerably above four fifths in the Jamaica returns, the reader will reach the conclusion that it must be a main instrument of the fatality of these colonies, without our copying all the statistical details by which it is demonstrated.

The author applies himself to the investigation of the causes producing these fatal diseases; but rather, to our mind, with the effect of showing that medical men have much to reconsider in their existing opinions (a valuable effect, we would remark,) than of establishing any decisive conclusion of his own. Heat presents itself as a probable cause, but he objects to this, that in Antigua and Barbadoes, where the range of the thermometer is rather higher than in Dominica, Tobago, Jamaica, or the Bahamas, the sickness amounts to little more than one third of its prevalence in the latter stations. The prevalence, too, of epidemic fever during the winter months, of which the reports furnish many examples, is an argument against the abstract effect of heat. Moisture, abstractedly considered as a cause of disease, is met by similar arguments. British Guiana has more rain by one half than Jamaica, but the mortality among troops in the latter situation is twice as great as in the former. Were excess of moisture the cause of the excess of disease in the western hemisphere, the same effect should be expected from it in the east; but the Malabar coast, which is deluged with rain six months in the year, is generally the most healthy quarter in the Madras presidency. We would remark on this reasoning of the author, in the force of which we fully concur, that it is in accordance with the received medical opinions on the subject, according to which an absolutely wet surface is less noxious than one in the intermediate condition between wetness and dryness. This cannot be more forcibly illustrated than by the remark of travellers in Africa. When the rain begins to fall there, the emanations are so noxious that the inhabitants retire to their houses, and endeavour to exclude even the least access of air. As the rain continues, and the ground becomes thoroughly wetted, the sickness abates, to be renewed with greater violence on the retiring of the rains and the ground becoming dry.*

The reporter, however, finally concludes that there is an influence in the production of sickness in the conjoined operation of heat and moisture. After stating that numerous instances have occurred in which, of adjacent islands, or even contiguous stations of the same island, subject of course in an equal degree to the operation of these agencies, the one has been desolated with fever, whilst the other has enjoyed a degree of salubrity equal to that of Great Britain, he yet finds on an average of a series of years (though not uniformly or equally in each year), that the greatest number of admissions into hospital and deaths take place in those months when the greatest degree of heat is combined with the greatest moisture.

The varieties in the physical and geological character of the soil the author does not regard as throwing any light on the subject under investigation. At many stations where the soil is exactly the same, the rate of mortality is very different; and on the other hand, where soils differ, the mortalities accord. While the soil and the physical characters, he remarks, are the same in every year, the sickness and mortality are extremely variable, and only in certain seasons attain an extraordinary degree of intensity. This remark is true, not only of the West Indies, but of every other climate in the world. We should have expected so philosophic a writer to have sought an explanation only of the ordinary state of insalubrity of any given district in its permanent condition. Epidemic prevalence is something extrinsic and superadded; and however glad we should have been to derive from a statistical, or any source, an explanation of these visitations, we cannot help thinking this argument derived from them misplaced, when it is sought to be applied to the connexion, real or supposed, between the physical and geological character of the soil and the rate of sickness and mortality.

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* Cyclopædia of Practical Medicine, article Malaria and Miasma, vol. iii. p 61.

To the agency of marshes in generating fever, Major Tulloch attaches less importance than the majority of his professional readers, we are convinced, will expect. He says:

> "That the vicinity of marshes, swamps, and lagoons, is generally subject to fevers, both of the intermittent and remittent type, is a fact sufficiently established by multiplied experience, both in tropical countries and within the temperate zones. But that remittents or yellow fever may be generated where no such cause is in operation to produce it; and that, consequently, it is impossible to establish a necessary connexion between this cause and the appearance of that disease, is sufficiently established by the fact, that the sickness and mortality in British Guiana and Honduras, where swamps and marshes most abound, are considerably less than at Up-Park Camp, and several other stations in Jamaica, remote from the operations of any such agencies." (*West Indian Report*, p. 102.)

This extreme insalubrity of certain of the Jamaica stations, especially that of Up-Park Camp, it is extremely difficult, or rather impossible, to explain upon principles ordinarily received regarding the generation of the class of fevers which produce the mortality there. This latter station is described as being about two miles north of the sea-port of Kingston, and lying at the eastern extremity of a well-cultivated and fertile plain, having a graduated slope towards the sea. About a mile to the north is a mountain ridge, the foot of which is slightly wooded, and four or five miles to the eastward extends another of greater elevation, but of which the surface is clear and open. There is no marshy or swampy ground nearer than three or four miles, and the soil is of so absorbent a nature, that it is necessary to dig to a considerable depth before water can be procured. The camp is elevated 200 feet above the level of the sea, and enjoys the advantage of a regular sea and land breeze, the former during the day and the latter during the night, which reduces the temperature considerably below what prevails at most of the other stations. The buildings occupied by the troops are raised on arches, well ventilated, and consist of three ranges, those for the officers in front, for the men in the centre, and for the hospital in the rear. The barracks are of brick, two stories high, the hospital of the same material, but of one story and a basement, with balconies all round.

Notwithstanding these apparent advantages, the mortality has been considerably above the average of this unhealthy island, being in the ratio of 140·6 per thousand, of the mean strength annually on an average of 20 years. This mortality is principally attributable to fevers, 120·8 per thousand of the mean strength having perished annually from this cause alone.

All the circumstances stated respecting Maroon Town, one of the healthiest stations, not only in Jamaica, but in the West India colonies generally, would, according to received opinions, threaten a much greater mortality than Up-Park Camp, if we except the greater elevation and more moderate temperature of the former station, for this post is situated on an elevation of upwards of 2,000 feet above the level of the sea, and the thermometer seldom rises higher than 80°, and is sometimes at night in winter at 52°. Being surrounded by high mountains, the climate is variable, and the temperature liable to sudden transitions. There is much rain, and the evaporation subsequently caused by a tropical sun produces frequent and dense fogs. The soil is a deep red clayey loam, extremely tenacious, rendering it almost impracticable to walk out for some hours after a shower, and retaining moisture for a very considerable time. Yet at this station the actual mortality has not exceeded 22 per thousand of the force annually, being the same as among the foot-guards in London on the average of the last seven years.

The circumstance to which Maroon Town owes its salubrity appears to be the great elevation and low temperature; for though there may be periods when the wetness of the climate and the impermeable nature of the soil may place the surface in a state of moisture beyond what is favorable to the production of malaria, yet there must be comparatively dry periods when such a locality, but for its elevation, would be pestilential in the extreme. This subject of elevation is shown to be a very important one, and fortunately it is one on which Major Tulloch and Sir A. Halliday are generally in accordance. The researches of Humboldt have shown that yellow fever is never known beyond the height of 2,500 feet; and Major Tulloch shows that at an elevation of from 2,000 to 2,500 feet, troops are likely to be wholly exempt from fever, or to encounter it in so very modified a form, that the mortality from all causes will not, on an average of a series of years, materially exceed that to which an equal number of European troops would be subject in the capital of their native country. He adds, very happily, that the diseases of the tropics seem, like the vegetable productions of the same regions, to be restricted to certain altitudes and particular degrees of temperature. Sir A. Halliday agrees generally with these views, but qualifies them by stating that an airy, *isolated* mountain in the neighbourhood of a pestilential marsh will always be more unhealthy than a marsh itself. He questions if a single mountain, rising out of a marshy plain, would be quite free from the influence of the poison, at even the elevation of 2,500 feet, but is certain that, as regards a range of mountains, that the third from the marsh will prove quite healthy at a much lower elevation than that fixed upon by Humboldt, and even if it is considerably lower than the two that intervene. We attach considerable importance to this statement, proceeding as it does from a man of Sir A. Halliday's observation and experience, and we would remark generally that this writer's observations on the localities to be selected as stations for troops in the West Indies indicate deep study and profound knowledge of the subject.

Sir A. Halliday adverts to certain sources of sickness and mortality which are left untouched in the Report, being indeed such as only a professional man could discern: one is deficient experience in the medical men in these colonies, arising from the constant changes consequent on the want of all inducement, either in the shape of colonial allowances or the prospect of promotion, to remain in so noisome a climate; vacancies, when they occur, being filled up from home, not from those in the station, medical men may be said rather to pass through these colonies than remain in them, and hence the soldiers are attended in sickness by a succession of inexperienced officers. The evils of this system, if it is correctly described by the author, must be very great; for in the epidemics or endemics of any given country the practitioner is useless, or worse, if inexperienced. The tone in which another supposed source of sickness and mortality is dwelt upon by Sir A. Halliday is creditable to his feelings as a man and a Christian. Believing that one half of the sickness and mortality which prevail among the troops arises from drunkenness and other immoralities, he forcibly and consistently contends for an extension of the present very deficient means of religious care and instruction, for the purpose of mitigating these evils. There is only one military chaplain for the whole Windward and Leeward Island command, and he is stationed in an island, Trinidad. There is in the different garrisons a church-parade every Sunday morning, "where some clergyman of the colony reads over, in a hurried manner, the prayers of the morning service, or perhaps only a part of them, and of what he does utter, few, if any, of the soldiers can hear one word." This mere congregational ministration, performed in the manner described by Sir A. Halliday, will probably be destitute of all influence, or certainly possess an influence very inferior to that of the more intimate and searching communications for which the seclusion of the hospital and the season of sickness and distress furnish the most fitting opportunities. Such communications between clergymen and soldiers cannot take place under the existing circumstances of the different garrisons; and it is suggested that in various stations there should be two regularly-commissioned chaplains, a Protestant and a Roman Catholic. In favour of the latter appointment, he quotes a very impressive case, the force of which and of the author's advocacy of the measure we strongly feel; but we can tell him, that were government to act on his just and well-intentioned suggestion, a fearful outcry would be raised by a numerous and powerful party in the state, that they were employing their influence, and spending the wealth of the country for the propagation of error on a subject regarding which, above all others, only truth should be taught. We mention this not in censure of the proposal abstractedly—for why should not the poor Catholic soldier have the consolation of that form of Christianity which alone can reach his heart?—but to point out to him the difference of position between irresponsible advisers and responsible actors.

As a means of diminishing the mortality in the West Indies, Sir A. Halliday calls the attention of the secretary-at-war and the public to Dr. Carl Warburg's fever-drops. We do not object to anything Sir Andrew says on the subject. He has expressed, as he was bound to do, his conviction of the efficacy of this medicine in a disease over which the reports before us prove but too distinctly that our power has hitherto been very limited. We cannot, however, avoid a degree of prejudice against secret nostrums; and the weight of miscellaneous testimony adduced in advertisements which meet our eyes in various quarters, serves only, by calling up unpleasant reminiscences and associations, to render this feeling more intense in the case of the present medicine. But our conviction being that the knowledge of the real pathology of fever is still very imperfect, and that the modifications of our remedial means derived from this imperfect pathology have tended little, if at all.

to increase our power over the fevers, either of the West Indies or any other climate, we do not feel justified in rejecting assistance, in however questionable a shape it may present itself. The matter is a public one, and to government we think belongs the office, in the first place, of obtaining testimony, which should be unquestionable in character and ample in amount, of the efficacy of the medicine, and if such is obtained, of purchasing the secret for the public good. The station of Great Britain among nations, and particularly the extent of her colonial possessions render her the Power to which the investigation and the purchase properly belong; and for the former purpose her military hospitals in various quarters of the world furnish ready means, whilst, we are convinced, the different medical charities throughout this country (for Dr. Warburg informs us that it is a certain remedy for fever of any type) would cooperate in a work of general beneficence.

There is nothing more interesting in these Reports than the statistics of pulmonary diseases. They appear to us of great importance in reference to the etiology of these formidable maladies, by showing at least the error of certain views still prevailing on the subject, and by confirming, if they have not the merit of originating, more correct opinions. After a table showing the number of admissions and deaths from this class of diseases, in the Windward and Leeward Command, Major Tulloch remarks: "Though the proportion of admissions by this class of diseases is lower than among troops in the United Kingdom, in the proportion of 115 to 148, the ratio of mortality is much higher, as nearly 10½ per thousand of the strength have been cut off annually; whereas, in Britain, the deaths from the same class of diseases do not average at the utmost, more than 8½ per thousand. This arises from the *greater prevalence of consumption*, for out of an aggregate strength of 86,661 serving in the Windward and Leeward Command, not fewer than 1,023 were attacked by that fatal disease, being 12 per thousand annually, whilst out of an aggregate strength of 44,611 dragoon-guards and dragoons serving in Great Britain, only 286 were attacked, being about 5½ per thousand." "Not only," he adds, "is consumption productive of great mortality in this command, but inflammation of the lungs and chronic catarrh are nearly twice as prevalent and twice as fatal as among troops in Britain, thus showing how little effect a mere increase of temperature has in modifying these diseases." (*West Indian Report*, p. 8.) Major Tulloch might have extended his remark to uniformity of temperature; for this great mortality from consumption and inflammation of the pulmonary organs has taken place in a command where the greatest annual range in the thermometer is 13°, and in some parts of which it is only 4°.

In Barbadoes the mortality from diseases of the lungs is considerably above the very high average of the command, being 15·8 per thousand annually of the white and 18·7 per thousand of the black troops. It may not be amiss to remark that, whilst this report shows the general rate of mortality from fever to be considerably lower among the negroes than the whites, the former are found to suffer in a much greater proportion from consumption and other diseases of the lungs. In Jamaica pulmonary diseases are by one third less prevalent, and one third less fatal than in the Windward and Leeward Command. The mortality from them is estimated in the Report at 7·5 per thousand annually. The author remarks that, exclusive of those invalided on account of these diseases in the island and who died in their passage home or shortly after their arrival, this class of diseases has produced almost the same annual ratio of mortality as among the dragoon-guards and dragoons in the United Kingdom on the average of the last seven years. Consumption is, however, much more prevalent in Jamaica than Britain; for, whilst in that island 13 per thousand of the whole force have been annually treated for this disease, at home those who have undergone treatment on account of it have amounted only to between 5 and 6 per thousand annually, although the period over which the latter observations extend includes two severe epidemics of influenza, which probably laid the foundation of more cases of this disease than usually occur in this country. The author adds:

> "That this fact is the more remarkable, as in Jamaica catarrhal affections are not one half so common as in Britain. Out of an aggregate strength of 51,567, there occurred but 2,809 cases, including both acute and chronic, or 65 per thousand of the strength annually; whereas, in this country, out of an aggregate strength of 44,611, no less than 5,462 cases are recorded, or 122 per thousand annually. Inflammation of the lungs is still more rare. The baneful influence of the climate of the West Indies in accelerating the progress of consumption has long been remarked by medical authorities; but it does not seem to have occurred to them, nor indeed had they any means of ascertaining, that at least twice as many cases originate in that climate as at home, though those catarrhal affections to which they are generally attributed are there comparatively so rare." (*West India Report*, p. 47.)

We find, then, in Jamaica, with a high temperature, that catarrh and inflammation of the lungs are rare, whilst consumption is twice as frequent as in Britain. The confirmation this affords of the opinion of Louis, and its refutation of that of Broussais are too evident to require to be indicated. These statistical details have an important bearing, too, on an opinion promulgated by the late Dr. Wells, that there was a natural antagonism between diseases, the product of marshy effluvia and consumption—that the one excluded the other. In the West Indies, however, we see the prevalence of the two diseases, supposed to exclude each other, coinciding.

The portion of the reports from the Mediterranean bearing on the same subject, contains much interesting matter. At Gibraltar we find that the deaths from diseases of the lungs in general amount to 5·3 per thousand of the average force, whilst the mortality from consumption alone amounts to 3·5 per thousand. Major Tulloch presents us with the following commentary on the return:

> "The ratio of admissions by this class of diseases is to that in the United Kingdom as 141 in 148, the principal difference being that catarrhal affections are less frequent in Gibraltar, while inflammation of the lungs is much more so; the cases of the latter are, however, of a milder character, as only 1 in 45 died of those admitted into hospital in Gibraltar, while 1 in 18 died of those admitted for the same cause among the dragoon-guards and dragoons in the United Kingdom. The total mortality by diseases of the lungs would appear to be less at this station than at home; but that, we apprehend, arises from many of the consumptive patients being invalided, who, if they die on their passage or after their arrival in England, are not included in the returns of the station where their diseases originated. That this is sufficient to account for the difference may be supposed from the fact stated in the Medical Report for

1835, that during the thirteen years previous the average number of deaths from consumption in Gibraltar was $12\frac{3}{10}$ annually, besides about five sent home labouring under the same disease, of whom few or none recovered." (*United Kingdom and Mediterranean Report*, p. 11, *a*.)

The mortality from diseases of the lungs in Malta has been in the ratio of six per thousand during a period of twenty years. The author gives the following important commentary on the table containing the details:

> "The climate of this island appears from the preceding results to be by no means favorable to persons predisposed to these diseases; the mortality is higher than in Gibraltar, and there is every reason to believe that, could we have taken into account the number invalided, and who died on the passage, it would have proved even higher than at home. It is somewhat remarkable that, in a climate where the thermometer never sinks to the freezing point, where the temperature at night is generally within a few degrees the same as during the day, and where those sudden transitions from heat to cold, to which this class of diseases is generally attributed in other countries, are extremely rare, the ratio of admissions should be only about one fifth less than in the United Kingdom. It may serve as a striking illustration how little influence the climate of Malta is likely to have in diminishing the tendency to consumption, that the proportion attacked by that disease among the troops there during the last seven years has averaged $6\frac{7}{10}$ per thousand of the strength annually, while in the United Kingdom, during the same period, the proportion attacked of the dragoon-guards and dragoons was but $6\frac{4}{10}$ per thousand annually." (*United Kingdom and Mediterranean Report*, p. 24.)

The author adds some remarks to show that the fatal influence of diseases of the lungs is not confined to the troops alone, but extends in a corresponding degree to the inhabitants. He refers to returns which prove that 6,664 deaths have occurred in 13 years from this class of diseases, constituting a mortality of 513 annually, which, upon an average population of 100,000 of all ages, is about $5\frac{1}{8}$ per thousand of the strength, being scarcely one per thousand less than among the troops, notwithstanding the night exposure of the latter in the course of their military duties. He adds that, though the climate of this island has been supposed favorable to diseases of the lungs, its inhabitants appear to suffer from them nearly as much as those of high northern latitudes, for the returns of Sweden show that there were only 14,087 deaths from this class of diseases out of the whole population in one year, being in the ratio of $5\frac{6}{10}$ per thousand, or within a fraction the same as in Malta.

In the Ionian Islands, the death, from all diseases of the lungs are 4·8 per thousand of the mean strength annually. Major Tulloch makes the following remarks on these numbers:

> "Notwithstanding the variable character of the climate, the rapid alternations of temperature, and the tempestuous weather which frequently prevails in this command, diseases of the lungs are both less prevalent and less fatal than at Malta or Gibraltar: the admissions into hospitals being respectively as 90, 120, and 141, and the deaths as 4·8, 6·0, and 5·3 per thousand of the strength annually. The principal exemption in the Ionian Islands is from catarrhal affections, which are not half so prevalent or half so productive of mortality as in the other Mediterranean stations, or in the United Kingdom. Most of the deaths arise from consumption, but neither is the proportion attacked so high nor are the fatal cases so numerous as in Malta, where there exists a comparatively equable temperature, and that mild condition of the atmosphere which is supposed favorable to persons predisposed to that disease.

In Malta, on the average of twenty years, about 6 per thousand of the troops have been attacked annually by consumption; and in Gibraltar and the United Kingdom, nearly the same ratio; while in the Ionian Islands only 5 per thousand have been attacked, and the deaths have been fewer in the same proportion." (*United Kingdom and Mediterranean Report*, p. 35, *a.*)

When we pursue the subject through the reports from British America, we observe the same discrepancy between ordinarily-received opinions and statistical facts as has been displayed in the results of observation in other quarters of the globe. In Bermudas, with great uniformity of climate, and an absence of those extremes of cold to which such diseases in northern latitudes are frequently ascribed, we find inflammation of the lungs and consumption decidedly prevalent, and the mortality from pulmonary disease 8·7 per thousand annually, which is higher than among troops in the United Kingdom and the Mediterranean stations. In Nova Scotia and New Brunswick, with severe winters and sudden atmospherical vicissitudes, we find diseases of the lungs less prevalent than in the United Kingdom, in the proportion of 125 to 148, and less fatal in the proportion of 7·1 to 7·7. In Canada, distinguished for the severity of its winters, and so remarkable for sudden alternations of temperature that the thermometer has been known to fall at Quebec 70° in twelve hours; the admissions for pulmonary disease are 148, and the deaths 6·7 per thousand, the latter being much lower than in the United Kingdom. Major Tulloch points out the following striking facts, which require no comment: At Bermuda, there have been attacked annually by consumption, of every thousand, 8·8; in Gibraltar, 6·5; and in Canada, 6·5.

Major Tulloch modestly observes, that his object in the Reports is rather to state effects than to speculate on causes. We admit that he has been more successful in the case of pulmonary diseases, in showing what are not their causes than what are; but we feel, too, that in the state of our knowledge respecting these diseases, the former knowledge is a necessary and very important preliminary to the latter. One set of facts, however, in these Reports has an important bearing on another. We find between the classes, officers and soldiers, the most perfect equality exists as to mortality from fever, whence the reasonable inference is, that the cause of fever is general—that it is in the climate. But in the case of bowel complaints and diseases of the lungs, there is the greatest discrepancy in the extent to which these classes are respectively affected. From the former disease, the soldiers suffered in comparison of the officers in the proportion of nine to one, where, for five days in the week, the diet of the soldiers consisted of salt provisions; in colonies, on the other hand, where such provisions were issued to the troops only two days in the week, the mortality in the two ranks from these diseases approximates so closely as to be nearly on a par. Here is evidence almost demonstrative, which is further confirmed by the fact stated by Sir A. Halliday, that the fresh-meat rations, supplied to the soldiers at the request of Lord Howick, have effected a great diminution in the prevalence of bowel complaints. The relative prevalence and fatality of consumption in the two classes is very disproportionate; for in the Windward and Leeward command, the proportion of officers and men treated for the disease stands as 6 to 15; and in the Jamaica command, as 4 to 15; whilst the deaths among officers are, in the former station, one fourth, and in the latter one fifth, of what occurs among the troops generally.

To what is this prodigious discrepancy to be attributed? The author declines hazarding a positive opinion; but he refers to the views of Sir James Clark (and we are happy to observe his respectable and respectful reference to the labours of this truly enlightened physician), that improper diet and impure air are the most certain exciting causes of consumption among those not hereditarily predisposed to it, and to the experiments cited by him, which have proved that tubercular affections may be induced in animals by confinement in close humid places and innutritious food. Major Tulloch deems it consequently not improbable that crowded barrack-rooms and a restriction to salt-diet may, particularly in a tropical climate, produce a similar effect on the constitution of soldiers.

Sir A. Halliday, though he admits the *predisposing* influence of the causes assigned by Major Tulloch, thinks that the exciting cause of the prevalence of pulmonary disease among the soldiers is to be found in the barracks being generally seated on some lofty eminence. The officers' journeys, he says, to and from these stations, are less frequent than those of the soldiers, and are performed in a less hurried manner; the latter, from overstaying their leave, being obliged to hasten to barracks, and this too when they are heated by indulgence in the grog-shops. We do not feel satisfied with this *rider* to Major Tulloch's very reasonable deduction, thinking the arguments adduced in its support, anything rather than conclusive. For instance, we find the following statement: "The troops at Antigua are principally quartered on a range of heights about 400 feet above the level of the sea, commanding the entrance to English harbour. The dryness of the atmosphere in this island is also remarkable; and hence we find that the ratio of deaths from diseases of the lungs is as high as nine per thousand." Now, besides that the argument here is so illogically stated, that we are at a loss to know whether the "range of heights," or "the dryness of the atmosphere," is the source to which the *high* rate of mortality is traced; we find, that in the Windward and Leeward Island command, in which Antigua is situated, the average mortality from pulmonary disease is ten per thousand, so that the heights or the dryness, as the case may be, reduce instead of augmenting the mortality.

For only one other subject contained in these Reports (desirous though we are to put the reader into as full possession as possible of their contents) can we at present find space in our pages. This subject is the sickness and mortality among troops in the United Kingdom. We shall not detail the reasons which have induced the reporter to confine his examination to the casualties of the household troops and dragoons; suffice it to say, that these forces alone can furnish correct information as to the influence of the climate of Great Britain on the health of soldiers.

Among dragoon guards and dragoons, the admissions into hospitals amount to 929 per thousand annually, and the deaths in hospitals to 14 per thousand; and if we add to this latter number the deaths from suicide, murder, drowning, and other casualties, amounting to $1\frac{3}{10}$ per thousand, we have an annual mortality of $15\frac{3}{10}$ per thousand of the mean strength. The estimate was formed from a period, during a portion of which cholera and influenza prevailed, and 2 per thousand will be a reasonable deduction for the excess of mortality produced by these diseases: consequently $13\frac{3}{10}$ per thousand will form the average mortality. The age of the soldiers constituting this force averages from 29 to 30. A fair estimate of the mortality among persons of the same age in civil life shows it to be at the rate of 11·5 per thousand annually.

This is a great discrepancy, and of a kind to indicate that the military profession, even under the most favorable circumstances, operates prejudicially to the health of those employed in it. It should be remembered, however, that the estimate of those in civil life is drawn from the kingdom generally, which furnishes a much lower rate than would be formed were the estimate deduced from the civil population of towns, in which, be it remarked, the troops are quartered. The mortality of an urban population in civil life, of the same age as the soldiers, is 16 per thousand annually; it hence appears that the apparent high ratio among the troops, as compared with the general mass of the population, arises not so much from any deteriorating influence in their profession as from the disadvantage of their being subject to the insalubrious atmosphere of densely-populated districts.

In the foot-guards the ratio of mortality is 21·6 per thousand of the mean strength annually, or nearly one half higher than in the dragoon-guards and dragoons, and this excess of mortality arises entirely from diseases of the lungs. These troops are principally, as is known, stationed in the metropolis; but the conclusion that this striking excess is produced by the climate of London would be inadmissible, for it is fully shown that the climate of London is not more insalubrious than that of many other towns in which the troops are quartered throughout the kingdom. When the mortality among other classes exposed to the influence of a metropolitan atmosphere is examined, we find it in much lower ratio than that of the foot-guards. The average annual mortality of the civil population, between the ages of 20 and 40, is 15 per thousand; that of the East India Company's labourers 12¼ per thousand, at the same period of life; and that of the household cavalry, likewise exposed to the climate of the metropolis, is 14·5, or not so high by one half as among the foot-guards.

It is stated that diseases of the lungs are the source of the prodigious excess of mortality in these troops, as compared with other classes of the military in this country. The following numbers show this statement to be correct. Of the dragoon-guards and dragoons there die annually, from diseases of the lungs, 7·7 per thousand; of the household cavalry, 8·1; of the soldiers of the West Indian depôts, which are stationed in England, a proportion of whom have been in the West Indies, and are likely to have received some contamination of their constitution from a climate tending to induce consumption, 9·6; of the foot-guards, 14·1. When the civil population is selected for comparison, the same preponderance is manifest; for, by calculations deduced from the London bills of mortality from 1830 to 1835, it has been ascertained that out of a thousand deaths among the civil population, the number by diseases of the lungs were 328, or scarcely one third of the whole; whereas, out of

745 deaths among the foot-guards, no less than 487, or upwards of two thirds, were from these diseases.

Major Tulloch offers no explanation of the extreme prevalence of this class of diseases in the guards, beyond the remark that it "originates in some peculiarity in the moral or physical condition of that description of troops, from which the others are comparatively exempt." In this view all must concur. The excess cannot depend on the climate of England or even in London; it lies within the very narrow sphere of the guards themselves, and its cause might, we think, be ascertained without much difficulty; and it is material that it should; for, in a minor degree, this excess of diseases of the lungs pervades the army at home generally when it is compared with the civil population; and it seems probable that the whole is dependent on some common cause, which exists in its greatest intensity in the guards. The report informs us that "nearly four fifths of the fatal cases of diseases of the lungs arise from consumption, being as many as from all other causes in the army at home. The highest estimates in civil life rate the mortality from this disease at a seventh part of the deaths of all ages; or, if the observation is confined to adults alone, it may possibly amount to a fourth part, being at the utmost only half as high as among the troops."

The congregation of soldiers into barracks is the main distinction, which can be supposed to affect their health prejudicially, between them and individuals of the same station in society around them; and to crowded barracks, and consequent atmospheric impurity, we are strongly disposed to ascribe the proneness to consumption among our troops. We cannot now enlarge on this opinion, or on certain reasons, extrinsic to the disease now under consideration, we have for holding it. That Major Tulloch's suspicions, at least, take the same direction is manifest from the following remark:

> "From the very extensive calculations of Mons. Lombard, on the influence of professions on consumption at Geneva, it has been found that persons whose occupations were of an active description, inducing muscular exercise, and carried on in the open air, were not half so liable to that disease as those whose occupations were sedentary or carried on in shops and manufactories. If the aggregation of a number of men into one apartment, even though the space is not very confined, creates a tendency to this disease, then it clearly points out the propriety of affording the soldier as ample barrack accommodation as possible, not with a view to his comfort alone, for that is a matter of minor consideration, but in order to check the ravages of a disease which creates more mortality than all the others to which troops in this country are subject." (*United Kingdom Report*, p. 13.)

We now take leave of these Reports, with the opinion we formed of their value from a first perusal enhanced by further attention to their contents. They constitute a rich repository of facts from which practical lessons may be immediately drawn, and to which many an enquirer will turn in a future day, and invariably with profit. The pamphlet of Sir A. Halliday is not so closely reasoned as the reports; it contains, also, some important errors, which have been pointed out by Major Tulloch since its publication; but it is, on the whole, a very creditable performance, and does honour to the zeal and good feeling of the author.

ART. XI.

Illustrations of the Comparative Anatomy of the Nervous System. By JOSEPH SWAN. Parts III. and IV.—*London*, 1837—1839. 4to, pp. 53 to 106. Pl. 14 to 25.

ON a former occasion we expressed our opinion of the utility of this work, and the present numbers have justified the approbation we then bestowed upon it. We are perfectly satisfied that it will tend still further to advance the high reputation which Mr. Swan has already acquired, as a practical anatomist of the nervous system. It is much to be hoped that the example set in this work, of graphically illustrating the structure and distribution of the nervous system in the different classes of animals, will soon be followed by illustrations of the other organs of the body. Works of this description, as we before remarked, are much wanted, and would prove highly useful to the student in comparative anatomy, by serving as guides to direct him in his practical examinations of the internal organs in different classes of animals. Nearly all the great discoveries in the structure and functions of the different parts of the human body have derived their origin from, or have been confirmed and rendered practically useful through, the aid of comparative anatomical examinations of similar structures in other animals. It was by examining the structure of the vascular system in the lower animals, and by comparing the phenomena which occurred in them in the living state with similar phenomena in the human body, that Harvey succeeded in demonstrating the circulation of the blood; and it is only by the aid of similar means,—the performing of experiments upon the lower animals, but which necessarily requires a previous knowledge of the structure of the parts in the animals operated upon,—that the functions of many parts in the human body can be fully elucidated. In our own time, the splendid discovery of Sir Charles Bell of the distinct functions of the roots of the spinal nerves was obtained by means of comparative anatomy, being first demonstrated by Bell, Magendie, and others, upon the lower animals; and this important addition to our knowledge, "the second great discovery in physiology," has recently been more fully confirmed by Müller, in his experiments upon the cold-blooded vertebrata. These facts alone are sufficient to show the utility of works such as the one now before us, and we strongly hope that what Mr. Swan is now doing for the comparative anatomy of the nervous system, will be done by him, or some other able anatomist, for that of the other important parts of the body.

We have before noticed the beauty of the engravings in Mr. Swan's work, as admirable specimens of art, but we cannot help observing, that we could have wished that he had executed the drawings for them himself, as a few inaccuracies and blemishes observable in some of the plates, and which detract a little from their value, would then, probably, have been avoided. We are quite certain that anatomical drawings by an anatomist, even when but roughly executed, are much to be preferred, and will always convey a more vivid idea of the object, than those which are made by a professed artist unacquainted with anatomical peculiarities. One of the faults to which we particularly allude in these drawings is the stiffness with which some of the objects are represented. They remind us more of carved wooden structures than of the free and pliable vessels of a living animal body. An instance of this is seen in plate xv., in the delineation of the heart of the turtle, with its vessels, the pulmonary artery and right aorta. Bold and vigorous delineations by an anatomist would have conveyed a much more correct notion of these parts than the finished drawing before us. We cannot better explain our preference, than by requesting our readers to compare with these the graphic delineations of some of the arteries of the human body, in one of the earlier publications of Sir Charles Bell, some of which were both drawn and etched by Sir Charles himself, with a vigour and truth to nature hardly to be surpassed.

With these remarks upon the execution of the plates, we now turn with pleasure to an examination of their contents. Plate xvii. is the one which interests us most. It has been stated by some anatomists that the spinal nerves of the python, and other ophidians, have but one root derived from the spinal cord; but Mr. Swan has clearly shown, in his delineation of a portion of the cord of the boa-constrictor, that two roots certainly do exist, notwithstanding what has been asserted to the contrary with regard to the python. This, indeed, we should have expected would be the fact, if the opinion respecting the difference of function in the two roots of the nerves in the higher animals were correct, as it is now proved to be by Müller's experiments upon the roots of the nerves in frogs. Mr. Swan also notices an equally interesting fact with respect to the cord itself, which, in the boa, is continued to the end of the tail, and "has cineritious matter within it similar to that in mammalia." He also remarks that "each anterior bundle (or root of the spinal nerves) is rather larger than the posterior." (p. 64.) This difference in the size of the roots of the spinal nerves is an important characteristic, inasmuch as it seems to indicate the preponderance of the motor over the sensitive functions, in this class of animals; and hence, from the circumstance that in the human subject and the higher vertebrata the posterior roots are the largest, while the function of sensation appears to be developed to the greatest amount, leads us to infer that the preponderance of either function in the spinal nerves of all animals corresponds with their relative size.

In some of his dissections of birds, which we had looked for with much interest, Mr. Swan has been particularly happy; especially in his tracing of the sympathetic system, to which he seems to have paid most attention. We could have wished that he had confined himself simply to anatomical details, since, in the few instances in which he has ventured to theorize upon the uses of the structures he describes, he has not been so fortunate in his conclusions as in his dissections of the parts themselves. Thus he states (p. 103), that "in the pelican the greatest part of the nerves supplying the upper and lower jaw, and the pharynx or great bag, are covered with a black membrane, which is provided on account of the thinness of this extensive part, and for preventing any privation of the influence of the nerves, and a consequent retardation of the circulation of the blood, from exposure to a low temperature." We certainly do not perceive the conclusiveness of this assumption, since if this be really the use of the membrane, we might fairly expect to find a similar membrane enveloping the nerves in all parts of like thinness, and exposed to an equally low temperature, as in the web of the feet of the pelican, and in that of the swan and other water-birds, but such does not appear to have been found by Mr. Swan, in his dissections of these birds. While, on the other hand, he states (at p. 77), that in the crocodile "the nervous system is almost entirely covered by a black membrane." Again, in his attempts to generalize on the utility of certain parts of the body, he is not more fortunate; as when he states (p. 103), that "parts covered with feathers generally receive spinal nerves, and seldom branches of the fifth; hairs, in the human body and animals, exist in parts supplied by both, but the longer hairs and wool in particular, in those furnished by the spinal; there are exceptions to all these statements; it is, therefore, most probable that the structure of the skin, independently of the nerves, is a principal cause of the difference." Here he attempts to indicate a law with reference to the influence which particular nerves exert over the production of wool, hair, or feathers; forgetting, in the first place, that, according to the views now generally received, the fifth nerves and the spinal nerves are precisely analogous; and next, that the appendages of the skin, whether in the form of wool, hair, or feathers, are only modifications of the same structure, and are dependent, not upon the particular nerves distributed to the part in which they are produced, but upon the skin itself, for their peculiar formation. It is in such anatomical details, then, as are exhibited in Plates XXI, XIV, and XXV, that Mr. Swan appears to most advantage; and in his summary of the differences which exist in the structure of corresponding parts of different animals, as in the following observations on the brain.

> "The brain of birds differs from that of many of the amphibia, in the form and comparatively large size of the hemispheres. In the greater dimensions of the anterior lobes, in proportion to the olfactory nerves; in the very thin parietes of the posterior part of the lateral ventricles; in the shape and magnitude of the striated bodies; in the larger size and different situation of the optic thalami; in the flattened summit, but greater circumference of the optic lobe; and in the shape of the cavity, which corresponds with their external form. In the external shape and appearance of the cerebellum; in its having convolutions; in the thickness of the parietes of its small cavity, which communicates with the other ventricles. In the absence of longitudinal eminences extending towards the calamus scriptorius, and forming the vermicular cord; in the greater thickness of the oblong medulla. It agrees in the presence of the anterior, posterior, and soft commissures; in crura extending to the oblong medulla, and in the similar arrangement and origin of the nerves. It differs from that of many of the mammalia, in the want of convolutions and the corpus callosum, or great commissure. In the great thinness of the posterior parietes of the lateral ventricles; in the radiated form of the septum, and the different construction of the floor of this cavity; in its being continuous with the sides of the ventricles, and not separate, like the fornix. In the hollow optic lobes, instead of the quadrigeminal bodies. It agrees in having small lateral lobes, similar to the lobules attached to the sides of the cerebellum in the monkey and some other animals, and also in their being placed in a hollow, partly surrounded by a semicircular canal, which in man, and some animals, is occupied by part of the petrous portion of bone. It differs in the continuation of the third ventricle into the cerebellum, as well as into the calamus scriptorius; in the want of the annular tubercle, although there is a considerable enlargement of the oblong medulla at this part." (p. 100.)

We are surprised that in this summary Mr. Swan has not noticed the similarity which exists between the brain of birds and marsupial animals, in the absence of the great commissure, as pointed out some time since by Mr. Owen, in the Philosophical Transactions. But he has noticed one peculiarity in the muscular system which we do not remember to have before met with. He states that "in the pinion very distinct little muscles are observed for giving motions to the quills, and thus form a variety of the cutaneous muscle." (p. 98.)

In conclusion, we beg again to recommend Mr. Swan's work to the student in comparative anatomy; at the same time, we hope that the forthcoming numbers will appear a little more quickly, since only four numbers have as yet been published, the first of which appeared in 1835, and the third in 1837, while the fourth has but recently made its appearance.

Art. XII.

Lectures on Diseases of the Eye. By John Morgan, f.l.s., Surgeon to Guy's Hospital and Lecturer on Surgery at that Institution. *Illustrated by coloured Plates.—London,* 1839. 8vo, pp. 221.

It is some four or five years since these lectures were first announced for publication. They only appear now, the author tells us in his preface, at the request of his pupils, and not from any wish on his part to appear before the profession and the public as an author of what will, he fears, be considered by them as a very imperfect work on the subject of ophthalmic surgery generally. This admission, he trusts, will shield him from hypercriticism.

> "Before I proceed to describe to you," says Mr. Morgan, "the symptoms and treatment of the diseases of the eye, I wish you to understand the particular object which I have in view, in delivering these lectures. In the first place, you will recollect that it is not my intention to describe, with the absurd minuteness of a professed oculist, each trifling deviation from the ordinary diseases of this organ, which have been unnecessarily made the subject of a separate description and of a separate name. It is my wish to generalize the subject, to show you, by pointing out the analogy of the diseases of the eye with those of other parts, that ophthalmic surgery and general surgery are one and the same science." (p. 9.)

The proposition that a well-educated surgeon is competent to treat diseases of the eye is often asserted and maintained; but it is obvious that its correctness or incorrectness must depend entirely on the meaning attached to the expression "well-educated surgeon." Undoubtedly we ought to apply the epithet to him only who is as well acquainted with diseases of the eye as of other parts of the body. Do we not meet, however, with many who are generally reputed "well-educated surgeons," whose knowledge of ophthalmic surgery is nevertheless very imperfect? "It is a melancholy truth," says our author, "that amongst the general body of medical practitioners in this country, ophthalmic surgery is too frequently neglected, notwithstanding its universally acknowledged importance." (p. 2.)

We do not suppose that it is because the general pathology of the eye has been (as Mr. Morgan appears to think, from the pains he takes to show "that ophthalmic surgery and general surgery are one and the same science,") considered different from that of the rest of the body, that the diffusion of a knowledge of ophthalmic surgery has not more universally obtained; but we believe the real cause is that the special pathology and the details of the *art* have been neither taught nor studied so generally as they ought to be. The surgeon must view the eye not only as a component part of the body, and subject to all its general diseases; but he must view it under another and an altogether peculiar light; he must view the eye in fact as a living dioptric instrument, the vital constitution of which exerts more or less influence over its purely physical constitution, and contrariwise. Hence, though the eye may be perfect as an optical instrument, yet, on account of some disease of the retina, optic nerve, or brain, the function of vision shall not be performed. On the other hand, as one condition necessary for the perfection of the instrument is the complete transparency of the lenses of which the eye is composed, and the integrity of the pupil, so opacity of one or other lens, or closure of the pupil, by preventing the rays of light from impinging on the retina, though perfectly sound, will oppose an effectual obstacle to vision. All this shows that the diagnosis and treatment of diseases of the eye demand, in some degree, a specific and peculiar consideration. It is not enough for Mr. Morgan to tell us, for instance, that every disease to which the conjunctiva is subjected, is nothing more nor less than the common disease of a mucous membrane. It must also be strongly inculcated that affections of the conjunctiva involve results different from what is found in connexion with affections of other mucous membranes.

There are peculiarities of disease requiring corresponding peculiarities of treatment in every organ more or less, and the more complicated and artificial the structure of the organ the more numerous will those peculiarities be. There may be no difference in the abstract between inflammation of the conjunctiva and of other mucous membranes, but many and important differences in the detail. More than one half of the practice in ophthalmic medicine and surgery is a practice of detail, and thi depends on the nature of the organ. These details are to be learned by experience only, and in fact must constantly be attended to. It is by attention to details that the mere oculist has so often made head agains the regular surgeon. Teach the student practically the details of eye-surgery as you teach him the details of compounding—the details o anatomy—the details of other branches of surgery, and you wil qualify him to treat the diseases of the eye as well as any other.

We are no advocates for unnecessary distinctions and names; but w think that when a specific character can be established in any morbi state, and that character influences, directly or indirectly, the treatment that morbid state should be distinguished not only by a separate description, but also by a name, for the sake of fixing attention on i and avoiding circumlocution. We do not mean to say that no unnecessary descriptions and names have been given by writers on ophthalm surgery, but we think that Mr. M. underrates, and in a too sweepin way, the distinctions which have been made in the morbid states of th eye. "That which in ophthalmology," it has been remarked, and w think justly, "has been decried as unnecessary minuteness, we unfortunately fail to find in the diagnosis of the diseases of other organs."

We maintain that it is a dangerous fallacy to assert in an unqualifie manner that the diseases of the eye do not differ from those of othe organs. The truth is, the eye, like other organs, has its general patholog and its special pathology; and the attempt to instil into the minds o students that there is nothing special, is a true way to favour indolence "Lax and general notions," says Mr. Pearson, in the admirable prefac to his work on Surgery, "floating in the understanding, will be of little advantage until they are reduced to something limited and specific; and except knowledge be in the detail, the application of it in particular instances will be attended with almost insurmountable difficulties. He, therefore, who desires to practise surgery with probity and success, must study it both as a science and as an art; for a man destitute of principles is little better than a surgical automaton, while the man of mere erudition can only be considered as a learned spectator."

Neither our limits nor the amount of original matter in this volume authorize any very detailed notice of it. We hope, however, to return to the subject at another time. But as it is dedicated by a teacher to his pupils, it may perhaps be expected that we should state what we think of the work as a manual for the use of students. It is always an invidious task to pronounce a summary opinion of a work, because there are few which have not some excellencies as well as defects. By noticing the principal examples of each, we act as counsel for and against, leaving the judgment to the reader; but in the case of a work for students, we are in a manner compelled to be not only counsel for and against, but also judge.

Mr. Morgan successively discusses the diseases of the conjunctiva, of the cornea, of the iris, of the sclerotic, and of the retina, cataract, the malignant diseases of the eye, then the different operations for cataract and artificial pupil, and last of all entropeon, ectropeon, and the morbid affections of the lacrymal conduits. "In these, my lectures on *Diseases* of the Eye," says he, "it is not my intention to describe the effects of mechanical violence upon the organ of vision and its appendages, as these, when ophthalmic and general surgery are recognized as one science, must be considered as a subject for the lectures on wounds, contusions, &c. generally, and to enter into a description of the nature of the tumours formed in the lids, and the mode of operating upon them would be needless, as similar tumours are met with in analogous structures in all parts of the body, and described to you in the course of surgical lectures, of which these form a part." (p. 216.)

After what has been said in the foregoing part of this article, we shall not stop to enquire whether these be sufficiently valid reasons for passing over in silence, in a student's manual on the Diseases of the Eye, the injuries of the eye and tumours of the eyelids; but having stated the case, we proceed to pick out one or two specimens, for and against, preparatory to summing up.

The following we think is happily expressed and practically just:

> "In active diseases of the eye, there is one general rule, which you cannot remember too well, it is never to rest contented with merely checking the progress of disease,—never if you think that you can prevent it, by pushing your remedies still further,—never allow a disease of the eye to remain stationary. In other organs, where you have neither the transparency or integrity of a delicate tissue to preserve, nor an expanded sheet of nervous matter to protect from the slightest causes of disease, this may not be a point of any very material consequence. But, in active diseases of the eye, the delay of remedies for twenty-four hours will, in some cases, prove a sufficient cause for complete disorganization of the organ." (p. 8.)

The remarks at pp. 28, 29, on a point in the anatomical history of inflammation being but a speculation, and one by no means warranted by the facts adduced (for these we think are ill observed), might have been dispensed with.

The directions laid down at p. 62, for the treatment of acute purulent ophthalmia, we believe to be the best calculated to remove present danger and prevent subsequent bad results; but the following account of granular conjunctiva, we cannot do otherwise than consider a specimen of crude pathology, based on an imperfect knowledge of the anatomy of the part. Granular conjunctiva, says Mr. Morgan, "is occasioned by chronic and irregular thickening of the membrane, accompanied more or less by the organization of adhesive matter, which previous inflammatory action had poured into the cellular membrane beneath." (p. 103.) There is no account taken here of the papillary body of the palpebral conjunctiva, nor of the circumstance that between the latter and the tarsal cartilages there is no great show of cellular membrane.

At page 76 of the present Number of this Review, there is a note commenting on the vagueness with which measurements in lines are given in English works; of this we have an example in the work before us. Thus, if we were to follow Mr. Morgan's directions, as given in the text, in making the section of the cornea in extraction, we should make the opening too small. Figures 2 and 4 of plate xv. do not tally with the text, and are, on that account, more correct guides. On the other hand, in figures 7 and 8 of plate xv. we have the cataract-needle, and in figure 6 of plate xvii. the iris-knife, represented introduced too close to the cornea, whilst the directions in the text are somewhat more correct.

In regard to the plates, eighteen in number, and containing 74 figures in all, they are in general very illustrative of what they represent; and if the artist had taken a little more pains to accomplish accuracy of drawing and to preserve proportion, the figures would have had a claim to be considered more than mere diagrams, the title by which Mr. Morgan characterizes them.

To sum up: Mr. Morgan's book is of a size large enough to have given a full exposition of the subject of which it treats, had it been drawn up in the concise, aphoristical, and even dogmatical form, which is best for a student's manual; and which, considering the perfection of our knowledge of the eye, the subject admits of. But, unfortunately, although the descriptions and remarks do not differ in the main from what is usually met with in books on the eye, they are often diffuse and indefinite, and sometimes so obscurely expressed, that, in more than one instance, our efforts to comprehend the author's meaning, we confess, were unrewarded with success. And this is rendered worse by the circumstance that, without giving any synonomy, he often makes use of names in a signification different from what is met with in the books mentioned by him as classical. Mr. M. appears to be but little acquainted with the history and literature of his subject.

So much for the mode. As to the matter of the volume, the extracts we have given (and many more of the kind we could give) show that the discussions on the general pathology and therapeutics of the eye present much that is judicious and excellent; but great defects are exhibited whenever the author descends to the consideration of the details of special pathology and therapeutics.

The preceding remarks have been dictated by no spirit of hypercriticism; and we hope that what we have said, in regard to the necessity of attending to details in the study as well as in the practice of the diseases of the eye, will not be construed into any wish, on our part, to perpetuate the "unfortunate and disgraceful separation" between general and ophthalmic surgery. The only way, we believe, to hasten the time "when the distinction between a surgeon and an oculist shall cease to exist," will be to make our young medical men oculists as well as surgeons.

Art. XIII.

Observations on the Oriental Plague and on Quarantines, &c. addressed to the British Association of Science. By John Bowring. *Edinburgh,* 1838. 8vo, pp. 45.

In noticing this pamphlet it is not requisite that we should enter into any discussion of the doctrines of contagion, or of the host of annoyances in the shape of quarantine regulations, lazarettos, inquisition of private documents, &c. to which merchants and travellers in the East are variously subjected in consequence of their reception. Dr. Bowring's statements, although worthy of every credence as far as his own personal observations are concerned, have yet too much the appearance of *ex parte* evidence, and his arguments too much of the character of special pleading, to justify the foundation of any very decided conclusions upon them adverse to existing laws and opinions. It cannot, however, be denied, that a strong case is made out for instituting a close and sifting enquiry: this, to be effective, must be carried on by candid and unbiassed observers in the centre of the plague districts; and it is but justice to the author to state, that to obtain an investigation of this description, is the professed object of this treatise.

Dr. Bowring's observations may be considered, first, in relation to the plague itself, and the quality of the evidence by which the doctrine of its contagious nature is supported; and, secondly, in relation to the efficiency of quarantine regulations in preventing or limiting the progress of the disease.

Considering the loose nature of the testimony upon which popular belief is often founded, it cannot be a matter of surprise that Dr. Bowring should find "much of the evidence floating about in the public mind as to the contagiousness of the plague to be of a very untrustworthy character," or that many "extraordinary absurdities," many "amusing inventions" have been resorted to, to account for the outbreak of the disease where no visible means of its introduction into a secluded locality present themselves. This is in strict accordance with what daily passes under our own inspection in many other diseases, and in some in which the specific contagion is of a far less questionable character than that of the plague. We believe that, were the medical records of small-pox to be investigated with a view to this point, some absurdities quite as extraordinary, some inventions quite as amusing, would be found brought forward to account for the outbreak of this latter disease, as the following instances adduced by the author with respect to the introduction of plague. "A very timid person," says Dr. Bowring, "an alarmed contagionist, who was attacked and died of the plague, had shut himself up in his chamber; it was found that his son had, for his amusement, let up a kite from the roof of the house, and it was supposed that the kite-string had been touched by a bird, which bird was imagined to come from the infected quarter of the city (Alexandria or Cairo, it is not stated which); the plague entered the house down the string of the kite, and the son's father became the victim." "In another case, where the plague penetrated a house kept in the strictest quarantine, a cat had been seen to spring into a basket of clothes returning from the wash-house, and thence to leap into the window of the house in question. It was said the clothes belonged to some family which had probably had the plague; but, at all events, the cat was the only intruder who had violated the cordon, and was therefore the introducer of the disease. In a third instance," continues Dr. Bowring, "an Arab girl had hung a shirt out of a window to dry; the plague attacked the house, and I was told there could be no doubt that somebody in passing the street had touched the shirt, and was thus the cause of the introduction of the malady." But it is useless to multiply examples: it is perfectly obvious that the Arab girl had the means of communication with the street; and with the temptations to violate quarantine, and the natural heedlessness of individuals, especially of the ignorant, it is impossible to say what extent of communication might not have taken place. Instances of this description, as it appears to us, prove nothing, unless we are placed in possession of the fullest details of the precautions adopted, of the nature of the localities, and of the history of the persons inhabiting them.

A much stronger ground of objection to the received opinions of the contagious nature of the plague is afforded by the numerous instances in which, notwithstanding the abandonment of every precaution, both in respect of individuals and families, the disease has not been communicated from infected persons to those around. Several striking examples of this kind, occurring both during the general prevalence of the disorder and at times when only sporadic cases were observed, are related by Dr. Laidlaw of Alexandria, in a letter to Dr Bowring. It is not necessary to quote any of these in this place, but the following observation is too important to be passed over. "I have never seen," says Dr. Laidlaw, "a case of plague occurring sporadically where any person about the patient or in contact with him was attacked; and I cannot find any one that has *seen* one, although it is talked of among the Levantines as a common occurrence." The instances of inoculation with the blood or pus of plague patients, in which no effect followed, we are not disposed to place much reliance upon, as they are not only too few in number to warrant any conclusions being drawn from them, but the results obtained from this practice, upon the whole, were variable and uncertain.

We must refer to the pamphlet itself for the further elucidation of this part of the subject, and more especially for the arguments in favour of the endemic and epidemic character of the disease, derivable from its prevalence and fatality in low or close and confined situations, and from its entire cessation or altered character consequent upon the removal of the inhabitants into more healthy localities, or the scattering of the population over a greater extent of country. We proceed now to give the author's opinions as to the utter inefficiency of quarantine regulations for the attainment of the objects for which they are adopted.

Certainly, if we are to take up with the ultra views of many of the directors of quarantine establishments, the question as to the efficacy, not only of the existing laws in preventing the admission of plague, but also of any which may be hereafter devised, is reduced within very narrow limits. No quarantine, as Dr. Bowring observes, "can prevent the fish from passing up and down the Danube," or the bird from winging its flight through the air. It is not, however, by directing the shafts of ridicule against such absurdities as ignorant and interested individuals will always be found ready, from superstition and self-will or for special purposes, to advance and maintain, that the question can be set at rest. Rational evidence of the actual fact of quarantine inefficiency, as established by experience in places where every precaution has been used, must be brought forward, and the impossibility of providing against the existing defects shown, before the abstract principle of the inutility and consequent injustice of quarantine can be admitted.

One of the leading objections to the efficacy of quarantine regulations is, that wilful or interested persons will always find the means of evading them; and it must be acknowledged that in countries where there is a great extent of frontier to be protected, it seems scarcely possible to keep up a sanatory cordon in such a state of efficiency as to exclude all unauthorized communication. "Can the strictest quarantine," asks the author, "interdict the Arab of the desert from wandering where he will? Will the adventurous Khurd, the migratory Turkôman, the money-seeking Hebrew, the fanatical pilgrim, the potent sheik, be stopped in their peregrinations by the intervention of quarantine? In the universal system of corruption and bribery which exists in the East, can any functionary be depended on for imposing and enforcing a perfect obedience to sanatory regulations, and anything less than this is nugatory?" There is too much reason to fear that this is a just view of public or general quarantine. It is in accordance likewise with what has been observed elsewhere, and in the instance of diseases unquestionably of a highly contagious nature. Dr. Otto of Copenhagen, himself not opposed to quarantine regulations, has made the remark, that the abolition of the quarantine laws in Denmark in regard to smallpox has not caused any spreading of the disease; but, on the contrary, since the temptation to concealment has been thus removed, one cause of its more ready communication from individual to individual is done away with. But, according to Dr. Bowring, seclusion under private restrictive regulations is neither more easy of attainment nor more effectual than the general quarantine. "During the plague of 1835, the harem of the Pacha of Egypt consisted of about 300 persons; notwithstanding the severest cordon, the plague entered, and seven persons died within the cordon. The cordon itself was composed of 500 persons; these were in constant contact with the town where the plague was violently raging, and of these 500 only three died." This and similar instances, at the same time that they show the inefficiency of existing quarantine regulations entirely to exclude the pestilence, could it be shown that no infringement of the quarantine had taken place, would go far to prove that we must seek for other causes actively operating in the production of plague besides contagion.

This, however, by no means establishes the principle, that all sanatory measures should be thrown aside. A disease which originates from malaria, or other miasmata, may yet be capable of propagation by contact or otherwise from person to person, and it still remains a question of vital importance to determine the best mode of limiting the diffusion of such a disease. Whether the plague be considered as a purely contagious disorder, as endemic, and at the same time contagious, or as purely endemic, this question still remains; if the last of these views be correct, there can be no doubt that quarantine in every shape must be not only inefficacious, but fatally injurious in all its operations; if either of the former, in so far as the disease is contagious an effective quarantine must operate to the advantage of those who are secluded from infected persons. Whether a removal of the inhabitants from an infected district, and the scattering of them over a more extended surface in a healthy locality, might not in every case be more effective than any system of seclusion whatever, is a subject for future investigation. Some facts mentioned by Dr. Bowring, as well as others which have come under our own notice, in the malignant cholera and the continued fever of this country, would seem to countenance the affirmative of this question.

However this may be, it is quite clear, if we are to receive Dr. Bowring's statements, that in the article of fomites little or no precaution is taken,—bedding, blankets, clothing, cotton, and others of the most suspected articles, which are known to have been in contact with the persons of those infected with plague, being received on board of ships or not, according to the opinions, caprices, or interested views of those concerned; and, as it would seem, without injurious consequences.

We conclude these observations with quoting an instance of the inefficacy of the system of seclusion, which, but for the fatal termination, would be ludicrous in the extreme. "Among the medical men who were in Egypt, during the great plague of 1834-5, opinions were about equally divided as to the contagiousness or non-contagiousness of the disorder. But it is remarkable that, while only one of the non-contagionists died, several of the contagionists were victims. Those who took the greatest precautions were among the sufferers. M. Lardoni was a remarkable instance. He was the most timid of men; he never visited his patients but on horseback, and his appearance is thus described: "His harness was wholly of unsusceptible materials, his saddle closely covered with oilcloth, his stirrups were braided, and his reins made with filaments of the date tree; he had a huge oilskin cloak in the shape of a sack, which rose above his head and descended beneath his feet; he was always escorted by four servants, one before, one behind, and one at each side, so that no person could approach him. A thousand other ridiculous precautions were adopted by him; they were all in vain; he was attacked, though, for two days after the attack, he declared it was impossible it should be the plague; on the third he announced that it was really the dreaded calamity, and died soon after."

PART SECOND.

Bibliographical Notices.

Art. I.—*An Account of some new Instruments for Tying Polypi of the Uterus, Nose, and Ear, and enlarged Tonsils; with Cases.* By William Beaumont, Surgeon to the Islington Dispensary.—*London,* 1838. 4to, pp. 35.

This work contains, as its title indicates, an account of the author's inventions for facilitating the removal of polypi by ligature. The instruments first constructed by Mr. Beaumont were intended for tying polypi of the ear alone: but the contrivance, with the requisite modifications, has been since applied to the removal of polypi from other organs. With regard to the employment of the ligature, and the preference now generally yielded to it over the knife, Mr. B. justly remarks, that "in uterine polypi, the circumstances attending the former operation are less repugnant to the patient than those attending the operation of excision," as being less formidable, and not requiring exposure: and, as relates to the nose, where practicable, "it is by far the least painful operation of the two." The application of ligature to polypi of the ear is a new operation; and Mr. B. informs us that he has tied five such, in all of which cases, hearing was more or less perfectly restored. The use of the ligature in enlarged tonsils, Mr. Beaumont thinks, "may have the advantage over excision, of safely allowing the extirpation of the whole tonsil;" but recommends, "if inflammation supervenes, that the tonsil be cut off with curved, probe-pointed scissors, at the part where it is indented by the ligature."

We shall insert an account of the instruments Mr. Beaumont has invented, in his own words.

> "The instrument for tying uterine polypi consists, among other parts, of two rami parallel to each other, save that one is slightly curved towards its point, so as to correspond in some measure with the posterior parietes of the vagina, and the more readily to allow the body of a polypus to pass between the rami; which parts of the instrument are temporarily joined together at the handle, the distance between them being capable of increase or diminution according to the size of the polypus to be tied. The curved ramus is solely for the purpose of aiding in the placing of the noose around the pedicle of the polypus, and may be removed from the rest of the instrument and from the vagina, when that is accomplished. The straight ramus, besides assisting in the application of the ligature, is also, with other parts attached to it, the means by which the noose is tightened, and rendered unyielding. This instrument is perhaps rather complex, but it should be borne in mind that it is to accomplish a complex purpose. It is first to carry a noose around the pedicle of a tumour, in a narrow passage; it is then to constrict the pedicle, so far as to strangulate the tumour; and lastly, to jam the running end of the noose in the knot, so as to prevent any elasticity of the pedicle from enlarging the noose." (pp. 9, 10.)

This general description is succeeded by an explanation of the plates (which are very nicely executed) and directions for the use of the instruments; but for these particulars we must refer our readers to the treatise itself. To tie a polypus of the nostril, or meatus externus of the ear, it is requisite that the instrument be on a much smaller scale than that for tying uterine polypi, and a flat probe must be substituted for the finger, in performing the requisite manipulation to place the polypus between the rami of the instrument: and Mr. Beaumont further advises, that "after tying a polypus of the nose or ear, if the noose should not be applied close to the attachment of the growth, that the two ends of the ligature be twisted until the polypus itself should make a turn or two, not so as to be broken off, but that the pedicle, by being twisted, should itself slough off from the part at which it grew." (p. 14.) For tying an enlarged tonsil, one ramus of the instrument is dispensed with, and in its place a three-pronged instrument employed, in the eyes of which the noose is to be confined: but here, again, we must refer to the plate and its accompanying explanation.

The greater bulk of the letter-press is occupied by a detail of cases. The first related is one of large uterine polypus, which appears to have existed, and increased in growth, during two or three pregnancies, causing premature birth of the children, and had even "been felt or seen between the labia for a year and a half before the patient's last confinement!" The constitutional symptoms were in this case urgent. After examination, the body of the tumour was expelled from the vagina, and the pedicle was thus easily brought within reach of the finger. It was found to be about an inch and a half in length, and the size of a man's thumb. The first ligature (of strong platted silk) broke on being tightened, but left a deep groove, on which another noose was successfully applied: the pedicle was then cut through, at its junction to the body of the tumour, with a curved, probe-pointed bistoury. Little or no pain was experienced, but the large jet of blood from the tumour indicated the danger that might have attended the operation of simple excision, performed on the person of a patient already much exhausted and debilitated. A threatening of peritonitis was subdued by mild antiphlogistic measures, and on the sixth day the noose and pedicle were found loose in the vagina: in three weeks the patient was well. Menstruation recurred afterwards at its regular periods. The tumour, which was of fibrous texture, weighed, after maceration, nearly two pounds and a half. A second case, in which the polypus was smaller, was equally successful. The third case was terminated abruptly, by an attack of inflammation of the lungs, which proved fatal. The fourth case was quite successful. Case five is one of nasal polypus: the noose was, in this instance, "passed easily over the tumour, about two inches down the nostril, and there tied:" the ligature parted on the fifth day, the polypus having previously shrunk, and assumed "a brown, slough-like colour." Four cases of polypus of the ear succeed, all of which terminated favorably. The last case related is one of enlarged tonsil, on which the ligature was applied with a slip-knot noose. "The operation occupied but two or three minutes, and for that time gave some pain, which however ceased immediately after, but recommenced in the course of eight or ten hours, and increased by the next morning, when (twenty-four hours after tying the tonsil) I cut it off, with curved, probe-pointed scissors, at the part indented by the noose, so as to remove the ligature as well as the tonsil. The patient was immediately and entirely relieved from pain: he said that he did not feel the incision, and there was no bleeding; the saliva was barely tinged with blood. There followed little or no inflammation, the patient had no recurrence of pain, and in a few days went out of town." Three weeks after the operation, all that remained was a slight hollow in place of the extirpated tonsil.

The success which has crowned Mr. Beaumont's operations is a sufficient testimony of the efficiency of the means he employs; and he therefore justly merits the thanks of the profession for the publicity he has given to his inventions.

Art. II.—*Illustrations of Cutaneous Diseases. A series of Delineations of the Affections of the Skin, in their more interesting and frequent forms; with a Practical Summary of their Symptoms, Diagnosis, and Treatment.* By Robert Willis, m.d. Fasciculi I. II. III. —*London*, 1839. Folio.

It must be admitted that there is no class of diseases in which pictorial illustration is more useful, or even necessary, than in cutaneous affections. And it cannot be denied that after all the publications of this kind that had taken place, a good work for practical reference, at a moderate price, was still a desideratum when Dr. Willis undertook the superintendence of that before us. We are sorry to say, after examining this work, that the desideratum still exists in a great degree. These illustrations are certainly cheap (four folio plates and as many pages of letter-press being to be had for five shillings), and the plan of the work is strictly practical; but we cannot say that either the plates or the text is good. As works of art, indeed, the plates are very inferior, and the colouring is wretched, generally speaking; while the history and treatment given of the different diseases are meager in the extreme. This latter defect, however, is owing to the faulty plan adopted by the author, not to any incapacity on his part to do the subject justice; and for the execution of the plates we suppose he is not responsible.

With this unfavorable judgment of the work we yet recommend it to our younger readers, as calculated to be of service in enabling them to discriminate the puzzling class of diseases of which it treats. It is cheaper than Bateman's work, and the large size of the figures gives it decided advantages over others of much superior execution, but in which the figures are small, and the affections delineated as it were piecemeal. We had hoped that Dr. Carswell, whose unrivalled work on Pathological Anatomy has carried pictorial illustration to its highest pitch of excellence, would have followed up the publication of that work by another on cutaneous diseases. Had this been done, such a publication as the present would never have been attempted; and should it yet be done, it will soon lose all claim to even the temporary and partial favour which we are disposed to claim for it. We think Dr. Willis's reputation as an author will be somewhat lowered by this work.

Art. III.—*Practical and Surgical Anatomy.* By W. J. Erasmus Wilson, Lecturer on Practical and Surgical Anatomy and Physiology. *Illustrated with Fifty Engravings on Wood, by Bagg.—London,* 1838. 8vo, pp. 492.

This is one of that very useful class of works intended to assist the student in the commencement of his application to practical anatomy, and one which the experience of seven years in the dissecting-room, as a teacher of anatomy, has convinced the author that the student requires. His design has been to interest the beginner, generally, in the pursuit of anatomy, and to assist him in acquiring an intimate acquaintance with its details; and, throughout the work, the subject of anatomy is shown in its most interesting relations to surgery.

The business of dissection is defined as being to divide and turn aside the integuments and fasciæ; to free the muscles from their cellular tissue, separating them, so as to display the vessels and nerves which lie between; and tracing these last to their ultimate ramifications. To attain dexterity and confidence in performing surgical operations, it is well observed that every dissection on the dead body should be made with care and precision, the parts divided or sought for being the same in both cases, although under different circumstances. These remarks should be kept in mind by the youngest student. It occasionally happens, in the course of practice, that observations casually made in the early part of every practitioner's study become unexpectedly useful; and this circumstance, which sometimes occurs in the practice of medicine, is much more likely to occur in relation to anatomical and surgical facts, which make the strongest impression when presented to the eye of the student for the first time. Without the careful superintendence of an experienced person, or some work aspiring to be more than a mere nomenclature of parts, much of the time spent in the dissecting-room must be misspent, and serve no other purpose but to give a false appearance of industry, without any of the results of application.

Upon the whole, we look upon this as a very valuable addition to the guide-books of the anatomical student. The greatest pains appear to have been paid to its composition; the surgical notices have had the benefit of Mr. Liston's revisal; the subject of the anatomy of the liver is treated of with the aid of Mr. Kiernan's researches; the description of the anatomy of the testis and thymus gland, and of herniæ, was facilitated by the permitted inspection of Sir Astley Cooper's beautiful preparations; that of the deep perineal fascia, and of the compressor urethræ muscle, by similar aid from Mr. Guthrie's collection; the anatomy of the eye is made with the assistance of Mr. Dalrymple's demonstrations; and in comparative anatomy the author acknowledges the advantage of Dr. Grant's friendly instruction. The work is throughout illustrated with numerous woodcuts, which clear up all the difficulties and obscurities; and yet there seems to have been no disposition in the author to make his publication a picture-book, for not one unnecessary figure is introduced. We strongly recommend Mr. Wilson's book to our younger readers, whether students at hospitals or in the field of public or private practice.

Art. IV.—*Outlines of Human Physiology.* By William Pulteney Alison, m.d. f.r.s.e., Professor of the Institutes of Medicine in the University of Edinburgh, &c. *Third Edition.—Edinburgh,* 1839. 8vo, pp. 457.

We welcome the appearance of a new edition of this valuable work, for two reasons: first, because we think it a pity that a treatise of such acknowledged excellence should be allowed to remain in the rear of the rapidly-advancing march of physiological science; and, second, because it speaks well for the improving taste of the students of this country that a steady demand should exist for a work of so abstract yet at the same time philosophical a character. Dr. Alison's treatise is not one which will assist those who wish to gain a mere smattering of physiology. It embodies all the generalizations which he regards as well established; together with such hypotheses as may assist, if properly employed, in the attainment of truth; but for a mass of individual facts the student must look elsewhere. This plan has its advantages, especially in a work intended principally as a text-book for lectures; and we cannot imagine one better calculated to excite enquiry in the minds of an industrious student, and to induce him to seek for information from other and more scattered sources, whilst, at the same time, it exercises his discrimination in applying the laws here laid down to the facts which he meets with in his researches. But we are afraid that the book will never find favour with those who need to be *attracted* to the study of a science which appears to us to be unsurpassed in interest. It contains no showy novelties, nor brilliant speculations. It affords, however, a most solid foundation for advantageous study; not only in the scientific value of its contents, but in the philosophical spirit which pervades it, and which impresses itself upon its readers.

In the present edition the Supplement, which was published by Dr. A. about three years since, is incorporated with the text. A most valuable addition which is thus made, is the chapter on the general laws and conditions of vital action, with which the treatise now commences. In this, the author states himself to have endeavoured " to define, with as much precision as the present state of our knowledge permits, the nature of those vital properties or powers to which we must refer as the agents immediately concerned in producing the phenomena of life...... Using these terms only as general expressions for the unknown causes of phenomena, which have been studied, classified, and generalized, and which are peculiar to the living state, I confidently maintain that the reference to such ultimate facts or general principles in physiology is perfectly in accordance with the rules of true philosophy." The only danger in such a course seems to be that we may be too easily satisfied with referring to such " ultimate facts" for an easy explanation of phenomena, which the extending grasp of the physical sciences may be hereafter found to include. There is a large class of phenomena to which the term *vital* can only be provisionally applied; whilst there are unquestionably others completely beyond the pale of dynamics and chemistry, on which the science of vitality, expressing its peculiar powers and actions, must be based. We are not yet in a state to separate these with certainty, and a premature attempt will be more injurious than beneficial.

The results of many recent physiological enquiries will be found embodied in this volume, which is, in almost every respect, *au courant* with the present state of the science. To the student we would particularly mention the subjects of muscular contraction, and of nutrition and secretion, considered in reference to nervous influence; and also that of the capillary circulation, as deserving his careful attention in reading this volume. Dr. Alison's views on these points, though opposed to those of most other physiologists, appear to us equally profound and well supported; and we hope to see them gain ground amongst the rising generation, though opposed to some antiquated prejudices. We cannot, then, do more than strongly recommend his work to all industrious and enquiring students, as well as to those who are desirous of adapting their knowledge of *principles* to the advanced state of the science; the only mode of keeping pace with it open to those who are actively engaged in the practical duties of their profession.

ART. V.—*Over de Overeenkomst en het Verschil Tusschen de Jicht en de Scrophulosis, vooral met Betrekking tot de Longetering; eene Voorlezing door* A. A. SEBASTIAN, M.D., Hoogleeraar in de Geneeskunde aan de Hoogeschool te Groningen.—*Groningen,* 1838.

On the Relations and Differences between Gout and Scrofula, with reference especially to Consumption. By A. A. SEBASTIAN, Professor of the Medical Faculty of the University of Groningen.—*Groningen.* 1838. 8vo, pp. 102.

THE chief object of this short treatise is to prove that gout and scrofula are in many points very closely allied to each other, or rather, perhaps, to show that they are mere modifications of the same diseased state. They both depend, according to our author, upon imperfect assimilation of the food, and the one occasionally passes in an imperceptible manner into the other, so that a subject who has been affected with scrofula in early life is extremely apt to suffer from gout in old age. The symptoms of the two diseases are to a certain extent alike; in both, the same tissues and organs are liable to be attacked, as the eye, the skin, the joints, the serous membranes, &c.; and both occur most frequently in the same localities.

Consumption depends upon internal scrofula, and consequently is also allied to gout. Tubercles and gouty concretions are composed alike of animal and earthy substances; and the former are occasionally found to contain uric acid as well as the latter. Thus, according to Sebastian, scrofula, gout, and consumption, are mere modifications of each other. A scrofulous patient, under favorable circumstances, will suffer from gout only, when in less favorable circumstances he would have fallen a victim to phthisis. In the children of gouty subjects the hereditary taint is often changed from that of gout to that of phthisis; and the latter disease makes its appearance accordingly.

Such are the leading features of the treatise before us. It is a statement of general doctrines not sufficiently supported by evidence; but we think the author's intention is more to direct notice to a subject of considerable practical importance than to make any important additions to our previous knowledge of the diseases in question.

Art. VI.—*Vegetable Organography; or an Analytical Description of the Organs of Plants.* By M. A. P. De Candolle. Translated by Boughton Kingdon. *With Plates.* Parts I.-IV.—*London*, 1839.

The celebrated work of which this is a translation is known to all botanists throughout the world as one of the most classical productions that have illustrated and advanced scientific botany. To every British botanist who has not access to the original work, the present publication will be a great boon, and we think English literature much indebted to Mr. Kingdon for the pains he has taken to make so valuable an addition to its stores. The translation seems accurate and neat; the plates are very good; and the whole publication is no less elegant than useful.

Art. VII.—*Diet and Regimen, Physical, Intellectual, and Moral, as Means in the Prevention and Cure of Disease.* By Robert Dick, m.d.—*Glasgow*, 1838. 8vo, pp. 386.

So many works on the subject which Dr. Dick has chosen for publishing a volume upon have appeared within the last few years, that it is difficult to conceive an author's inducement for appearing in the same course, unless he is conscious of possessing knowledge which has not hitherto been communicated to the public, or of powers of language more persuasive or convincing than that employed by his predecessors. Dr. Dick scarcely lays claim to either; and he sets out with what we pronounce to be a very erroneous notion, contained in the first sentence of his preface, that "It is *not* to be expected that on topics which are treated of in this volume, a work entirely original should be submitted to the public." It would be useless to dwell on Dr. Dick's frequent and somewhat curious appeals to his Glasgow townspeople; in which, at least, there is much originality. Perhaps he considered that they would read a book from him with more pleasure than one from an author unknown to them, and we hope he has found it so; but we must maintain that, as so many good and useful works are now to be found in every bookseller's shop, on the subject of Diet and Regimen, an author on these matters, if he cannot be original, would wisely keep his manuscript in his portfolio. Yet, probably, every work of this kind does good to somebody; and Dr. Dick's book is lively and pleasant enough, and may be read without any intellectual fatigue.

The portion of his work which he appears to consider peculiarly his own, that is to say, not taken from previous English or foreign and untranslated writers, nor even from Dr. Copland who has assisted Dr. Dick and seems to assist everybody with his multifarious knowledge, is that in which he treats of the mental and moral faculties, in three chapters: one on Our Intellectual Nature and its Regulation; one on Our Moral Nature and its Regulation; and one on "Love, Madness, Music;" which last might be a fragment of an unfinished novel. In the first of these chapters, the passages which have given us the most pleasure are those in which he recommends particular courses of reading for patients labouring under the influence of different passions. For the rest, many interesting subjects are touched upon; but the remarks are not such as to retain the attention or convey any novel instruction.

ART. VIII.—*A System of Anatomy for the Use of Students of Medicine.* By CASPAR WESTON, M.D. *With Notes and Additions*, by W. E. HORNER, M.D. *Seventh Edition, entirely remodelled and illustrated by numerous Engravings*, by J. PANCOAST, M.D. &c. Two Vols.—*Philadelphia*, 1839. 8vo, pp. 491, 560.

THE numerous editions through which this work has run since its first publication in 1814 sufficiently attest the high value placed upon it in America. Although decidedly a work of merit, and well suited to elementary instruction, it is not calculated to supersede the many valuable manuals already in the hands of our students. The woodcuts are very indifferent, and the original plates commonplace and rather old-fashioned: the best illustrations are those copied from the elegant drawings of Sir Charles Bell.

ART. IX.—*The Surgical Anatomy of the Perineum.* By THOMAS MORTON, formerly one of the House-Surgeons of the University College Hospital. *Illustrated with Lithographic Plates and Wood Engravings.*—*London*, 1838. 8vo, pp. 80.

IT may seem ill-natured on our part to find fault with a treatise on which a great deal of care has evidently been bestowed; but as the present is another endeavour to make anatomical knowledge easy, we consider it obnoxious to the objections formerly made by us in noticing works of a similar class: we cannot but have our apprehensions concerning any means resorted to for facilitating the acquirement of anatomy at the expense of actual dissection. We are well aware that Mr. Morton only intends his work to be employed as an aid to the student in pursuing his dissections; but our experience has taught us to place but little reliance on the practical disposition of the idly-disposed, where the temptation of *getting-up* their anatomy from plates is thrown in their way.

Mr. Morton is true to the promise of his title-page, in giving the surgical anatomy of the perineum, and indeed, in great measure, of the pelvis likewise. He seems to have freely consulted the most recent French authorities; and we could wish that he had extended his enquiries to the German anatomists: Müller, in particular, has been occupying himself in minute dissections, both human and comparative, of the male generative organs; and his results, as connected with the muscular structure of the urethra, the arterial distribution in the corpora cavernosa, and the nervous plexus in these parts, present much that is interesting and important. The lithographic plates with which Mr. Morton's work is illustrated are nicely coloured, and, though rather stiff, are on the whole accurate. If then, the student will agree to receive it on our terms, viz. that he shall not abate one stroke of his knife because he has such assistance presented to him, we most cordially recommend Mr. Morton's treatise, as a satisfactory guide in the dissection of the perineum and pelvis.

Art. X.—*The Physiology or Mechanism of Blushing; illustrative of the Influence of mental Emotion on the capillary Circulation; with a general view of the Sympathies, and the organic relations of those Structures with which they seem to be connected.* By Thomas H. Burgess, m.d. &c.—*London*, 1839. 8vo, pp. 202.

We opine that our Review does not find many readers among the fair sex; its character is too profound for the Lady Bountiful who smatters in medical subjects; and it will scarcely furnish much light reading to those who live on periodical literature. To whom else to recommend a work on the Physiology of Blushing, would have sorely perplexed us, if we had not found upon examination that the title but faintly shadows forth the character and scope of its contents. To be sure, the first sentence of Chapter I. is in the true young-lady style of unmeaning sentimentality —"Blushing may be styled the poetry of the soul!" But our author mends upon acquaintance; and out of his two hundred pages there are really some (though, by the way, *not* upon his chosen subject,) which contain better matter than this promises. As our readers will probably feel some curiosity to know how he treats of so novel a subject, we shall present them with a sketch of the table of contents, which want of room only prevents our transferring to our pages, as proving how admirable a specimen the book is of that interesting genus of literary productions which treat de omnibus rebus, &c.

The first section is devoted to the *Natural History* of Blushing, of which the "*Poetry*" of this phenomenon, with "Virgil's imagery," and "Homer's description of 'Pale Fear,'" constitutes the first chapter. The Sensibility of Plants, that of the lower Animals and of Man, are then discussed. The motions of the former are considered as being either the "effect of the vital principle in a state of concentrated action," or as depending on the "germ of true sensation;" to which last opinion the author inclines. Sensibility in man is defined to be "that state of feeling which draws a distinction between right and wrong!" Then follows a chapter on Blushing as an Evidence of Design; and next a disquisition on the varieties of the human race. In Chapter V., the varieties of the Blush are classified, as into the True Blush, the Blush of Feeling, &c.; and in the last chapter of the section, the important question is considered whether the idiot, the lunatic, or the intoxicated man can blush; and this is decided in the negative. Section ii. is concerned about the *Anatomy* of Blushing; and here we have one chapter on the Nervous System, another on the Capillary Vessels, and a third upon the Skin, in which the structure and functions of these parts are systematically treated of. In the third Section is discussed the *Mechanism* of Blushing. In this part of the work, after the "reflected movements of sympathy," the "Epileptic Aura," the identity between electricity and nervous influence, and a few other equally disconnected subjects have been dismissed, the author proceeds to establish his peculiar (?) doctrine,—namely, that the "Blush-exciting impulse" is conveyed from the brain to the stomach and solar plexus, and along the ganglionic nerves to the capillaries,— which we believe to be the doctrine usually entertained by physiologists at the present time, (except as to the necessity of the transmission of the impulse to the solar plexus, which we take leave to doubt,) and which it scarcely required two hundred octavo pages to establish.

This abridgment but slightly justifies what we have said of the discursive character of the book; however, if our readers are inclined to judge for themselves, we venture to predict that they will derive some amusement if not much instruction from the treatise. The conclusion, however, is too rich to be overlooked. What will the following subjects think of their fellowship?—"Moral and Physical Treatment of the Mental Emotions arising from Diseased Sensibility—Early Moral Training and its advantages—Physical Treatment—The salutary influence which gymnastic exercises exert on the Intellectual and Moral powers—Pernicious effects of Boarding-school Physical Education—Gymnastics recommended by Herodicus, Hippocrates, Galen, Celsus, Sanctorius, Van Swieten, *Macartney*—Cultivation of Gymnastic Education in Paris—Gymnasium of MM. Pravaz and Jules Guerin."

One word to the author of the work we have been so freely criticising. Let him choose for his next attempt a better subject; let him cultivate precision of language, and closeness of thought; and discard or sparingly use the flowery ornaments which garnish his present composition. Then if the desire of authorship should still be strong within him, he may stand a chance of attaining at least a respectable station as a writer, and may please others besides the superficial and trivial-minded, to whom alone, we fear, his present work will recommend him.

ART. XI.—*Elements of Medical Jurisprudence.* By T. R. BECK, M.D., and J. B. BECK, M.D. *Sixth Edition.* In Two Vols.—*Philadelphia,* 1838. 8vo, pp. 670, 743.

THIS work has been so long before the profession, and has justly acquired so high a character, in Europe as well as in America, that it may seem superfluous to notice it. We are, however, desirous of calling the attention of our younger readers to the best and most complete treatise which is anywhere to be found on this important department of medical science; and we are, at the same time, glad of the opportunity of offering our tribute of respect to the learned and estimable physicians who are its authors. When Mr. Taylor's "Elements" is completed, and we earnestly desire to see the publication of the second volume, the English student will have a work, on a smaller scale, and on a somewhat better plan, which will meet all his wants; but, in the meantime, we earnestly recommend the treatise before us, the present edition of which (the sixth) contains considerable amendments, and many important additions, particularly in the part relating to Persons Found Dead.

PART THIRD.

Selections from the British and Foreign Journals.

I. THE FOREIGN JOURNALS.

ANATOMY AND PHYSIOLOGY.

On the Disposition of the Blood-vessels in the Skin of Smallpox Patients.
By Professor SEBASTIAN, of Groningen.

ERUPTIONS of the skin, properly so called, have their seat in the dermis, and are covered by a healthy transparent epidermis. Such are the various exanthemata and other pustular, vesicular, and papular eruptions.

In the short paper before us Dr. Sebastian's object is to investigate the cause why, in such eruptions, the epidermis is separated from the dermis only in certain circumscribed portions, to form pustules or vesicles, and to point out the abnormal changes which take place in the dermis. With this view he injected portions of the skin of patients who had died during the height of smallpox, with a penetrating injection, and submitted them to examination after being deprived of the epidermis, and otherwise prepared, generally by being dried, and saturated with turpentine.

He found the sites of the individual 'pocks highly injected, and the vessels forming a fine vascular network, corresponding in extent with the area of the pock, and separated from the skin on which no pocks had existed by two larger vessels, which encircled the site of the pock, and which, by giving off branches running towards a common centre, formed the vascular network. This vascular expansion may also be formed by branches derived from one vessel only, and the merging of several smaller into one larger expansion is the cause of confluent smallpox.

The result of this examination Dr. Sebastian considers as tending to remove the difficulties of the question. In consequence of the cause which produces smallpox a greater flow of blood takes place towards the skin, which by and by becomes inflamed, and the inflammation concentrates itself in certain circumscribed portions; these in a short time begin to secrete a serous fluid, which, as the inflammation increases, becomes purulent. The nature of the eruption depends, therefore, upon the vascular state of the dermis, but the cause which produces this peculiar vascular condition is as obscure as ever. Professor Sebastian leaves the question undecided whether the vascular network is a new formation, or whether it is the result of the greater injection of vessels already existing.

Nat. Tijds. i., 2. 1838.

Observations regarding the Reproduction of the Lens.
By Dr. PAULI, of Landau.

DR. PAULI, being of opinion that the experiments made as to the regeneration of the lens had yielded contradictory results, because animals of too small a size had been chosen for the operation, and extraction, the only mode of operating here admissible, had not been performed with sufficient care and circumspection, resolved to try what could be done by taking the larger domestic animals for the subject of his experiments. He accordingly extracted the lenses in an old hound

(Jagdhund) and in a bull. The former was killed 163 days, and the latter 211 days after the operations. "On examination I found," says Dr. Pauli, "in the right eye of the hound no trace either of capsule or lens. Behind the lower margin of the uvea there appeared to be some crystalline flakes; but I will not assert this positively. In the left eye there was in the situation of the lens a body similar to that which had been extracted, but softer and smaller; it was transparent. At the incision in the capsule the new lens was more flattened; the edges of the incision in the capsule appeared somewhat drawn into the new substance, and more firmly connected with it than in the rest of the circumference of the capsule. In both eyes of the bull I found nearly the same changes. The size of the new lens was not, however, much more than the half of that which had been extracted, but it was firmer and denser than that in the left eye of the hound—a circumstance which might be owing to the hound having been killed about two months sooner than the bull. The edges of the incision in the interior wall of the capsule were, as in the hound, apparently inclined towards the new lens, and less easily separable from it than at other places. Both animals retained vision until their death."

Monatschrift für Medicin, &c. Jan. and Feb. 1839.

On the Episternal Bones. By G. Breschet, Professor of Anatomy at the School of Medicine, Paris.

M. Breschet has discovered two new bones, which form part of the sternum, and are situated upon its superior and lateral part. In the young subject they are attached to the sternum by a synovial membrane and a fibrous capsule. They have somewhat the appearance of the pisiform bones, but of a greater size. In the adult these two episternal bones are anchylosed with the sternum, but the line of separation is distinct. These bones are not regularly rounded, but are flattened where they look towards the sternum.

Breschet considers these bones to be analogous to the episternal bone of the ornithorrynchus, ant-eater, and armadillo. The latter bone was described by Meckel, Rudolphi, and others: it articulates with the scapula. The episternal bone of the human subject is considered by M. Breschet to be the rudiments of the cartilage of the thirteenth rib, which is not unfrequently met with. It does not properly belong to the sternum, and is formed independently of it, but is soon anchylosed with it, as is the cartilage of the first rib. This episternal bone in the *Bradypus Tridactylus* is continuous with the sternum anteriorly, and posteriorly with the costal element of the seventh cervical vertebra; in this manner an additional rib is formed, and that at the expense of the last cervical vertebra, which has been generally considered as appertaining to the dorsal region.

Annales des Sciences Naturelles. August, 1838.

Case of Spontaneous Rupture of the Spleen. By Dr. Nückel, of Cöln.

Mr. J., æt. 25, had suffered for a fortnight from diarrhœa, but did not seem very ill, being able to get about. He took lead and opium, in small doses, without benefit. For two days before his death he kept his bed, on account of pains in the abdomen; on the last, he was suddenly seized with a feeling of anguish (Angstgefühl), and with cold sweats, &c.; he expired after a few hours. On examining the body, forty-eight hours after death, there was found a great effusion of blood in the abdomen and pelvis, which was traced to an angular rent in the spleen, three or four lines broad, and situated on the lower part of the anterior and outer surface of the viscus. The spleen measured about five inches in length and four in breadth. Its surface was of a dark, livid colour. Its coat was so rotten that the fingers pierced it in handling it; the parenchymatous substance resembled a dark red paste. The great vessels in the abdomen were sound. The stomach and upper portion of the duodenum were normal, but the ileum was covered with numerous ulcers. *Medicinische Zeitung. May* 8, 1839.

MEDICINE.

On the Communication of Phthisis Pulmonalis to Domestic Animals.

By Dr. Malin, of Lübbenau.

[We give the following narrative as we find it: without doubting the statement, we refuse to admit the inference.]

A man, fifty-eight years old, labouring for several years under a pulmonary consumption, had a house-dog which licked up, with great eagerness, his purulent sputa. In half a year the dog became affected with a cough and expectoration of matter, and at last died completely emaciated. Another, a wolf-dog, one year old and one foot high, was procured. This likewise commenced to lick the nauseous mess, although it got milk and flesh. In the course of half a year it also sickened and died within twenty weeks.

To convince himself, Dr. Malin opened the thorax. Both lungs were almost completely destroyed by suppuration. In the right there was a large closed vomica. *Wochenschrift für ges. Heilkunde. April* 6, 1839.

Remarks on the uncertainty of the diagnostic signs of Albuminuria.

By A. Toulmouche, m.d. &c.

[M. Toulmouche relates two cases, observed by himself at the infirmary of the prison at Rennes, for the purpose of showing, on the one hand, that the urine may be strongly albuminous, and other alleged symptoms of Bright's disease be present during life, and yet no morbid change be discovered in the kidneys after death; and, on the other, that the renal lesion may exist without producing any of the symptoms presumed to be its necessary consequences. The case adduced in support of the latter position, is extremely defective in its details, and proves nothing but that M. T. occasionally contents himself with a very slovenly examination of his patients. The other is decently reported, though the state of the kidneys should have been more fully described, and adds to the list of facts of similar character already accumulated: we subjoin the more essential particulars of this case.]

Dano, æt. forty-three, admitted June 26, 1837, with diarrhœa and gastralgia; the former of which continued, in spite of treatment, during a part of the month of July. *July* 12.—The patient began to cough, and œdema of the inferior extremities to appear; she had no fever, but complained of pain in the loins and limbs, which were presumed to be rheumatismal, and treated by the vapour-bath. During the month of August, the œdema sometimes increased and sometimes appeared to diminish with the lumbar pain. *Sept.* 9.—Pain in the side, treated as pleurodynic: on the 20th, the heart was ausculted, but no abnormal condition discovered. *Oct.* 20.—The habitual pain in the loins, and progressively increasing œdema, as well as the deposition of a thick stratum of albumen from the urine, when treated by heat and nitric acid, (on the 23d it is noted as being three or four lines thick,) led to the diagnosis of Bright's disease. The medicines hitherto employed had been diuretics, tonics, anodynes, &c.; dry cupping in the loins and the use of the vapour-bath were now prescribed. The patient was unable to bear the latter more than three times; and on the 18th *Nov.* the œdema having meanwhile reached the abdomen and fore-arms, she expired in a state of tranquil coma. *Inspection twenty-two hours after death. Brain.*—An effusion of blood about the size of a large hemp-seed in the centre, nearly of the right hemisphere, the cerebral substance around it tinged yellow, some limpid serosity between the arachnoid and pia mater. *Thorax.*—Two pints of bloody serum in the left pleura, pseudo-membranes; cicatrices of caverns in the summits of both lungs, which are œdematous, miliary tubercles, bronchial membrane red; heart small, its tissue pale, polypiform concretion filling right ventricle. *Abdomen.*—Two pints and a half of mily serosity in peritoneum, firm, dense, membranous adhesion of a portion of the ilium to the bladder; ulcerations through the entire tract of the small intestine, most numerous in the neighbourhood of the cæcum, the fundus of some of these ulcerations is studded with miliary tubercles; vast ulceration in the ascending colon; spleen small, shrivelled, and firm; liver of ordinary size and healthy, numerous calculi in the gall-bladder; kidneys of very small size and perfectly healthy, both substances pale; bladder contains a good deal of limpid urine, its coats thin and healthy. *Gazette Médicale. Fevrier*, 1839.

On Chlorosis accompanied by Menorrhagia. By M. Trousseau.

The coincidence of diminished or suppressed menses with chlorosis is so common, that the fact is frequently overlooked that a chlorotic state may exist in conjunction with menorrhagia; in both we have extreme paleness, discoloration of the blood, a tendency to dilatation of the heart, *bruit de soufflet* in the principal arteries, and neuralgia in different regions; when the menstrual flow is deficient, these symptoms are said to arise from chlorosis, when it is superabundant, from anemia. We frequently find that chlorosis may be traced in the first instance to a copious nose-bleeding or to any other unusual loss of blood, arising accidentally or produced therapeutically; so that a transitory anemia will give rise to permanent discoloration and increased liquidity of the blood. This altered state of the circulating fluid is itself sufficient to give rise to hemorrhage, which, thus occurring alternately as cause and effect, does not allow the patient to escape from its evil consequences. This form of chlorosis is however much less frequent than that which is attended by amenorrhœa; it forms only one fourth of the cases occurring in adults, and perhaps one twelfth in those of young girls. In twelve cases collected by M. Trousseau, there was no serious lesion of the uterus. It would appear at first view that the treatment adopted for amenorrhœal chlorosis would not be suitable for the menorrhagic form; thus, iron, which is given with so much success to restore the menstrual secretion, would hardly appear appropriate when that secretion is in excess: but is not the effect of iron on chlorotic patients rather tonic than emmenagogue? We generally find the health partially reestablished before the menses return: the complexion regains its natural tint, the depraved appetite, the pain at stomach, the palpitation of the heart cease, and the *bruit de soufflet* in the arteries is lost, so that the patient frequently recovers the appearance of health before the menses reappear. Presently this secretion is reestablished, in consequence of the general return of the system to a healthy state. Iron then is only emmenagogue because it is tonic or reconstituent. Reestablished health is not owing to the returning menses, but the contrary. On this view we shall have no difficulty in conceiving that preparations of iron will be of the greatest utility in menorrhagic chlorosis, in which it will operate both as tonic and hemostatic. Giving freely, the preparations of iron between two menstrual periods, we shall find the blood rapidly regain its lost constituents of colouring matter and fibrine, and the secretion will be much less abundant but more coloured. If more powerful anti-hemorrhagic means are required, the ergot of rye will be very useful. Uterine hemorrhages are generally more violent in the night than during the day, and they occur with the greatest violence about four or five o'clock A.M. Without pretending to account for this singularity, we shall find it highly useful to give a dose of *recently-powdered* ergot (gr. xv. to ℈j.) in the evening, and again about four in the morning. In many cases, the more simple and agreeable administration of acids will be sufficient; of these, the best is the citric, given in its natural form of lemon-juice. If these means are necessary, the chalybeate must be suspended during the menstruation, and afterwards continued whilst any symptoms of chlorosis remain.

Journal des Connaissances Medico-Chirurgicales. Dec. 1838.

On Ligature of the Limbs, as a means of shortening the duration of the paroxysm of Intermittent Fever. By DRS. PENBECK and GOEDECHEN.

[THE following cases contain no novelty; but it is well occasionally to recall the attention to important facts in pathological and practical medicine.]

CASE I. A man, aged fifty, of robust constitution, had been suffering for more than three months from tertian fever, which had resisted all rational means of treatment. Dr. Penbeck, therefore, advised the patient to try the application of the ligature. As the sensation of cold had hitherto commenced in the feet, and had from thence spread over the rest of the body, the ligature, as soon as the first symptom of the cold fit was perceived, was applied tightly immediately above the knee; the cold was, in consequence, soon dissipated, but some heat followed with rather profuse perspiration. The patient continued to apply the ligature immediately on the slightest feeling of cold, at the usual period for the return of the paroxysm, and succeeded in arresting the further development of the cold fit. Heat and perspiration, however, still continued; but these symptoms were removed by quinine, which had formerly been given without relief.

CASE II. A woman, aged fifty-two, was attacked with intermittent fever, and refused to take any medicine for its removal. Recourse was, therefore, had to the ligature as the only means which promised to be beneficial. The first application had scarcely any effect; the second shortened the cold fit and diminished the heat and perspiration. By each succeeding application the paroxysm became less severe, and disappeared entirely with the sixth, and in fourteen days the patient was in a condition to undertake a fatiguing journey.

CASE III. A sailor, aged thirty-seven, of weak constitution, was attacked in February, 1834, with septan fever, complicated with gastric affection. A treatment, principally directed against the latter, removed the fever, and the patient was dismissed the hospital. He was again received, on the 4th of April, with tertian fever; the cold fit lasted about one hour and a half, and was extremely severe; the hot and sweating stages, which were moderate in intensity, lasted respectively half an hour and a quarter of an hour. After some preliminary treatment, to remove some gastric symptoms, the ligature was applied, at the approach of the paroxysm, to the extremities, with the effect of reducing the cold stage to one hour's duration, and of changing the fever to its original septan type. Each succeeding application of the ligature reduced the intensity of the paroxysm, and in three weeks the patient was dismissed cured.

Zeitschrift für die gesammte Medicin. Band 9, Heft 1.

On the Mechanical Action of Tartarized Antimony. By G. POLLI.

ACCORDING to Giacomini the effects produced by the application of tartar-emetic to the skin do not in the least depend on its dynamic action, but on the mechanical action of its minute crystals. Hence the effect of emetic frictions on the economy must be quite different from that produced by the same salt taken internally. In proof of his opinion, Giacomini adduces the sharp angular form of the crystals and alleges that their action is increased by the addition of powdered sugar, that the same result will be produced by any other crystallized salt or even by glass, and that the aqueous solution applied in local baths causes no irritation. But, on the other hand, considering that the cutaneous efflorescence, produced by tartar emetic, invariably presents the same characters, that a similar eruption is frequently developed by sympathy in parts far distant from the seat of friction, such as the scrotum, neighbourhood of the anus, &c., and that pustulation follows when the ointment is simply spread on the skin, M. Polli inclined to the old opinion of its acting dynamically. To settle the question, he and several of his friends made a number of experiments of which the principal results were as follows: 1st. Tartar-emetic friction always produced a papular eruption, with tendency to pass into the pustular form, and in three, a similar affection or pruritus was observed at the genitals or anus. 2d. The eruption never appeared before the thirty-sixth or after the forty-eighth hour after friction. 3d. Simple friction, with cloths dipped in water, when performed where the sebaceous glands are prominent, produced, in about half an hour, an eruption of rosy papulæ, without any tendency to become pustular or to suppurate; where the skin was smooth erythema only followed. 4th. The repeated application of local baths, made with a saturated aqueous solution of tartar-emetic, never produced any eruption. 5th. Ointments made with the same proportion of sulphate of potass, or glass, sometimes produced a slight papular eruption about two days after the experiment, but in the majority of cases had no such effect, and in no instance where the skin was smooth. The eruption was always proportional to the violence of friction.

Giornale delle Scienze Med.-chir. No. XIV.

Cure of a Stubborn Case of Aphonia by means of Ammoniacal Vapours.

By Dr. Gerner.

A young lady was affected, in consequence of a cold, with complete loss of voice, which had already existed three months, notwithstanding all the remedies which were tried. Dr. Gerner, supposing the cause of the affection to be a relaxed state of the mucous membrane of the trachea, at last cured the patient completely in three days by the inhalation of ammoniacal vapours, disengaged from a mixture of a solution of muriate of ammonia and carbonate of potass.

Zeitschrift für die gesammte Medicin, &c. *Feb.* 1839.

On the Use of Dr. Blaud's Pills in Chlorotic Affections. By M. Adome.

A medicine very much resembling the pil. ferri comp. of the London Pharmacopœia has acquired great celebrity in the south of France on account of the cures it has effected in cases of chlorosis. It bears the name of its inventor, Dr. Blaud, who is Senior Physician to the Hospital of Beaucaire. The formula which he gives is as follows:

Sulphate of Iron, half an ounce.
Subcarbonate of Potash, half an ounce.

Mix with *mucilage*, and triturate the mass to a proper consistence, and divide into *forty-eight* pills.

The objections to this formula are that the pills are excessively large; that they have a very repulsive odour; and, particularly, that a chemical change quickly takes place in the mass, the carbonate of the protoxyde being, after a short time, converted into the sesquioxyde of iron. Dr. Blaud, however, maintains that, whatever chemical changes occur, his pill is of equal advantage medicinally, and corroborates this statement by a long list of cases, in which a cure was obtained, generally, in three or four weeks. M. Adome, in a memoir presented to the Academy of Medicine, proposes to remedy the defects above mentioned by incorporating with the pills a portion of *sugar* and of *pulv. altheæ*. As soon as the pills are made, he rolls them on a plate moistened with a mixture of mucilage and sugar, and then covers them with a fine powder, composed of sugar and gum-arabic, aromatized with some essential oil, to correct the disagreeable odour. This process is repeated a second time, and the pills are then, in a great measure, protected against the influence of the oxygen of the atmosphere.

Revue Médicale et Bulletin de l'Académie. *Dec.* 1838.

SURGERY.

On Varicocele, and especially on the radical Cure of that Affection.

By H. Landouzy.

Sixty persons out of every hundred are affected with varicocele. Hence the necessity of studying this disease. The term varicocele, as usually employed, includes the two terms varicocele and circocele, the first of which implies an abnormal enlargement of the veins of the scrotum; the last, of those of the spermatic cord, testicle, and epididymis. Varicocele never occurs without circocele, and, in fact, always forms a consequence of it. The term varicocele is employed in preference to that of circocele, and is understood to mean a dilatation of the veins of the scrotum and cord. The age at which it most frequently begins is from ten to thirty. Of forty-five cases, ten of which are reported by others, and thirty-five occurred in the practice of Landouzy himself,

13 were individuals between 9 and 15 years of age,
29 . . . 15 . 25
3 . . . 25 . 35.

The anatomical conditions which dispose to the frequent occurrence of varicocele are the depending position and great length of the spermatic veins; the weakness of their parietes; the absence of valves; and, especially, the changes in respect to volume, which they are constantly undergoing. We may add to these, the pressure of a column of blood reaching from the second dorsal vertebra to the testicle, and occasional impediments offered by the inguinal canal. The disease is more frequent on the left than on the right side. It is, indeed, extremely rare on the right side, and almost never occurs only on that side. In eight out of seventeen cases, the veins of the right side were enlarged simultaneously with those of the left, but to a much less degree. It is very rarely necessary to operate on the right side. Out of 120 operations performed by M. Breschet, one only was on the right side. The chief reasons which have been assigned for the greater frequency of varicocele on the left than on the right side, are the following. 1. The right spermatic veins open into the vena cava in a direction parallel to the axis of that vessel, while the left opens into the left emulgent vein at right angles to the current of blood which flows through it. 2. The greater length of the left spermatic vein. 3. The pressure of the contents of the sigmoid flexure of the colon. With regard to this last cause, Landouzy observes that only one out of seventeen patients was affected by constipation. Amongst the occasional causes of varicocele may be mentioned, all those which either prevent the return of blood to the heart, or determine it in large quantity to the organs of generation. These need not be particularized. There seems to be no close connexion between varix and varicocele. Of fifteen cases of varicocele, one only was affected with varices, and of twenty persons who had varicose veins in the lower extremities, no single one had varicocele. The symptoms of this disease are slight at first, and its existence is usually discovered by accident. There is a feeling of weight in the testicle, perineum, and loins, and an unusual twitching in the course of the cord; the scrotum is long, pendant, and soft, and increases rapidly in volume under the influence of heat or fatigue. The patient carries the hand, at every instant, to the scrotum, in order to give it a more favorable position. If the patient is not subject to much fatigue, if he does not remain for any length of time in the erect posture, and avoids all the exciting causes of the disease, a suspensory bandage will guarantee him against further suffering. But if the disease is allowed to go on unchecked, it becomes a source of constant suffering. A short walk causes extreme fatigue, the breathing becomes hurried, the face is bathed in sweat, and expresses the deepest distress. In some cases it is impossible to assume the erect posture, without the aid of a suspensory bandage. The case of one of the most celebrated dramatic authors of France is mentioned, who had acquired the habit of composing whilst rapidly pacing his chamber. This disease entirely put a stop to his perambulations, and with them to his literary productions. He was restored by an operation performed by M. Breschet. There is one symptom which our author has never known to be absent, but which has been omitted by other writers on this subject. It is an increased perspiration of the skin of the scrotum on the side affected. This secretion is, in some cases, so abundant as to require the use of a fold of linen. The superficial veins may acquire an enormous size. One case is mentioned in which they equalled, and even surpassed, the volume of the crural vein. Some cases are quoted from Pott and Sir A. Cooper, in which the disease seems to have made a sudden attack; in these instances, Landouzy thinks that the disease had existed in a less marked form, before the acute attack commenced. The atrophy of the testicle, which took place in more than one instance in which varicocele followed an accident, is justly attributed to the accident, and not to the varicocele which was the consequence of it; nevertheless, when the disease is very much advanced, the enlarged veins compress the testicle, and cause the absorption of it. Out of fifteen cases, our author found the testicle in a more or less advanced state of atrophy in nine. The occurrence of atrophy of the testicle, as a consequence of varicocele, is established by quotations from Celsus, Callisen, and Pott. Sir A. Cooper, however, does not seem to have met with any examples. One case, mentioned by Pott, is the only one in which atrophy of both testicles took place, but Landouzy has often observed the right testicle partially atrophied in varicocele of the left side. The atrophy of the testicle is proportioned to the extent of the varicocele. Not so, however, the pain, which is often most considerable where the veins are least enlarged. This fact is attributed to an enlargement of the small veins surrounding some nervous fibres. It is to the acute pain experienced in some cases, and to the constant uneasiness present in all, that the deep melancholy common to almost all diseases of the urinary and genital organs is to be ascribed. The chief object of Landouzy's paper is to prove the superiority of M. Breschet's method of compression to all others which have been recommended. Thirteen cases are related, in all of which great relief, in the majority a perfect cure, was effected by this means. The danger, too, of inflammation of the veins is much less than when other methods are resorted to: the cure, moreover, is accomplished in a less space of time.

As a preliminary step to the performance of M. Breschet's operation for varicocele, it is necessary that the diseased veins should be considerably distended with blood, in order that none of them may escape the compressing action of the forceps; for this purpose, in summer it will be sufficient that the patient should walk for some time previously, but in winter it will be desirable that he should also take a warm bath. This precaution being taken, and the scrotum being previously shaved, the patient stands upright before the surgeon, who, if the varicocele is on the left side, grasps the right side of the scrotum with his left hand, whilst with his right he endeavours to discover the situation of the vas deferens; this is not difficult; its normal situation is at the posterior part of the cord, its form that of a cylindrical stem, equal through its whole extent, its volume that of a large crow-quill, its consistence is hard though elastic, and may be compared to that of a nerve. But the best means of assuring yourself that you hold the vas deferens, is to press it between the fingers, when the patient should feel a peculiar painful sensation, referred both to the testicle and the groin, and which can scarcely deceive either the patient or operator. Having discovered the vas deferens, the operator draws it towards the septum scroti with the thumb and forefinger, and endeavours to separate the veins from it, and to collect them towards the external part of the scrotum. This sort of subcutaneous dissection forms the only difficult part of the operation, and requires patience and attention; the separation of the vessels must be made with the greatest care, in order that no vein should remain with the vas deferens and spermatic artery. The veins being thus separated, an assistant places the first forceps on the upper part of the scrotum, transversely, and as high as possible, but far enough from the root of the penis to prevent the formation of an eschar on it; it will be found convenient to raise the penis against the abdomen. The branches of the forceps should be carried as far as possible towards the septum, excluding the vas deferens, and at the external part of the scrotum a pedicle of skin about two lines in width, and containing capillary vessels only, should be left uncompressed. As soon as the first pair of forceps is properly placed, it should be screwed tight. The second pair should then be placed, in like manner, as low down as possible, without comprising the testicle, and should be tightened in the same way. An improvement in the construction of the forceps is the introduction of a supplementary blade, which may be depressed daily, by means of a screw, so as gradually to increase the pressure without augmenting the pain. It is necessary to be careful, that this increased pressure commences towards the septum, otherwise the vas deferens might be included between the blades. In general, severe pain is felt in the scrotum and groin, immediately after the operation; but this ceases in a few hours, and no further suffering is produced. A compress dipped in cold water should be applied to the scrotum, which should be slightly elevated. The forceps may be removed, from the fifth to the seventh day. The bridle of skin on the outside of the scrotum facilitates much the cicatrization of the wound, the edges of which would otherwise be widely separated by the erections of the penis, and by the weight of the testicle.

Journal des Connaiss. Méd.-Chir. Jan. Mar. 1838.*

A Case of Wound of the Heart. By Dr. STEIFENSAND, of Crefeld.

WOUNDS of the heart have always been accounted extremely dangerous; and although there are examples in which death followed upon penetrating wounds of the heart, only after a lapse of some days, or even weeks, yet in general such injuries are regarded as necessarily fatal. Lately, however, doubts have arisen as to the necessarily mortal character of wounds of the heart, and cases are quoted in which cicatrices of the heart, or even foreign bodies lodged in its substance, were found after death. The following case is interesting, as it shows the continuance of the functions of the heart for some days after severe injury.

C. H., a young man, aged twenty, was stabbed on the evening of the 16th September, 1837, probably with a knife, by some person unknown. After receiving the wound, he walked about 100 paces, and then fell into the arms of his companions, exclaiming that he had been stabbed. He was carried to his house, about a mile distant, and a surgeon was immediately sent for. The wound was about an inch in length, situated to the right of and close to the sternum, between the third and fourth ribs, and running in an oblique direction downwards. It had ceased bleeding; a compress was applied, and some antiphlogistic medicine prescribed. For some days the wound appeared to be superficial, but the patient was always extremely restless, and much distressed by thirst. On the 20th, whilst at stool, there was a sudden hemorrhage from the wound, which was, for the moment, stopped by the application of a fresh compress, but returned repeatedly, so that the surgeon, who till now had not been apprehensive of danger, became alarmed, and called in Dr. Steifensand. On the afternoon of the 22d, Dr. S. found the patient lying on the back, pale, with no pulse, the extremities cold, and the respiration oppressed. The beat of the heart was audible on applying the ear to the chest, and was accompanied by a peculiar short metallic sound. Black blood flowed from the wound, and the quantity was increased by pressure on the walls of the thorax. The probe was arrested by the cartilages of the ribs, and the state of the patient did not warrant a more particular examination. The bandage was applied anew, and the patient recommended to remain as quiet as possible. The restlessness, however, still continued, and he died on the morning of the 23d.

On examining the body, the right cavity of the chest was found filled with dark fluid blood. The knife had traversed the cartilage of the fourth rib of the right side, had divided the internal mammary artery, and passed through the pericardium into the right auricle, near its junction with the ventricle. The wound of the pericardium was about three lines in extent, that of the auricle about two lines. The right lung was collapsed, and pressed upwards.

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* These papers have since been published as a separate work, by M. Landouzy.

In general, wounds of the heart are not followed by immediate death. Ollivier has collected fifty-four cases of penetrating wounds of the heart, among which were twenty-nine of the right ventricle; and in these, with the exception of two cases, death ensued between the fourth and twenty-eighth days. Twelve of the cases were wounds of the left ventricle, and these, with the exception of three, which survived respectively forty-nine hours, three days, and ten days, proved immediately fatal. The greater frequency of wounds of the right ventricle depends evidently on its position, and the more sudden death in wounds of the left ventricle is owing to the difference of structure and function.

Casper's Wochenschrift. No. 15. 1838.

Development of Hair in the Posterior Chamber of the Eye.

By Dr. Theodore Ruete, of Göttingen.

This was the case of a man thirty years of age, by trade a tinker. On the cornea, which was in other respects quite natural, there was a slight cicatrice; the anterior chamber natural; the iris appeared unchanged in structure, but its pupillary margin was, to the greatest part of its extent, adherent to the capsule of the lens. The latter was opaque, and appeared to have several fissures in it. But the most remarkable thing was the appearance of four hairs behind the pupil, two longer and two shorter. They sprang out of the bottom of the posterior chamber, from the capsule of the lens. Besides these, a still longer hair pierced the iris to the left of the pupil, and lay stretched on the iris in the anterior chamber. This state was traced to an injury from a chip of tinned iron, which struck his eye in an incandescent state, three years ago.

Monatsschrift für Medicin, Augenheilkunde und Chirurgie. Jan. and Feb., 1839.

Artificial Anus made in the Groin, with success.

An infant three days old did not present any traces of the anal opening. The raphé of the perineum extended without interruption from the scrotum to the point of the coccyx. The abdomen was tender and tympanitic, but there was no vomiting. The infant had taken the breast several times, and had passed its urine without difficulty. An incision of several lines in length was made over the supposed situation of the anus, and carried to the depth of three quarters of an inch or more, but without success. It was decided then to open the cæcum in the right iliac fossa. An incision was made near the anterior iliac spine; a small knuckle of intestine presented itself, which was replaced, and the cæcum was found without difficulty. It was opened, and several ounces of meconium immediately escaped, followed by a remarkable amelioration of the symptoms. The progress towards cure was very rapid; the alvine evacuations continued to be passed by the artificial opening, and on the eighth day after the operation the sutures were removed.

Medizinische Zeitung für Heilkunde in Preusen.

On the Use of Conium Maculatum in Scrofulous Ophthalmia.

By Professor Otto of Copenhagen.

Kopp, of Hanau, recommends for scrofulous ophthalmia the conium maculatum. His formula is: ℞. Ext. Conii Maculati ʒj. Aquæ Cinnamomi Spirituosæ ℥jv. Solve. Of this he gives children of two or three to four years old, and older, four drops three times a day, daily adding a drop to each dose. Blisters behind the ears, and compresses, wet with tinct. thebiaca, to the eyes were at the same time used. Professor Otto says he has cured more than thirty cases of scrofulous ophthalmia by this plan. He has, with Kopp, raised the doses as high as thirty to thirty-five drops without any bad result.

Wochenschrift für die ges. Heilkunde. April 6, 1839.

Singular Case of a Living Snake in the Stomach. By Dr. MANDT, of St. Petersburg.

A RUSSIAN peasant, by name Abraham Isajeff, of the district of Oranienbaum, æt. 36, was suddenly roused from sleep under a tree, on the 27th of July, 1838, by the distressing sensation, as if ice-cold water was passing from his mouth to the stomach. Immediately afterwards he felt something moving in his stomach, and could even perceive the motion by the hand pressed on the part. The most distressing sensation was that of extreme coldness in the stomach; but the conviction that he had swallowed a venomous serpent rendered the poor man almost distracted. He refused to eat for fear of nourishing it, and took tobacco, brandy, emetics, purgatives, &c., partly by the advice of his friends, and partly under medical direction, in the hopes of killing the animal, or of ejecting it upwards or downwards; but all in vain. He was seen on the following day, about twenty-six hours after the accident, by Dr. Selle, who was able to detect the motion of the supposed animal by the hand, through the abdominal parietes; and who also recognized, by means of the stethoscope, sounds produced by it, viz. "a half-gurgling, half-wheezing sound, and a noise, as if a hard body struck an extended surface at irregular intervals." Dr. Selle watched the case narrowly, of all the particulars of which a minute account is given. The motions and sense of coldness continued in the region of the stomach until the 31st., when they seemed transferred to the umbilical region, and then were, for the first time, accompanied by a sense of local heaviness or weight (Schwere). This day the patient was seen by Dr. Mandt, who conducted the after-treatment. He thought he could perceive, on examining the abdomen, "a longish foreign body" in it, but was not quite certain. On the 1st of August the patient was also seen by Dr. Hassing, staff-physician-general. This was the last day the patient felt the movements, or sense of coldness in the abdomen, but the weight still continued. The man being now convinced that the snake was dead, was roused from his mental agonies, recovered his spirits and courage, and began to eat. Purgatives of calomel, castor-oil, and jalap, and clysters of infusion of valerian were persevered in which acted readily and strongly on the bowels; but without the ejection of any foreign substance. On the 7th of August the man went to see his family, promising faithfully (verspricht aber heilig) to take his purgatives, and to examine carefully all his evacuations. He showed himself again on the 9th, but could not be kept from his own home any longer. He returned, however, to the doctor's the following day, "happy and triumphant, exhibiting in a glass the expelled snake, covered with excrement," which, he said, had come away from him in two pieces; the tail-end at three o'clock, and the remainder five hours thereafter. The only parts wanting of the animal were one side of the lower jaw, the rest of the head, and a small bit of the body between the tail and abdomen. It was upwards of a foot long, and about the thickness of a full-sized man's finger, and was recognized as the common viper (*vipera berus*), by its colour, the character of the tail, &c. In several places the vertebral column was broken; and the skin everywhere exhibited marks of the digestive action, in loosened scales, &c. The internal organs were all sound.

[From the whole history of this case, so minutely detailed in the original, and from the character of the narrator and witnesses, it is hardly possible to doubt the truth of the narrative as above detailed: still it will not escape the sceptical, that the positive *proof* of the two grand facts, the swallowing and the discharge of the snake, is still wanting. It is, at least, *possible*, that the snake presented to Dr. Mandt, and of which a representation is given, may have never entered or passed from the intestinal canal of Abraham Isajeff; but we must say, in justice to this highly respectable physician, that no one who reads this interesting history, will be much disposed to doubt the facts as there related.]

Rust's Magazin für Heilkunde, B. liii. Heft 3. 1839.

Ivory Bougies.

Charriere, surgeons' instrument maker in Paris, has exhibited to the Academy bougies and other instruments, made of flexible ivory (ivory from which the calcareous matter has been extracted). They are according to the pattern of some bougies given to him by Dr. Jüterbock, of Vienna. They serve the purpose completely of elastic gum instruments, and have the great advantage, that they may be made in a few days, whereas the preparation of caoutchouc instruments occupies several months. In a practical point of view, the ivory bougies have the advantage that, when they are dry, any desired bend or curvature may be given to them, which is retained notwithstanding their elasticity. The dryer they are on introduction the more they expand, without losing in durability and firmness.

MIDWIFERY.

On the Causes of Face Presentations, &c. By Dr. J. F. Osiander, Professor of Medicine at Göttingen.

This Essay contains but little practical information respecting face presentations, as the groundwork upon which that information must be based, viz. a minute description of the manner in which the face presents and passes through the pelvis and external passages, is very defective. The author does not agree with Boer that the chin is invariably directed forwards, although this opinion was confirmed by Siebold, who declared, that he had never met with a face presentation where the chin was turned towards the sacrum, an opinion also confirmed by Naegele, &c. He remarks, that it is scarcely credible how much the different parts of the face are squeezed together during labour, a circumstance in which we fully agree, having, on more than one occasion, had the opportunity of *seeing* the face as it was passing over the perineum. In speaking of the relative frequency of face cases, he shows an apparent discrepancy in the experience of different authors: thus La Motte, out of 400 labours, observed only two presentations of the face; Mauriceau, in 700 cases, only four; Giffard only one case in 200; Portal three in 80, and Smellie nine: whereas Boer is stated to have observed 243 cases of face presentation in seventeen years, of which only three cases required artificial assistance; the correctness of this statement Dr. Osiander considers invalidated by the manner in which the reports of the Vienna Lying-in Hospital were kept; a circumstance which we should be inclined to doubt, when we consider the great extent of this establishment, and the high character of the celebrated Boer. The author gives a long list of causes; all of which, with the exception of an unusual quantity of liquor amnii, are in our opinion merely imaginary.

Hamburg. Zeitschrift. December, 1838.

MEDICAL JURISPRUDENCE.

Case of alleged Poisoning by the Sulphocyanic and Hydrocyanic Acids. By Dr. Graff, of Darmstadt.

A mechanic, who had gone home the previous evening apparently in good health, was found the following morning lying dead in bed, with the door of the room and the windows fastened. A medico-legal inspection of the body was required to be made, and this accordingly took place about 24 hours after death.

On a table in the room was found a small glass retort with its body broken. It contained yellow-coloured crystals, afterwards proved to be the ferrocyanate of potash, mixed with a gray and blue-coloured matter. Near it was some strong sulphuric acid in a bottle; and in another small bottle, which seemed to have been used as a receiver, was about a scruple of a clear yellow liquid. The deceased was dressed, and lying on the bed in a composed attitude. The countenance was pale and tranquil: a mucous frothy liquid, tinged with blood, issued from the nostrils. A strong and offensive smell exhaled from the body, but there was no odour of hydrocyanic acid mixed with it. The skin was generally in a state of horripilation. On opening the cranium, the only appearance observed was congestion of the vessels of the brain. The mouth and fauces were free from excoriation and inflammation: but the mucous membrane of the lower part of the trachea was inflamed, and of a deep red colour. The lungs were congested and dark coloured. Dark blood was contained in both sides of the heart, but especially on the right side. Coagula were found in both cavities, as well as in the aorta and venæ cavæ. The stomach, having been included between ligatures, was removed. Externally, the vessels near the pylorus appeared congested; but at the greater end there were several dark-coloured patches. It was then opened, and the contents reserved for analysis. The mucous membrane was pale, without inflammation or excoriation: but at the fundus, for about three square inches, it was of a dark brownish black colour, softened and pulpy: there was no sign of inflammation beneath or around this discoloured and softened membrane. Some half-digested food was mixed with the contents. The bladder was much distended with urine, the other viscera presented nothing abnormal. The only other appearances remarked were that the nails of the fingers were of a deep violet hue; and the penis, when the body was first seen, was in a state of semi-erection. There was not the least odour of prussic acid in any of the cavities or organs.

From these facts the following conclusions were derived:

1. Death was the result of suicide. This was established by the circumstantial evidence. It was ascertained that the deceased had some knowledge of chemistry. He had made no secret of his intention to destroy himself, and had repeatedly enquired of friends respecting the best method of preparing prussic acid.

2. Death must have taken place at night between ten and eleven o'clock, since the deceased was seen to sup with his accustomed appetite at eight in the evening; and the half-digested state of the contents of the stomach, seems to show that he had not survived his evening meal longer than two or three hours. [It is a remarkable omission in the report, that, although the appearances of the body, &c., are marked down in no less than 104 sections, not a word is said concerning the presence or absence of *warmth* or *rigidity* in the body when first discovered; conditions which, properly observed, throw considerable light on these important questions of survivorship.]

3. Death was most probably due to the action of a powerful narcotic, mixed with some acrid or corrosive poison. The post-mortem appearances establish this. (?) The absence of erosion in the mouth and fauces shows that the corrosive poison could not have been very concentrated. Any of the strong mineral acids could not, therefore, be suspected.

4. Death took place rapidly. This was proved by the fact, that there was not the least sign of redness or irritation around the spot in the stomach, which had become corroded. Death did not result from this corrosive action, but from the influence exerted by the poison on the brain and nervous system. The position of the body seemed to show that death was not preceded by pain.

5. The poison was doubtless a mixture of the sulphocyanic and prussic acids, probably prepared and taken by the deceased immediately after its preparation. Very different statements are made relative to the appearances left by prussic acid in the body. Vogt says it produces redness of the mucous membrane of the alimentary canal and air-passages, and rapid putrefaction. Orfila denies that redness of the mucous membrane is a result of its action, and affirms that the body is a long time before it putrefies. In a case reported in Hufeland's journal, the blood had a smell of bitter almonds, the cerebral vessels were congested, and the stomach was much inflamed and softened. The blood was liquid, and thick like oil. In a case of Casper's, the only appearances were, a strong odour of the

acid, and the blood of a dark violet colour. In this case, putrefaction took place slowly; but the absence of all odour of prussic acid, and the corroded state of the stomach, rendered it unlike the recorded cases of poisoning by that liquid. The most-carefully conducted experiments by Dr. Merk did not lead to the detection of this poison, in the contents of the mouth, œsophagus, stomach, or intestines; but he thought he discovered a faint trace in the yellow liquid, contained in the receiver already mentioned. This, however, was too faint to allow an opinion to be expressed with any medico-legal certainty. By pursuing the process supposed to have been adopted by the deceased, namely, the distilling of sulphuric acid with ferrocyanate of potash, Dr. Merk obtained principally sulphocyanic acid mixed with a small quantity of prussic, formic, and sulphurous acids. As sulphocyanic acid is a corrosive and deadly poison (?) without smell, the whole of the facts connected with the death of the deceased were considered to be at once explained by his having prepared and swallowed this substance. Three drops of the liquid obtained by Dr. Merk, killed a young rabbit in half a minute! and on inspection, the appearances very closely resembled those found in the body of the deceased. On the other hand, Prof. Liebig, of Giessen, was requested to determine what products were obtained on distilling sulphuric acid and ferrocyanate of potash. He procured no sulphocyanic acid, but simply formic and sulphurous acids. Moldenhauer, another chemist, on performing the experiment obtained this acid, mixed with the others mentioned. The contents of the recipient found on the table, in the deceased's room gave, on analysis, sulphuric acid, ferrocyanate of potash, and traces of sulphocyanic acid. (?) The contents of the deceased's stomach, amounting to sixteen ounces, were divided into two equal portions. One half, after phosphoric acid had been added, was distilled into a receiver containing a solution of pure potash. A persalt of iron was added to the alkaline solution after the distillation had gone on sufficiently, but the test did not indicate the presence of either of the suspected poisons. The other half was distilled into a receiver, containing the ammoniaco-nitrate of silver: but this, on subsequent examination, gave only the faintest traces of what was supposed to be cyanide of silver. The conclusion came to was that sulphocyanic acid, mixed with a small portion of the other acids already mentioned, had destroyed life.

Henke's Zeitschrift, 1838.

[Remarks.—The interest of this case is in great part lost, from its not being at all clearly made out, what poison the deceased really took. Minute chemical details occupy about one half of the report; but they leave the case in greater mystery, than they found it. The case is so far important, that if it really be one of poisoning by the sulphocyanic acid, it is the first on record, and is much opposed to the generally received opinions of toxicologists, for it is here represented to be as fearfully destructive as hydrocyanic acid itself. The experiments of Prof. Meyer, of Bonn, have, however, shown that this acid has but very little energy as a poison. Three drachms of the pure acid, in three doses, had no effect when introduced into the stomach of a rabbit; while we are told by Dr. Graff, in this paper, that *three drops* killed a rabbit in *half a minute*. How can such an extraordinary discrepancy be reconciled?—We think by showing that Dr. Graff was in error, supposing that in his case the animal was not killed by suffocation, a very common source of failure in experimenting with poisons on rabbits.

In the case reported, Dr. G. seems to have adopted the idea that prussic acid could not have been taken, because there was no odour on inspection, or in the apartment; a fact, however, wholly insufficient to justify the inference, since the acid may be taken in a small but still a poisonous dose; and after twenty-four hours' exposure, or even less, it may not be indicated by any smell issuing from the body. The corroded state of the stomach was considered another proof of this poison not having been taken: but this might have proceeded from some corrosive substance having been swallowed simultaneously with prussic acid. At any rate, the sulphocyanic acid, to which this effect is assigned by the reporter, although an irritant, is not a corrosive; and its action on cork, referred to by him as evidence of its possessing this property, is really due to another cause; namely, to its combining with the minute traces of iron contained in that substance. But further:—Is sulphocyanic acid a product of the distillation of ferrocyanate of potash and sulphuric acid? We confess we do not see how this can happen, and we would rather abide by the results obtained by Liebig than trust to those of the other chemists referred to by the author, who seem to have undertaken the investigation with the full intention of obtaining sulphocyanic acid. The details of the experiments of the latter chemists are by no means satisfactory; and considering that sulphocyanic acid may be readily detected in an infinitesimal proportion, our impression is that they obtained none, or but the faintest traces. On the other hand, prussic acid is known to be a product of this distillation, when the sulphuric acid is diluted; and also, but perhaps to a less extent, when the acid is concentrated. It appears to us that this was the poison which the deceased really prepared and swallowed, probably mixed with sulphuric acid. Our opinion is founded on these facts: he must have died quickly; there was no sign of vomiting or purging; there were no marks of irritation or inflammation in the alimentary canal; and he had materials at hand from which prussic acid might be made, with sufficient chemical knowledge for its preparation. The non-discovery of any traces of it in the contents of the stomach might be accounted for.

To test the correctness of the preceding views, we distilled equal parts of ferrocyanate of potash and strong sulphuric acid to dryness. A small quantity of a yellowish coloured liquid was obtained in the receiver, in which the odour of hydrocyanic acid was plainly perceptible. Nitrate of silver gave a white, curdly precipitate, showing the presence of hydrocyanic or sulphocyanic acid, or both. The liquid was now divided into two parts:—To one, a few drops of sesquichloride of iron was added, and a slight reddish tinge was produced, which disappeared on adding chloride of gold. This showed the presence of the faintest traces of sulphocyanic acid. To the other portion of liquid, a saturated solution of protosulphate of iron was added. By the subsequent addition of potash and dilute sulphuric acid, a comparatively large proportion of Prussian blue was left; thus proving that the main constituent of the distilled liquid was really *hydrocyanic acid*. The residue in the retort, which was dry and hard when dissolved in water, yielded a considerable quantity of Prussian blue, and the liquid chiefly contained sulphate of potash. We consider that the facts of the case thus receive an explanation, and we see no reason therefore for admitting that it forms an exception to the general experience of toxicologists. The sulphocyanic acid, if obtained at all by the deceased, must have been in too small a quantity to have led to fatal consequences within so short a period. Its operation, besides, is that of an irritant, not of a narcotic.]

MEDICAL STATISTICS.

Report of the Results of Revaccination in the Prussian Army in the year 1838, *drawn up from Official Documents.* By Dr. LOHMEYER.

TOTAL number of individuals inoculated (revaccinated) 42,041, of which number there were cicatrices from former vaccination

distinct, in	33,819
indistinct, in	5,645
none, in	2,577.

The resulting vaccination was regular in	19,117
irregular in	8,672
did not take in	14,252.

In the cases of failure the vaccination was repeated,

with effect in	2,306
without effect in	10,424.

Of the individuals in whom the disease was perfectly regular, there were from

1	to 5 pustules in	8,787
6	.. 10	5,581
11	.. 20	4,056
21	.. 30	693.

Of individuals revaccinated with effect in the present and former years, there were affected, during 1838,

with varicella . . .	19
.. varioloid . . .	10
.. true smallpox . .	2

It appears, from the preceding statement, that the results of revaccination in the army, during the year 1838, closely resemble those observed in 1837, (see *British and Foreign Med. Rev.* vol. VII. p. 186;) in both years about 45 in the 100 exhibiting true vesicles running a regular course. The resemblance would have been still greater, but for the circumstance that many men were included who, according to their own account, had been previously revaccinated at their own homes, but who exhibited no marks on their arms; these were almost always among the failures. According to the reports of the medical officers, there were also many cases in which the development and course of the disease were disturbed, chiefly through carelessness on the part of the individuals in allowing the pustules to be broken or otherwise injured.

The opinion formerly pretty prevalent that, failing lymph immediately from the cow, the only proper matter for vaccination is that taken from children with good pocks, has now few supporters among the military surgeons, as they have ascertained by manifold experience that the lymph from well-formed pocks of adults, whether in the first or second vaccination, produces as fine and regularly-proceeding vesicles as that from children. Accordingly, it is the practice of most of the medical officers, only in the commencement of the revaccination to use lymph from children. Many even, like the Wurtemburg vaccinators, give the preference to the latter for communicating the disease to adults.

Inoculation with dry, preserved lymph, was in a great degree ineffectual. In a child vaccinated with lymph of this kind, there appearing no result, it was again vaccinated, eight days later, with fresh lymph from the arm, after which not only all the latter punctures, but also two of the former took and exhibited good and regularly-proceeding pocks. In the case of a man of the 6th artillery brigade, the pocks did not appear until six weeks after the inoculation (two on the right and three on the left arm), and their development was accompanied by so much inflammation in the vicinity, and gave rise to so much fever, that it became necessary to remove the patient into the hospital.

In several individuals natural smallpox appeared soon after the vaccination. In two cases this happened before the third day, and in these the vaccination had no result. In two other cases, on the contrary, the modified smallpox (varioloid appeared when the vaccine vesicles were in perfection, and then they sustained no alteration in their progress.

The whole number of smallpox cases in the army, in 1838, were 111, 56 being characterized as varicella, 43 as varioloid, and 12 as true variola; in this number are included the 31 cases formerly mentioned as occurring after successful revaccination. Seven cases of the smallpox were fatal, but no fatal cases occurred among the 31 just mentioned; in all of whom, on the contrary, the disease was mild, and indeed quite insignificant. The greater number of cases of smallpox (as was also the case in former years) took place in recruits shortly after joining the army, and before revaccination could be employed. Some of the older soldiers, however, were also attacked with the disease in some of its forms; for instance, some of the subordinate officers who had been previously revaccinated without effect, or who had entered the army previously to the introduction of the practice of revaccination. Most of the cases of natural smallpox occurred in the 7th division, viz. 37, and for the most part in the garrison of Minden. In the 4th division not a man failed with smallpox, although the troops mixed more or less with the inhabitants of their stations or of the vicinity. This favorable result is principally to be attributed to this circumstance—that in the district of the 4th division the recruits joining the army in the autumn of 1837 were, for the most part, subjected to revaccination immediately on their arrival; whereas, in other cases, it has been customary to put off the revaccination of such recruits, at least in a considerable proportion, until the spring. *Medicinische Zeitung. May* 8, 1839.

II. THE AMERICAN AND COLONIAL JOURNALS.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

On Narcotine as a substitute for Quinine in Intermittent Fever.

By Dr. O'SHAUGHNESSY.

On the 4th of August, 1838, at the meeting of the Medical Society of Calcutta, Dr. O'Shaughnessy laid before the Society the details of thirty-two cases of remittent and intermittent fevers treated by narcotine as a substitute for quinine, and of which thirty-one were cured. The cases previously described in the first Report of the Pharmacopœia Committee were twenty-seven, making on the whole sixty, of which the narcotine was successful in all but two.

The cases now communicated were as follows:

Two cases by Dr. Goodeve. One of them, the case of the late deputy-collector of Chittagong. Quotidian of several months' standing, spleen enlarged. Quinine was used without success, although given in every possible form. Arsenic was then tried and checked the fever, but did much mischief to the patient's general health. Narcotine was then given, and with such success, that Dr. Goodeve concludes it thus: "I do not hesitate in saying that this patient owes his life to the remedy in question." The other case was a patient labouring under inflammation of the bowels at the same time, where the administration of quinine would have been inadmissible.

Three cases are reported by Dr. Smith, of Hidgelee, who adds, "As far as these three cases go, I cannot speak too favorably of narcotine, and am very desirous of trying it more extensively." Capt. Marshall, of Calcutta, communicated three cases of severe ague occurring among his servants; all were rapidly cured, and Capt. Marshall says, "It would be presumptuous in me to offer any opinion as to the virtues of narcotine; all I can say is, that if ever I am ill of fever I shall unhesitatingly and confidently prefer it to sulphate of quinine or any other medicine I know of."

Mr. R. O'Shaughnessy described the case of a man on whom he had operated for stone, and who was attacked by violent ague on the day of the operation. The ague returned next day at the same hour. Mr. O'Shaughnessy considered it unsafe to employ quinine under these circumstances, and had recourse to narcotine. Four doses of this medicine were given, and Mr. O'Shaughnessy states, "The fever did not return, the wound was not in the slightest degree affected; there was no excitement or headach produced. After he took the first dose he slept soundly, which he had not done the two previous nights, and he was discharged cured of the effects of the operation on the fourteenth day after its performance."

Mr. O'Brien, the apothecary of the Native Hospital, reported three cases, Mr. Evans one, the Pundit Modoosoodona Gupta one, all successfully treated. The pundit's patient laboured under dysentery at the same time.

Dr. J. Chapman, assistant-surgeon of the Calcutta General Hospital, related the case of a European who contracted violent remittent fever at Kedgeree on the 16th of July, and was received in hospital on the 19th. Quinine was used in the usual manner on the first remission on the 20th, and again on the 21st, but the symptoms were rather aggravated than improved. The narcotine was then given, and its use was speedily followed by a complete remission. From that time the fever did not return, with the exception of restlessness and slight headach on the evening of the 23d. On the 28th, all medicines were omitted, and the patient was discharged convalescent.

Dr. O'Shaughnessy further submitted two cases, treated in his own house among his servants, both of which were cured. Lastly, he communicated fifteen cases extracted from the journals of the Medical College Hospital. In five of these cases quinine and arsenic had failed, in eleven there was enlargement of the spleen or liver, in one inflammation of the knee-joint. Seven of these cases were remittents, and two of these had died. Of the two fatal cases one was admitted on the seventh day of violent fever and died next day. In the second (a child) the spleen, liver, pancreas, and mesenteric gland, were immensely enlarged, and the case hopeless from the beginning.

Dr. O'Shaughnessy added that, besides the sixty cases now recorded, more than 100 ague patients had been treated by his pupils and acquaintance with perfect success by this remedy.

[In a subsequent number of the India Journal the following letter appears, addressed to Dr. O'Shaughnessy, by Mr. **Green**, *Civil Surgeon, Howrah.*]

"I have now employed the narcotine in sixteen cases of remittent fever, and such is my opinion of the efficacy of the remedy, that in instances of fever, intermittents and remittents, in ordinary healthy subjects, and in whom there is no complication of severe organic disease, I give it with the full expectation of arresting the next periodic return of the fever. I have seen the result follow in ten of the cases of the fever alluded to. I consider narcotine a more powerful antiperiodic than quinine. The remedy does not act silently. I have observed a degree of general heat follow its use in the first instance, and, subsequently, perspiration, so that it appears to excite in the system a salutary and powerful counteraction as to stop the morbid concentration that issues in fever. I have not observed narcotine to lead to local organic disturbance in the cases in which I have used it. In short, even from my scanty experience, I consider the remedy an invaluable one."

India Journal of Med. Science. Sept. and Nov. 1838.

III. THE BRITISH JOURNALS.

(FOR THE QUARTER ENDING MAY 31, 1839.)

ANATOMY AND PHYSIOLOGY.

Experimental Investigation into the Functions of the Eighth Pair of Nerves.

By **John Reid**, M.D.

This series of experiments was performed in continuation of those formerly published by the author, of which we inserted an abstract in our Fifth Volume (p. 308). A brief account of them was given by Dr. R. at the Newcastle Meeting of the British Association, where they excited considerable attention. They are of so interesting and important a character that we shall present their results somewhat in full; referring, as before, to the original memoir for the evidence by which these are supported. In saying that the excellence of this paper fully bears out the opinion which we formerly expressed as to the merits of the author as an experimental physiologist, we do not perceive that we can offer higher commendation. Dr. Reid possesses, in a remarkable degree, the qualities required for his task—ingenuity in devising experiments, skill in executing them, and caution in reasoning upon them; a combination which is wanting in too many of those who are held up as luminaries of physiological science. We are satisfied that any one who will compare, without prejudice, the two unpretending papers of Dr. Reid with the boasted and boastful *Leçons* of M. Magendie, will not fail to draw conclusions in regard to each of these points by no means favorable to the latter.

The experiments related in this paper almost entirely concern the functions of the pulmonary and gastric branches of the par vagum—those *quæstiones vexatæ* of neurology. A few additions are made, however, on the subjects of the former series; to these we shall first advert.

Glosso-pharyngeal Nerve. Dr. Reid has now frequently succeeded in producing *the usual muscular movements* of deglutition, by pinching the trunk of this nerve. He had before only produced convulsive twitches. As he has fully proved that it is not a motor nerve, but acts on the muscles by reflexion, it may evidently be regarded as the principal excitor of the movements of deglutition. That irritation of the trunk of the nerve should have less power of exciting them than impressions made on its extremities, harmonizes well with what we learn from other sources of the character of this class of actions.

Pneumogastric Nerve. With regard to the general character of this nerve, Dr. R.'s subsequent experiments have fully confirmed his former opinion, that it ministers to sensation; indications of severe suffering being given when its trunk is pinched, cut, or stretched. He finds an explanation of the negative results which some experimenters have obtained, in the fact that some animals appear much less sensitive to pain than others, even of the same species. "I have experimented on dogs which have endured the incisions necessary to expose the sheath of the carotid artery, without any apparent uneasiness." Magendie has made similar observations: "Such facts ought to make us hesitate before arriving at negative conclusions upon the sensibility of nerves, and forcibly point out a serious error to which limited observations are liable."

Laryngeal branches. On the functions of these, also, Dr. R. has obtained additional proof of the correctness of his views. He has found that, after the first paroxysm occasioned by division of these nerves, a period of quiescence and freedom from dyspnœa often supervenes, the respirations being performed with ease as long as the animal remains at rest; but an unusual inspiratory movement, such as takes place at the commencement of a struggle, induces immediate symptoms of suffocation, the current of air carrying inwards the arytenoid cartilages, rendered passive by the paralysed state of their muscles, so as to obstruct the opening of the glottis.

Cardiac branches. From seven experiments upon dogs, the following inference is deduced, which is opposed to that of M. Brachet: "After section of the vagi the pulsations of the heart may not only be quickened by muscular exertion, but also by mental emotions. Though in all probability the vagi are the usual channels through which mental emotions affect the heart, yet it appears that this may also take place through the medium of the ganglionic system of nerves."

Pulmonary branches. The author confirms his former statement that lesion of one of the pneumogastrics does not necessarily or even generally induce disease of the lung on that side; and he then discusses at much length the effects of lesion of this nerve upon the *respiratory movements.* A large number of experiments were performed to determine this point; and it was an object in these to avoid the disturbance arising from the paralysis of the laryngeal muscles as much as possible. The details of Dr. R.'s method of operating are given by him, that those who wish to test the value of the facts ascertained by him may repeat his experiments in a similar manner. "It is not so much," he remarks, "the frequency of false observations that we have to complain of in medical science as the errors which arise from making these observations under dissimilar circumstances, and from drawing general conclusions from insufficient data."

The first and most positive effect of division of the pneumogastric nerves upon the respiratory movements is a decided *diminution in their frequency.* The number of inspirations soon after the operation was, in the majority of the experiments, reduced about one half. In some cases the number of respirations was afterwards still further reduced, while in others it varied at different periods; and in a few it became more frequent shortly before death. The movements themselves are performed more slowly, in a prolonged and heaving manner, so as to give rise to the idea entertained by some experimenters, and by Dr. R. himself in the first instance, that they are more numerous. It is sufficiently evident, from these experiments, that the vagi are important nerves in transmitting those impressions to the medulla oblongata which excite the respiratory muscular movements. Dr. R. then goes on to prove, by experiments, that Dr. Cruveilhier and Dr. M. Hall are incorrect in asserting that respiration will not go on if the cerebrum and cerebellum are destroyed and the pneumogastrics divided; a contrary result having been obtained by him. The removal of the encephalon diminishes the frequency of these movements, however, whether before or after the section of the vagi. In kittens of a day old they were at first about 100 per minute; after the destruction of the brain and cerebellum they fell to 40; and on cutting the pneumogastrics they instantly fell to between 3 and 4 in the minute, and continued so for some time. It is thus proved that, although the pneumogastric nerves are the *principal*, they are not the *sole* excitors of the respiratory movements; and that there are others which act through a channel in which the encephalon is not concerned. In one of the experiments the spinal cord was divided high up in the neck after the removal of the brain and the division of the pneumogastrics. Respiratory movements were still performed, though with diminished frequency. This fact seems to confirm the doctrine of Dr. M. Hall, relative to the function of the cutaneous nerves, especially those of the face, as excitors of respiration. It is not certain, however, how much of the effect is to be attributed to the sympathetic, which, as has been recently proved by microscopic examination, contains many filaments of the cerebro-spinal system. The diminution in the frequency of the respiratory movements subsequent to division of the vagi has been noticed by other observers; but its constancy has not previously been shown, nor has it been pointed out that it *instantly* supervenes (unless the animal be excited by struggling), and thus cannot result from the circulation of dark blood through the nervous centres.

The next question discussed is that of the *motor* character of the pulmonary branches of the vagus. Regarding this Dr. Reid has not been able to obtain any positive evidence on either side. Certain pathological phenomena would lead to the conclusion that these nerves are capable of exciting muscular contraction in the bronchial tubes; but experiment does not bear out this supposition, though it does not forbid it. The affirmative results obtained by M. Brachet are not regarded by Dr. R. as worthy of confidence. With regard to the *sensory* functions of these branches, Dr. R. is of opinion that, although they are concerned in producing the sensation of "le besoin de respirer," this is *not* annihilated after their section, since animals will even then exhibit great uneasiness if the access of air to their lungs be prevented. The fallacy of the contrary results obtained by Brachet and Mr. Grainger is pointed out. We would suggest that this uneasiness may be produced by impressions transmitted from other parts of the system as well as the lungs, when imperfectly arterialized blood is circulating through the systemic capillaries, just as the sense of hunger seems to depend not only upon the emptiness of the stomach, but upon the demand for nutrition existing in the system at large. The sensibility of the mucous membrane of the trachea and bronchi appears to be impaired by section of the vagi; but it is always much inferior to that of the larynx; and the experiments made to test it are liable to a remarkable fallacy, arising from the fact that the air-passages are easily brought to tolerate stimulants which at first excite great uneasiness.

Morbid Changes in the Lungs consequent upon section of the pneumogastrics. The real character of these and the mode in which they are induced constitute a very interesting subject of investigation; and one whose importance in pathology is at once apparent. We are inclined to place the more reliance upon Dr. Reid's statements, as his office as Pathological Clerk at the Royal Infirmary of Edinburgh gives him peculiar opportunities of becoming conversant with morbid appearances, and induces accuracy in the statement of them. In fifteen out of seventeen animals experimented on, the lungs were found more or less unfit for the healthy performance of their functions. Of the two animals in which the lungs were found healthy, one died after the completion of the eighth day, apparently from inanition; the other was killed after the twelfth day, when it was apparently in perfect health. "The most common morbid changes found in the lungs of these animals was a congested state of the blood-vessels, and the effusion of frothy serum into the air-cells and bronchial tubes. In eight out of the fifteen these changes were strongly marked. In some portions of the lungs the quantity of blood was so great as to render them dense. The degree of congestion varied in different parts of the same lung, but was generally greatest at the most depending portions. The condensation was generally greater than what could be accounted for by the mere congestion of blood in the vessels, and probably arose from the escape of the solid parts of the blood into the tissue of the lung." In some instances the condensation was so great that a considerable portion of the lungs sunk in water and did not crepitate; but they did not present the granulated appearance of the second stage of ordinary pneumonia. In five cases in which the animals had survived a considerable time, portions of the lungs exhibited the second and even the third stages of pneumonia, with puriform effusion in the smaller bronchial tubes; and in two gangrene had supervened. "One of the most important points to ascertain in an investigation of this kind is the first departure from the healthy state; to decide whether the effusion of frothy reddish serum, by interfering with the usual changes of blood in the lungs, *causes* the congested state of the pulmonary blood-vessels and the laboured respiration; or whether this effusion is the *effect* of a previously congested state of the blood-vessels." In several of the experiments but a very small quantity of frothy serum was found in the air-tubes, even when the lungs were found loaded with blood, and when the respiration before death was very laboured. "This naturally leads us to doubt whether the frothy serum is the cause of the laboured respiration and the congested state of the pulmonary vessels in those cases where it is present, though there can be no doubt that, when once it is effused, it must powerfully tend to increase the difficulty of the respiration, and the impeded circulation through the lungs." Dr. R. has satisfied himself of an important point which has been overlooked by others, "that this frothy fluid is not mucus, though it is occasionally mixed with it, but is the frothy serum so frequently found in cases where the circulation through the lungs has been impeded for some time before death. From this and other facts Dr. R. concludes "that the congestion of the blood-vessels is the first departure from the healthy state of the lung, and that the effusion of frothy serum is a subsequent effect." What then is the cause of this congestion and of the retardation of the movement of the blood? We fully agree with Dr. R. in considering the diminished frequency of the respiratory movements, and the consequent check to the aërating changes as sufficient to account for it, since "it is an established fact that the flow of the blood through the lungs is dependent upon the continuance of the respiratory process." It has been supposed by some to be due to paralysis of the muscular fibres of the bronchial tubes; after every endeavour to obtain distinct evidence of this, however, he has been unsuccessful, though he does not deny the possibility of such an influence. Several cases of disease in man are mentioned by him, in which the lungs presented after death a condition similar to that observed in the lower animals after section of the vagi; and in these the respiratory movements had been much less frequent than natural during the latter part of life, owing to a torpid condition of the nervous centres. He controverts the opinion of Wilson Philip that the effusion is the result of a morbid secretion immediately consequent upon interruption of nervous influence, and points out how galvanism may operate in preventing it. The true mucus of the bronchial membrane has always been found, except when inflammation was present. The congestion of the vessels sufficiently accounts not only for the effusion of serum, but also for the tendency to pass into the inflammatory condition presented by the lungs as by other organs similarly affected. We are disposed to think, however, that this is not the whole truth, but that the interruption of innervation here, as in other cases, predisposes to inflammation, although it does not actually produce it. "The experimental history of the par vagum," remarks Dr. Reid, "furnishes an excellent illustration of the numerous difficulties with which the physiologist has to contend, from the impossibility of insulating any individual organ from its mutual actions and reactions, when he wishes to examine the order and dependence of its phenomena." Nothing, indeed, can be more various than the statements made by different experimenters of the results of their enquiries, of which an instructive summary is given by Dr. R. These differences, no doubt, arise from varieties in the conditions of the experiment; and the number of cases related in this memoir has enabled the author to point out several of these varieties, which had been unsuspected by his predecessors, whose experience was more limited. These are investigations in which no useful inference can be drawn from one or two experiments only; to avoid all sources of fallacy a large number must be made, and the points in which all agree separated from others on which there is a variation of results; and it must then be enquired to what the latter is due.

These observations apply equally to the other question which has been made by Dr. Reid a special subject of investigation—the function of the *gastric branches* of the vagus nerve—on which there has been a difference of opinion no less remarkable than on that just discussed. The experiments of Dr. Reid do not furnish grounds for positive conclusions on the subject; but they furnish important corrections of the results presented by others. He has frequently succeeded in exciting muscular contractions in the stomach by irritation of the nerve; but he has also frequently failed. It may be questioned, however, whether these movements are produced by the direct influence of the nerves, or whether they are excited by the contractions of the œsophageal filaments, which are unquestionably stimulated through the nerves. When these movements of the stomach are thus artificially induced, they commence at the cardiac orifice, and propagate themselves over a greater or less extent of the left portion of the viscus; they are evidently slower, more prolonged, and more vermicular than those of the œsophagus. These movements of the stomach, however, may take place after the section of the vagi, as is proved by some experiments presently to be mentioned, in which the food had been digested and propelled into the intestines, subsequently to this operation. "That lesion of the vagi is generally followed by vomiting (in those animals susceptible of it), loathing of food, and arrestment of the digestive process, has been incontrovertibly proved by numerous experimenters. That perfect digestion may occasionally take place after division of the vagi in the neck, even when the cut ends of the nerves are kept apart from each other, we are also fully convinced. In four of the seventeen animals experimented on for the purpose of examining the morbid changes in the lungs, we obtained sufficient evidence of the restoration of the digestive process." This evidence consisted in the acquisition of flesh and strength by the animal, which had at first suffered much from the effects of the operation; the vomiting of half-digested food, permanently reddening litmus paper; the disappearance of a considerable quantity of alimentary matter from the intestinal canal, and the existence of chyle in the lacteals. In all these cases it is to be observed that the digestive powers did not appear reestablished until the fifth day after the operation; having been in these animals, as in those which died before that period, completely arrested in the first instance. This fact accounts for the contrary results obtained by other experimenters; as it is only when circumstances favour prolonged life that such restoration can be manifested. Dr. Reid states that seven of the seventeen experiments were performed before he obtained any evidence of digestion; and that the four which furnished this followed one another almost in succession, so that it is easy to explain how some experimenters have been more successful than others when the number of their experiments is limited.

Dr. Reid next considers the effects of lesion of the vagi upon the *sensations of hunger and satiety*, and shows that these are not abolished by section of these nerves, as Brachet and others have maintained, though very probably impaired. From this and other facts he is led to the conclusion that these sensations, though referred to the stomach as their seat, originate in impressions made upon the nerves of the system at large, and have reference rather to its requirements than to the fulness or emptiness of the digestive cavities. He then adverts to the fact that the *secretion of gastric juice*, of the true acid character and solvent powers, is not always checked by section of the nerves; and he quotes the statement of Müller, that where it does appear to cease, the application of galvanism has not succeeded in reestablishing it. We hope, therefore, that physiologists will ere long be tired of quoting the oft-cited experiments of Dr. W. Philip in proof of the *dependence* of this secretion upon nervous influence, and the identity of the latter with galvanic electricity. The usual mucous secretions, too, were always found; and the inflamed appearance of the mucous membrane of the stomach described by Gendrin was never noticed. Another series of experiments was performed by Dr. Reid, with the view of testing the validity of the results obtained by Sir B. Brodie, relative to the effects of section of the nerves upon the secretions of the stomach, after the introduction of arsenious acid into the system. With this view he made five sets of comparative experiments, employing in each two dogs as nearly as possible of equal size and strength, introducing the same quantity of the poison into the system of each in the same manner, but cutting the vagi in one, and leaving them entire in the other. This *comparative* mode of experimenting is obviously the only one admissible in such an investigation. Its result was in every instance opposed to the statements of Sir B. Brodie; the quantity of the mucous and watery secretions of the stomach being nearly the same in each pair of animals subjected to experiment; so that they can no longer be referred to the influence of the eighth pair of nerves. Moreover, the appearances of inflammation were, in four out of the five cases, greatest in the animals whose vagi were left entire; and this seemed to be referrible to the longer duration of their lives after the arsenic had been introduced. The results of Sir B. Brodie's experiments may perhaps be explained by the speedy occurrence of death in the subjects of them, consequent, it may be, upon the want of sufficiently free respiration.

The following is Dr. Reid's general conclusion from his experiments on the influence of the eighth pair upon the secretive processes of the stomach, which, we think, gives a peculiarly judicious view of the nature of nervous influence upon these processes in general. "These experiments ought to be viewed in two different aspects; for while they show that the integrity of those nerves is not a condition absolutely necessary for the performance of secretion in the stomach, they yet prove that the secretions usually poured into the interior of that organ may be modified, in a most important manner, by causes acting through these nerves. We have here two perfectly separate and distinct propositions, which have sometimes not been clearly distinguished from one another. The difference between them may be illustrated in a very simple manner. The movements of a horse are independent of the rider on his back—or, in other words, the rider does not furnish the conditions necessary for the movements of the horse—but every one knows how much these movements may be influenced by the hand and heel of the rider." "When we extend our investigation into the manner in which the function of secretion is performed to the whole range of the vegetable and animal kingdoms, we can, I think, have little difficulty in arriving at the conclusion that the nerves exercise over secretion not an uniform and essential, but an occasional and controlling influence."

The *cause of death* after section of the vagi may usually, therefore, be looked for in the changes which take place in the lungs; occasionally, however, it results from inanition, produced by the derangement of the digestive organs. It is of course understood that means have been taken to ensure the access of a sufficient quantity of air into the lungs.

The following is a summary of the results obtained by Dr. Reid, as to the functions of the different portions of the pneumogastric nerve.

The *pharyngeal branches* are entirely or almost entirely motor, and move the muscles of the pharynx and soft palate.

Of the *laryngeal* branches, the superior are almost entirely sensory, supplying the mucous surface of the larynx and part of the pharynx. The few motor filaments they contain are distributed to the cricothyroid muscles. The inferior laryngeals are principally motor, and regulate the movements of the muscles attached to the arytenoid cartilages; they also furnish sensory filaments to the trachea, and a few to the larynx and pharynx. When an irritation applied to the mucous surface of the larynx produces contraction of the muscles which approximate to the arytenoid cartilages, it is by a reflex action through the spinal cord, in which the superior laryngeals probably act as the afferent nerves, and the inferior as the efferent or motor.

The *œsophageal* filaments are partly afferent and partly motor, the contact of food producing muscular contraction by reflexed stimulation. The degree in which this mode of action extends down the œsophagus seems different, however, in different species of animals.

The *cardiac* branches do not seem necessary to the sympathy between the heart's actions and mental emotions, although probably the usual channel of it.

The *pulmonary* branches seem to be the nerves chiefly concerned in transmitting to the medulla oblongata the impressions which excite respiratory movements, and are thus principally afferent nerves. It is possible that they contain motor filaments also.

The *gastric* branches appear to influence both the muscular movements of the stomach and secretions poured from its inner surface; but neither of these changes are absolutely dependent upon their constant influence.

Edinburgh Med. and Surg. Journal. April, 1838.

Experiments on the Blood, in connexion with the Theory of Respiration.

By John Davy, M D. F.R.S., Assistant Inspector of Army Hospitals.

In a former Number we gave a brief notice of Dr. Davy's paper, extracted from the Proceedings of the Royal Society. Since that period the paper itself has been published in the Transactions, and we now design to give a fuller account of it, in consequence of the importance of the subjects discussed in it, in reference to the theory of animal heat.

Dr. Davy's first enquiry was to ascertain whether the blood agitated with oxygen, or with atmospheric air, is capable of absorbing either of these two gases, without entering into a state of putrefaction? The blood experimented upon was from different animals—man, the ox, sheep, dog, and cat, and in every instance was first prepared by the displacement of its fibrine, which was done by agitating it in a bottle with small pieces of sheet-lead. Whether oxygen or atmospheric air was used, there was a marked diminution in the volume of gas; but when the former was employed, the quantity absorbed was by far the greatest. Thus "sixty-two measures of arterial blood from the carotid artery, agitated with thirty-three measures of common air, produced a diminution of two measures; and sixty-three of the same arterial blood, agitated with thirteen of pure oxygen, a diminution of three; whilst sixty-three of venous blood, from the jugular vein of the same animal, agitated with thirty-three of common air, produced a diminution of six, and seventy of this blood with thirteen of oxygen a diminution of eight." It thus appeared that the blood is capable of absorbing gases, and that the quantity absorbed by venous blood is very much greater than by arterial. In every instance, both of the absorption of oxygen and of atmospheric air, the colour of the blood was changed, the venous blood became of a vermilion hue, and the colour of the arterial was rendered brighter. The experiments quoted were all made upon the blood of the same animal; but in the experiments upon the blood of different animals, the degree of absorption varied considerably, and this was more particularly the case with the blood of man. In every instance, however, absorption to a certain amount occurred. Upon this subject Dr. Davy formerly arrived at a negative conclusion; and he accounts for the discrepancy between his present and former experiments by supposing, that it has arisen chiefly from the difference of season at which the two sets of experiments were made; the first having been made during the summer at Malta, in July and August, when the temperature of the atmosphere was from eighty to ninety degrees, while the latter was made in England during the winter, in very severe weather, when the greater part of the time the temperature of the air was below the freezing point; hence he is induced to infer that the temperature of the atmosphere has a great deal to do with the capability which the blood possesses of absorbing gases, and thinks from the result of his experiments, made both at a high and low temperature, that the blood has a less power of absorbing gases at a high than at a low one. If this be the case, and if animal heat be chiefly derived from the changes which take place in the blood through the agency of respiration, as it is now very generally considered to be, and as Dr. Davy's experiments presently to be noticed seem further to show, this difference in the absorbing power of the blood at a high and low temperature may fairly be looked upon as one of the chief means of preserving that equability of bodily heat observed in most of the higher animals in winter and summer. In support of this Dr. Davy found, on carefully comparing the blood of the jugular vein and carotid artery of the sheep at Malta, during the summer, that there was no perceptible difference in their colour; in each it was less florid than the arterial blood of the same animal in England in winter, and less dark than the venous, being, as it were, intermediate between the two. Another fact in support of the same view is that he found the temperature of the right ventricle of the heart as high as 109° F. in one instance; and, in the mean of nine experiments, 107·5, whilst that of the rectum, the mean of the same number of experiments was only 104·4° F. On the question as to whether blood contains carbonic acid, which is capable of being expelled by agitation with another gas, as hydrogen or oxygen, Dr. Davy has arrived at a negative conclusion, and unfavorable to the opinion of Dr. Stevens, "at least," as Dr. D. expresses himself, "in a strict and general sense," since in one of his experiments a very small quantity was expelled, but not sufficient to warrant the conclusion that this is usually the case when blood charged with carbonic acid gas is agitated with either of the other two gases. With respect to the condition of the alkali in the blood as regards carbonic acid, the experiments all led to the conclusion, that it is in the state of sesqui-carbonate. On the question, whether blood contains any air capable of being extracted by the air-pump, Dr. Davy again arrives at a conclusion different from that at which he arrived on the same subject in 1818. The presence of pure gas in the blood, extractable by the air-pump, was stated at that time by Sir Everard Home upon the authority of Mr. Brande, but the fact was not then confirmed; on the contrary, many celebrated physiologists and chemists arrived at a different conclusion, until the experiments of MM. Bischoff and Magnus. Mr. Brande had found that the quantity of gas amounted to as much as two cubic inches from one ounce of blood. This enormous quantity was so remarkable, that it was suspected there must have been some mistake; but the recent experiments of MM. Bischoff and Magnus, confirmed by Dr. Davy, render it most probable that Mr. Brande was correct, and that the negative results formerly obtained by Dr. Davy and others arose from the exhaustion of the air in the receiver not being carried sufficiently far. Indeed, Dr. Davy's present experiments almost prove this to have been the case, since it is only when the exhaustion is nearly or quite complete that the gas is evolved, and the evolution then takes place so rapidly and continuously, that the fact is indisputable. In all the experiments the blood operated upon has been obtained from animals without being allowed to come at all in contact with atmospheric air. In one set of experiments the jugular vein or carotid artery was exposed, two ligatures placed upon it, and then cut through, and the cut end of one connected with a phial filled with boiled distilled water, and the blood then allowed to flow until the water was believed to have been all dispersed; in another experiment, the blood was allowed to coagulate in the jugular vein between two ligatures, and the results in this instance were extremely interesting. At first there was no more air evolved under the air-pump than might fairly have been supposed to have adhered to the stopper of the bottle in which the coagulum was inclosed, after which there was no evolution for about five minutes, which thus led to the opinion that no gas was contained in the blood. At the expiration of that time, the exhaustion in the receiver being nearly complete, a film or bubble burst with considerable violence, and a bubbling commenced which was continued with some activity, thus rendering the fact of the extrication of gas indisputable. The quantity of gas extricated in the different experiments was exceedingly variable, so that Dr. Davy concludes that the quantity contained in the blood of different individuals, and in different states of health, is subject to much variation. No disengagement of air from the blood could be obtained while the blood was confined in a portion of the vessel between ligatures, so that a very slight compression appears to be sufficient to confine the air in the blood, or prevent it from assuming the elastic state. Dr. Davy differs from Magnus in his opinion respecting the composition of the gas extricated from the blood by means of the air-pump, in believing it to be entirely carbonic acid, and that there is neither oxygen nor azote, and this opinion he founds upon the fact that when potassa was mixed with the blood, no gas whatever was extricated, the whole being, as he supposes, absorbed by the potassa, which is not capable of absorbing either oxygen or azote. But on the question as to whether oxygen is contained in the blood, but which cannot be extricated by means of the air-pump, Dr. Davy was led to the affirmative. Both oxygen gas and nitrous gas are capable of combining with the blood, but cannot afterwards be separated from it again by means of the air-pump. From the circumstance that Dr. Davy had not employed so complete an apparatus in his experiments as was employed by Magnus, it is reasonable to expect that his results might not be so conclusive, and that the attraction of oxygen for the blood is greater than that of carbonic acid; consequently, that it is liberated with greater difficulty, and perhaps not until exhaustion in the receiver is complete; and this may be the case also with regard to azote. The important question therefore yet remains, does the blood contain oxygen or azote in a free state? If it be denied that azote enters into combination with, or is contained in a free state in the blood, from whence is derived the azote which constitutes so large a proportion of the primary constituents of animal tissues, particularly in those animals which subsist entirely upon food which contains not a particle of azote?—the graminivorous feeders—sheep, oxen, &c. This is a question often started, but still worthy of further enquiry. Another important fact examined by Dr. Davy is, whether there is any evolution of heat from blood when agitated with oxygen. Dr. Davy has distinctly proved this to be the case, care having been taken to ensure a correct result.

On the whole we think this paper of Dr. Davy's one that is deserving of considerable notice, as tending to throw much additional light upon a very difficult and most important subject. *Phil. Trans.* Part II. 183

Observations on the Blood after Death. By John Davy, M.D. F.R.S., Assistant-Inspector of Army Hospitals.

This is an important communication, of which we can only give an outline. The following table notices the state of the blood in persons dying of various diseases on the island of Malta, from 1828 to 1835.

Total number of fatal cases examined - - - - -	249
Heart collapsed, containing either no blood, or very minute quantities -	6
Blood liquid, and not coagulating on exposure to the air - - -	9
Partly liquid, partly coagulated, former not coagulating on exposure -	3
Partly liquid, partly coagulated, former coagulating on exposure .	6
Liquid and coagulating on exposure - - - - -	4
Coagulated and containing fibrinous concretions - - - -	115
Coagulated without fibrinous concretions, the coagulum soft - -	10
Grumous, not distinctly coagulated, without fibrinous concretions - -	2
Liquid and frothy, without any fibrinous concretions - - .	1
Blood florid - - - - - - - - - -	1
Fibrinous concretions without any cruor. - - - - - -	1
Blood coagulated in both ventricles and broken up - - -	3
Coagulated and broken up in left ventricle, not in right - - -	11
Coagulated in right ventricle, and broken up; left ventricle empty -	1
Cases in which the state of the blood was not noticed - - -	83

After some interesting remarks on these appearances, a second table is given of thirty-five cases, recently observed at Fort Pitt, in which attention was paid, not only to the state of the blood in relation to coagulation, but also to its state in relation to carbonic acid gas. The method employed was briefly the following. In every instance, after the removal of any fluid that might be found in the pericardium, the great vessels within it were divided; the blood which flowed out was collected and put into a bottle and secured with a glass-stopper, for subsequent observation and experiment. The cavities of the heart and their contents were next examined. Sometimes, the blood set apart, was seen again the same day, to ascertain, whether it had coagulated, or remained liquid; but more frequently, not until the following morning, when also commonly it was subjected to agitation in atmospheric air, included in a double-mouthed bottle, provided with stop-cocks, to one of which was attached a bent tube, for communication with a pneumatic trough.

The following are some of Dr. Davy's remarks on these appearances.

The instances in the first table were strongly indicative of the variable quality of the blood after death from disease; and the particular instances just given are as strongly corroborative of the same. The variable quality, as far as appreciable by the senses, seems to depend on the lymph, which, as regards coagulability, has appeared wonderfully little constant; in some instances, retaining its power of coagulation more than twenty-six hours after death; in others, coagulating almost immediately after the fatal event; and in some previous to that event. In a large number of cases, the lymph appears to have existed in the blood, possessed of the quality in question in different degrees. This is clearly to be inferred from the results of the *post mortem* inspections; namely, from finding at the same time, and in the same body, in the cavities of the heart, liquid coagulable cruor, soft crassamentum, and firm fibrinous concretions.

The fibrinous concretions, which are so frequently found in the heart after death, are far from being devoid of interest. They, too, are very various, both as regards form and consistence. They are also very various in relation to the manner in which they are attached to the heart. I have mentioned one instance, in which serum was collected in the interior of a mass; the inner part was soft, the outer surface firm. Some masses seem to have a certain regularity of structure. Some have the appearance of being inflamed, being blood-shot from entangled red particles of blood. Some exhibit the appearance of abscess, and contain, as it were in a sac, a puriform matter, generated by a peculiar process of softening. Generally, they tend to give the idea of the operation of an organizing principle, a *nisus formativus*, more or less active, until putrefaction commence, by which the animal matter is converted into common matter.

The different varieties of fibrinous concretions are admirably illustrative of certain morbid appearances, especially false membranes, bands of adhesion, the state of the cellular tissue in different kinds of œdema and induration; and, I may add, hepatization of the lung, with and without softening; and a similar state of the spleen, and perhaps tubercles and their softening. When I have examined, microscopically, the matter yielded by the lung which has been hepatized, undergoing softening, I have found it very similar to the matter into which a fibrinous concretion is converted, whether in the ventricles of the heart or in a vein, composed of particles of different sizes, most of them approaching the spherical in form, and some of them very like pus globules; and, I have found the same kind of matter in softened spleen, and also in softened tubercular matter.

No subject, probably, in pathology, is more deserving of careful investigation than lymph, in connexion with its varieties, supposing either that there are many different species of it, or that it is liable to sudden and great changes of quality. Relative to the former, take for instance the phlebolite, in its different stages of induration, from soft lymph becoming of cartilaginous firmness and appearance; the fibrinous layers deposited in an aneurismal sac, resisting changes, occasionally for years; and the fibrinous concretions which are formed in the large veins, especially in phthisis, which so soon soften and in part almost liquefy; how great is the diversity, how difficult in the present state of our knowledge the explanation.

Nor is the liquid cruor or the blood remaining either in part or altogether liquid after death, whether for a definite or indefinite time, without interest and importance, especially as regards pathological research, and the distinguishing between the effects of disease and *post mortem* effects, in the inspection of bodies. So long as the blood is liquid after death, it must necessarily observe the laws to which fluids are subject under similar circumstances; will accumulate in the dependent parts and where there is least resistance; and, should there be considerable pressure exerted, as from the disengagement of air in the stomach and intestines, it will appear as if injected in the organs removed from such pressure, whether included within unyielding parietes, as the brain, subject to occupy less volume from greater reduction of temperature after death than its hard, and, during life, cooler case; or contained in yielding envelopes, as the lungs in the pleuræ and other parts of the body generally, in the common integuments and in their peculiar capsules. In a considerable number of examples given in the table, cruor was found in the heart more than twenty-four hours after death. This cruor, in producing false appearances of inflammation and congestion, would act much in the same manner as liquid blood; and, in estimating the phenomena which are presented on dissection, much the same allowance should be made for it, or serious errors will be unavoidable.

From the experiments given in the table on the agitation in air of the blood collected after death, it would appear, that in the greater number of instances, gas was disengaged. In several instances, the air thus liberated was tried, and was found to be carbonic acid gas; proving that there was an excess of this acid in the blood. This excess, I am disposed to infer, especially from the results of experiments which I have recently made on healthy blood, an account of which is published in the Philosophical Transactions for the present year, is the effect of disease, and is particularly apt to take place in the act of dying, when the powers of secretion seem to be arrested, and when carbonic acid probably ceases to be eliminated in the lungs. In accordance with this, with very few exceptions, is the uniform dark colour of the blood usually observed after death, and equally in the left and in the right ventricle of the heart; and also, the little suffering commonly witnessed in dying, diminished sensation probably as in birth, or stupor, or coma, usually preceding and ushering in the fatal event.

From the results referred to, and from a few which I have obtained in operating on blood taken from persons labouring under disease, I am disposed to think that carbonic acid acts a very important part in the economy of life; and is connected when in excess, if not with the production of particular diseases, at least with their modification and progress and the production of certain symptoms, and that the careful examination of the blood, in relation to this acid, is an enquiry much wanted, and is likely to reward richly those who engage in it. At present, there is a great difference of opinion derived from experiment, respecting the carbonic acid in the blood; some enquirers, as Dr. Stevens, to whom belongs the merit of having opened this path of research, considering it, in the healthiest condition, always in excess and capable of being liberated from venous blood and even from arterial, by the physical agencies in play in respiration; whilst others take an opposite view, and deny that any free acid exists in the blood in its healthy normal state, or can be separated from it excepting by processes in which it is formed from its elements. This discrepancy, the enquiry I have mentioned, will probably explain or reconcile. It will elucidate also, probably, some other obscure points, which might be mentioned, especially were it extended to other fluids, as the urine and bile. I may mention, incidentally, that whenever I have obtained indications of the disengagement of carbonic acid from the blood after death, they have been afforded by the fluids spoken of, in a very limited number of trials, and also by any effused serous fluid collected at the same time, and subjected to the same test.

Edinburgh Med. and Surg. Journal. April, 1839.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS

Case of Ulceration of the Brain. By G. P. May, M.D., Maldon.

Daniel Prior, aged 15, thrown from a horse, Jan. 17. On being visited two hours after the accident, was found to have received an extensive lacerated wound of the scalp, across the right parietal surface, by which the bone was denuded to a great extent. Considerable hemorrhage from the wound took place, amounting to more than a pint in quantity. He retained perfect possession of his senses, and complained little of his head, but referred his sufferings to his elbow-joint, which appeared to have received a violent contusion. On the fourth day after the injury he was able to come down stairs and exercise himself in the open air; by this period, a great part of the wound had healed by the first intention. For three weeks everything went on favorably, and the boy appeared to be the subject of little or no ailment. About the commencement of the fourth week symptoms of constitutional irritation began to manifest themselves; the pulse became quick, and the discharge assumed, for the first time, an unhealthy character; this condition continued, without much alteration, for six days, during which time he complained of pain of head.

Feb. 16th, he became comatose, in which state he continued until his death, which took place the following day.

Sectio capitis.—The wound of the head had an unhealthy aspect. Around the denuded portion of bone, between the scalp and periosteum, was an accumulation of pus, of a very offensive character; a small piece of the bone was carious, and exhibited some dark-coloured lamellæ when broken up by the handle of the scalpel; this condition did not extend through the external table. On raising the calvarium, two or three drachms of fetid pus escaped. The dura mater and subjacent membranes of the right hemisphere were completely eroded in two places, one about the size of a shilling, situated immediately posterior to the Sylvian fissure, the other of smaller dimensions, nearer the occipital region; the ulcers extended about three lines into the substance of the brain; their bases were hard, their edges ragged and coated with yellowish matter; the texture of the cerebral mass around the ulcers was apparently normal; a great quantity of lymph and pus was effused over the anterior lobe beneath the arachnoid membrane. The pia mater was highly injected, and the vessels of the convolutions and the sinuses were much distended with blood. The whole of the left hemisphere was very vascular; the corpus callosum, septum, and fornix, were in an advanced stage of softening; an excavation, the size of a pigeon's egg, was discovered in the posterior lobe, filled with fetid matter. The appearance of the cerebellum, left hemisphere, and ventricles, was natural. The thoracic and abdominal cavities were not examined.

This case is very interesting, as illustrative of the ambiguity which so frequently invests diseases of the encephalon. Every tissue here appeared to be the seat of some morbid action. The membranes bore evidence of acute and extensive inflammation. The cortical substance was, in two places, eroded to some depth; and the internal parts of the brain exhibited that alteration of structure most usually considered to be the result of inflammatory action; but until within twenty-four hours of death, there was scarcely a symptom diagnostic of any of these lesions. Up to this period the patient retained perfect possession of all his faculties, mental and physical; there was no delirium, paralysis, convulsion, or contraction of the limbs, which has been regarded by some French pathologists as indicative of cerebral ramollisement. These, with most of the conditions symptomatic of cerebral and meningeal inflammation, were altogether absent. Dr. Abercrombie, in his valuable treatise, has detailed some cases similar in this particular, and represents "the danger of being guided by system in our diagnosis of affections of the brain, and the necessity there still is for extensive and careful observation of facts in regard to this class of diseases." *Lancet. April* 13, 1839.

On the Bimeconate of Morphia, a new preparation of Opium.

By Mr. P. Squire, Chemist to the Queen.

In reflecting on the artificial salts of opium, and the powers of opium itself, I was led to suppose that the natural salts existing in this drug would be the best therapeutic agent, could it be separated from the other ingredients, which have often been found obnoxious to its soothing properties. I therefore instituted a series of experiments with a view to accomplish this task, and have obtained the *bimeconate of morphia* in a form which appears to have succeeded in a most satisfactory manner as the subjoined reports will show; and should the same success attend its employment generally by the profession, I shall feel a high gratification in having enhanced the value of a medicine which is so justly esteemed for its efficacy in relieving the sufferings of humanity.

From RODERICK MACLEOD, M.D.

I have now used the solution of the meconate of morphia in cases sufficiently numerous to enable me to form an opinion with respect to its powers.

It appears to me to be a very mild and efficient preparation, rarely producing headach or other discomfort. It has repeatedly answered in the most satisfactory manner where opium has disagreed, and has succeeded in some cases where the other salts of morphia (the acetate and hydrochlorate) had failed to give relief.

From A. T. THOMSON, M.D.

I have given a fair trial to your new preparation of opium, the bimeconate of morphia, which, being separated from many of the other constituents of opium, undoubtedly possesses anodyne properties superior to any of the salts of morphia in ordinary use. I have not had many opportunities of administering it in the hospital, but where it has been given in painful affections, its influence has been most striking.

I have administered it in three cases, in private practice, well calculated to illustrate its properties. The first was a neuralgic pain of the left side of the face, extending from the temple to the molar teeth of the upper jaw. It remitted during the day, but never wholly subsided, and returned with frightful severity at night. Among other means which were employed, anodynes of various kinds were tried, both topically and internally administered, namely, extract of opium in combination with calomel, hydrochlorate of morphia, acetate of morphia, Battley's sedative, and the black drop; belladonna in small and frequently-repeated doses, and in the form of plaster; hyoscyamus, both tincture and extract, and the extract of aconite. Temporary ease was obtained from the salts of morphia and the extract of aconite, but no permanent advantage resulted, although the pain assumed more of an intermittent character, and consequently disulphate of quina and arsenical solution had been freely administered. The strength of the patient was truly worn out for want of sleep, and under these circumstances your solution was prescribed. Thirty minims were prescribed in a fluid ounce and a half of camphor mixture, at bedtime, and two hours' sound sleep were procured, and on awaking, the patient felt the most satisfactory abatement of her sufferings. She was ordered to continue it in doses of eight minims, in combination with fifteen minims of the arsenical solution, every third hour during the day, and to repeat the full dose of thirty minims at bedtime, whilst, at the same time, the bowels were kept moderately lax. The most gratifying results followed this plan of proceeding; the pain gradually yielded; in less than a week she had comfortable nights, after which the dose of the bimeconate was rapidly diminished, its use discontinued during the day, and on the third week I had the satisfaction of leaving her perfectly free from the complaint.

The second case was one of wakefulness, without any apparent cause. All the usual preparations of opium had been tried without much benefit, and with suffering from headach and nausea on the mornings following the nights in which the narcotics were administered. The solution of bimeconate of morphia was given, in doses of twenty minims; it effectually procured sleep, and was not productive of the morning distress which had supervened the use of the other preparations of opium.

The third case was one of anomalous pain of the hip, extending down the thigh, which recurred three or four times in the twenty-four hours. The patient had been frequently attacked with rheumatism, and conceiving it to be connected with that disease, the part was cupped, and the guaiacum mixture, with small doses of blue pill, was prescribed. Little benefit resulted, until a week afterwards, when I ordered the part to be blistered, and the denuded surface to be dressed with a piece of lint dipped in your solution three times a day. The pain rapidly abated, and on the fourth day, it was completely gone.

From the limited experience which I have had of the use of the solution, I am of opinion that it possesses decided anodyne properties, and stimulates less than opium or its tincture, and is much more certain in its influence than any of the artificial salts, or other preparations of the drug.

From HENRY BRANDON, Esq.

I have much satisfaction in complying with your request to furnish you with an account of the effect which the meconate of morphia (same strength as laudanum) has had upon me, in comparison with the other preparations of opium that I have taken. My experience as an individual may probably be interesting, as well as somewhat useful. I have been the martyr to a spasmodic affection of the muscles for upwards of fourteen years, and was obliged, after trying every other means in vain, to have recourse to opium. I have taken crude opium, laudanum, aqueous extract of opium, black drop, liquor opii sedativus, and the acetate of morphia, and I must declare that not one of them has succeeded so well as the meconate of morphia, which relieves so much sooner, and without disturbing the stomach, leaving the system altogether in a more natural state of repose.

My experience has taught me that anodynes ought to be taken in solution, having found that when taken in the solid form they irritate by producing qualms in the stomach, frequently amounting to nausea. The aqueous extract does this less than the crude, but they are much more free from the exciting quality when in solution, and the solutions themselves, the purer and more free they are from the resin and other impurities, the less do they produce that unpleasant effect. Whether there be any additional cause why your meconate of morphia relieves so much quicker and strongly than others, you yourself best know.

My firm belief is that the qualms or unpleasant sensations in the stomach interfere materially with the soothing properties of the opium, and finding your preparation so free from these objections, added to the opium taste remaining a much shorter time on the palate than is the case with the other preparations of opium, I am induced to continue the use of it. The greatest quantity of opium that I ever took was in June, 1838, at which time I was suffering under a dreadful attack, when in seven consecutive days, I took as much as 540 grains of the aqueous extract of opium, which is equal to 810 grains of the crude.

I have taken ten grains of the acetate of morphia at a dose. My total consumption of opium for the year 1838 was equal to 49 pints 1 oz. of laudanum, and upwards. It is singular that none of the preparations of opium have ever confined my bowels or injured my appetite, although I take no exercise; indeed, a large dose of opium invariably increases my desire for food, and sometimes to a ravenous degree. *Lancet. April* 6, 1839.

On Irritative or Pseudo-Fever, produced by the ingestion of warm fluids.

By NATHANIEL RUMSEY, M.D., of Beaconsfield.

MANY years ago I was myself the subject of typhus fever. Having passed through the disease, my debility was great, and convalescence interrupted by what appeared to be the daily occurrence of symptomatic fever; the cause not obvious, but suspected to be a pulmonary affection. After a great length of time, an unfavorable prognosis, and groundless anxiety in my friends, I recovered. The regular depression of bodily and mental power, which began about eleven o'clock A.M. daily, the heat of skin, quick pulse, and sweating, gradually left me. No confirmation was, by the termination, added to the opinion that local disease had existed; and the correct understanding of the case, to use the principle of the writer on education, was reserved for a future day. It became apparent to me at the distance of many years afterwards, made so by other facts, that these symptoms had been the effect of drinking daily for breakfast a large basin of warm milk, under the state of debility induced by the long-continued disease. I do not doubt that many similar facts may have occurred to me within the next few years, and as little understood; but in my subsequent experience, I have been brought to a confident belief that they are of no unfrequent occurrence, and that their cause is as clearly made out as it is important to be known.

These phenomena, which are, as it were, a mimicry of irritative fever, or that which is symptomatic of local disease, are particularly incident to patients reduced by loss of blood. I will therefore recite another case of recent occurrence, in a patient under that condition, to elucidate my meaning, and exhibit the importance of a correct view respecting it.

A lady having miscarried in the middle of the fourth month, suffered under repeated and alarming attacks of uterine hemorrhage for three weeks; in consequence, as it was supposed, and afterwards proved, of a retained placenta. I assisted in removing it at the end of that time, with much difficulty, and in separate portions, fearing much the consequences which might follow the unavoidable employment of the means used for this indispensable alternative. No symptoms but those of an exsanguined and weakened patient followed through the critical period of the next four days, after which, to the concern of my son, Mr. John Rumsey, who was in attendance, some circumstances arose which rendered him dissatisfied with the symptoms and progress of the patient. He reported that she had had, on several successive days, a paroxysm, marked by heat of skin, profuse sweating, great depression of all the powers of body and mind, a tongue thickly furred, slimy and dark in the centre, a pulse very feeble, with an increase of forty beats in a minute, and great despondency on her own part as to recovery. Hence, he expressed his fear that some source of irritation was still in existence (at that time a very probable thing), and augured unfavorably of the issue. Seeing her now, after some days' absence, a little enquiry led me to believe that this apparent fever was mere irritation, arising, as in the various other cases which I had seen, solely from receiving into the stomach a bulky, warm, and vapid fluid; and I was confirmed in this opinion by being able to trace, with the assistance of the patient, that the symptoms had, on one occasion, followed the use of two or three cups of tea, and at other times the liberal potation of warm chicken-broth. I was thus able, with great confidence and pleasure, to give a favorable prognosis.

I directed that evening that she should take a draught of good home-brewed ale, cold from the cask (Jan. 1 or 2), thus combining the exhibition of nourishment and a stimulus to the system with the application of cold to the stomach. The purpose was answered equal to my expectation. Animal food in small quantities and malt liquor were continued, and a careful disuse of the warm fluids observed. It is now several weeks since this change was made; no paroxysm has returned, and nothing has interrupted a daily improvement, the case having exhibited another instance of the peculiar affection I wish to describe.

I have seen this disturbance of the system usually assume the form of a paroxysm of fever, but on some occasions to consist of successive fits of fainting and hysteria; nevertheless coming on as a fit of fever, in point of time and duration.

If I might venture a conjecture as to the *modus operandi* of the warm fluids, I would suggest that the heart, morbidly sensible, responds too promptly to the stimulus of heat, when introduced into its neighbouring organ—the stomach, in so bulky a form as in the cases quoted and referred to; but in its endeavours to obey, incommoded by the pressure of bulk, and incompetent to a normal action by debility, makes its embarrassed struggles, disturbing the whole system by the failure and perversion of its function.

Nothing can be more simple than the indication of cure. Nourishment without bulk, stimulus regulated as to degree by judgment and discretion, not only without warmth, but actually cold internally; with rest, tonics, and attendance to warmth externally.

The importance of distinguishing the true character of these symptoms is not as the danger which they threaten, but as the danger which may be connected with an erroneous view; leading to a wrong prognosis, and the institution of remedies upon a wrong principle.

The previous debility or nervous temperament of the patient, the absence of satisfactory evidence of local disease, the effects of the concurrent use of warm fluids, and the febrile symptoms, often marked by unusual depression of spirits, may justly excite suspicions of the cause, and lead to a correct diagnosis; whilst nothing has served so powerfully to establish the view which I have taken as the subsidence of the symptoms upon the use of the cold tonic principle. The frequency of this condition has been such, that I fully believe its occurrence to the notice of any attentive observer having moderate opportunities could not be rare.

[The above communication is interesting and well worthy of attention; although we cannot consider the author's "conjecture as to the *modus operandi*" very satisfactory.] *Medical Gazette. May* 11, 1839.

Proceedings of the Pathological Society of Dublin.

1. *Pulmonary Calculus.* Mr. Crampton exhibited a mass of cretaceous matter, of stony hardness, taken from the lung of a phthisical patient; it was somewhat larger than a tennis-ball, and consisted of a series of spherical masses, which, when broken, presented the appearance of concentric laminæ; its composition was ascertained to be almost entirely carbonate of lime.

2. *Ileo-cæcal Abscess, with Perforation of the Intestine and Groin.*—Mr. Ferrall presented the recent parts in this case. The patient, a young girl, was admitted into the Meath Hospital, with tumour in the right iliac region, about fourteen days after the first attack; suppuration of the tumour had then occurred; the bursting of the abscess was soon indicated by a copious discharge of purulent matter from the bowels; soon after this another tumour formed in the upper part of the thigh, separated from the former by a deep sulcus corresponding to Poupart's ligament, below which an opening occurred, through which pus and ultimately fecal matter was discharged. Mr. Ferrall exhibited the mode of communication between the abscess and opening in the groin; the fistula took a direction at first downwards, and afterwards upwards and inwards, the omentum adhered to the parietes of the abdomen and cæcum; the communication from the abscess into the intestine was by two small openings separated by a slip of mucous membrane, and resembling the appearance often seen in the integuments when an abscess opens by a slough.

An important peculiarity in this case was the mode in which the matter had made its way externally, namely, by perforation of the iliac fascia, and descent on the outside of the femoral vessels.

Mr. Ferrall also showed that in this case the communication with the intestine did not, as Dr. Burne supposes, take place through the appendix vermiformis, the appendix being free from disease. The perforation had taken place from the abscess into the intestine, being the third form of the disease formerly described by Mr. Ferrall in the Edinburgh Journal.

3. *Separation and Discharge of the superior Epiphysis of the Os Femoris.* Dr. Carlisle exhibited the head of the femur, which had been separated from the shaft of the bone in consequence of scrofulous disease of the hip-joint. The patient, from whom the specimen was procured, was about twenty years of age, and of a scrofulous habit. At the age of fourteen he had suffered from acute rheumatism, which was followed by the usual symptoms of morbus coxarius. Suppuration took place in the joint, and matter escaped by several external openings. In this state he continued for four years; at the expiration of which period the head of the os femoris presented at one of the openings, and was extracted; its removal was followed by the rapid amendment of the boy's health. During the above period, several other bones became similarly diseased, and a large sequestrum was discharged from the humerus. The affected lower extremity was six inches shorter than the opposite limb.

4. *Disease of the Mitral Valve, without Valvular Murmur.* Dr. Stokes presented a specimen of disease of the mitral valve. It was principally remarkable on account of the physical phenomena observed during life. In this case the patient had long laboured under symptoms of morbus cordis with chronic bronchitis; and when the first was seen by Dr. Stokes, had a distinct and rough bruit de soufflet with the first sound of the heart. The second sound was unaffected. He remained under observation for several months, and always presented the same

phenomena. In eighteen months he again came under Dr. Stokes's care, in a very advanced stage of dropsical disease. All bruit de soufflet had disappeared, and the heart merely presented the usual signs of hypertrophy. He died in about a month, during which time he was repeatedly examined, but no valvular murmur was ever detected; the heart was much enlarged; the left auricle greatly distended and thickened; the auriculo-ventricular valve was greatly diseased, and the orifice presented the semilunar contraction described by Mr. Adams; the opening was not more than two lines in breadth; numerous irregular deposits of earthy matter existed on the ventricular side of the valve. Dr. Stokes detailed the particulars of another case, in which a remarkable auricular contraction of the mitral valve had been found, in which no bruit de soufflet whatever had existed for a considerable time previous to death.

Dublin Journal of Medical Science. March and May, 1839.

On the Effects of Colchicum and Lytta used externally. By Thomas Laycock, Esq. House Surgeon of the York Hospital.

Some theoretical speculations led me to try the following liniment in rheumatism:—

℞ Tr. Rad. Colch.; Tr. Camph. aa. partes æquales. M.

The patient who used this was a tall groom (Richard Bould), under the care of Dr. Belcombe, subject to rheumatic attacks, and who at the time was unable to lift his arm, on account of rheumatism of the deltoid muscle. I was agreeably surprised to find that, after the third application, and within twelve hours after the first, he was able to raise his arm freely to his head. The relief was, however, only temporary, but the application was used with equal success so often as the pain recurred. The patient was subsequently attacked by small-pox (after vaccination), and nothing was heard of the rheumatic pains until he was convalescent, when they attacked his hip. He reminded me of the liniment, and one trial removed the pain. I now prescribed it for two or three out-patients, and these derived benefit. I then omitted the tincture of camphor, and I now find the groom is relieved with equal celerity and certainty by the tincture of colchicum-root alone. Relief so constantly follows its application in his case, that I cannot doubt its utility. When the loins are affected he cannot turn in bed unless the tincture be previously used. He rubs one or two tea-spoonsful on the part affected. I have found it equally successful in another case, in which the deltoid muscle was affected.

I believe it is well known that the tincture and powder of the meloe vesicatoria, or cantharis, is very useful in atony or paralysis of the bladder, especially of hysterical and aged people. I have found, however, that an emplastrum lyttæ applied to the loins is equally efficacious, and much more manageable. A female, confined to bed in the last stage of laryngeal phthisis, could not pass urine without raising herself upon her knees. She was at last too weak for the effort, and it became a question how the difficulty could be surmounted. I recommended an emplastrum lyttæ to be applied to the loins or sacrum, until she felt able to empty the bladder in the recumbent posture. In half an hour after the application she succeeded. She lived for three or four weeks subsequently, and the plaster was in almost daily use until she died. In most instances from one to two hours elapse before the desired effect is produced; in hysterical retention about the latter period. The plaster is useful in other cases. A man came to the hospital with a catheter in his bladder; he had not made water without it for three weeks. It was removed, and an emplastrum lyttæ applied to the sacrum for three or four hours; he never wanted the catheter again, and went away in a week quite well. I am not aware that this method of using the fly is mentioned by authors. *Med. Gaz. March* 16, 1839.

On Rheumatism from Copaiba. By A. B. Maddock, M.D., London.

[The following communication merits the attention of practitioners. The subject noticed in it demands and no doubt will obtain further investigation. Ther are few things in medicine more important than tracing *exactly* the causes of diseases.]

It is now some years since I expressed my belief that this complaint (rheumatism) generally originates from the administration of copaiba; and I take this opportunity of stating that further experience and observation have confirmed this opinion. I had, last February, under my care, a gentleman, aged 19, of a strumous habit of body, labouring under gonorrhœa, for which he was ordered, by a practitioner, a copaiba mixture, two days after taking which he was attacked by acute rheumatism in the knees and feet, which rendered him a complete cripple. He had been in this miserable state a fortnight, when he applied to me. I immediately ordered him to discontinue the copaiba, and substituted the iodide of potash and compound decoction of sarsaparilla; he was almost immediately relieved, and shortly cured. I will readily admit that copaiba is, in many cases, a most valuable medicine; but in persons of a scrofulous constitution I am convinced that it is a most pernicious excitant; and, if there be any disposition to rheumatism, will most certainly produce it. I am glad to find that my first communication attracted the attention of Dr. Sigmond, (vide *Lancet*, vol. ii., 1837-8, No. 24, p. 826,) and that his opinion coincides with my own. The potassæ iodidum is the medicine which I have employed, and have almost uniformly found it successful.

Lancet. May 25, 1839.

Researches on the State of the Heart and the Use of Wine in Typhus Fever. By WILLIAM STOKES, M.D., M.R.I.A., Honorary Fellow of the King and Queen's College of Physicians, &c.

[THIS is a paper of first-rate value, both in a pathological and therapeutic point of view; to which we call the attention of all our readers. The following extracts contain the more important observations, but the whole Essay ought to be read.]

If we compare the inexperienced man with him who has had a long-continued practice in fever, we may often observe that the former employs a too vigorous antiphlogistic treatment in the commencement of the disease, and delays the exhibition of stimulants until the powers of life are sunk too low, while the latter is much more cautious in husbanding the strength of his patient, and shows much less fear in resorting to wine and other stimulants. It is in determining on the use of wine in fever that the junior or inexperienced man feels the greatest difficulty; it is in its exhibition that he betrays the greatest uncertainty and fear.

The hospital physician will be frequently asked by students to state the principle on which he administers wine in fever. I conceive that the question may be thus answered. Typhus fever is a disease which has a tendency to a spontaneous and favorable termination, but one in the course of which the powers of life are attacked by a most malignant influence. By wine, food, and other stimulants, we support nature, until the struggle is past; so that, to use the words of an ancient author, which embody a more profound principle than appears at first sight, we "*cure the patient by preventing him from dying;*" that is to say, we prolong his existence until the natural and favorable termination of the disease arrives. We do not allow our patients to die of exhaustion; and bearing in mind the depressing influence they have to struggle with, we give stimulants at the proper time, and with a bold hand. We give our patients an artificial life, until the period arrives when nature and health resume their sway. Yet, though we may admire the practice of an experienced physician in the use of wine in fever, it will often be found that he has a difficulty in expressing any exact reason for adopting the practice in a particular case. His practice is founded on a knowledge which is often incommunicable, an almost instinctive perception of the necessity for stimulation, characteristic of the great physician, and only to be obtained by a long and close familiarity with the disease. But is there any rule by which *the inexperienced man* can be guided? any one distinct phenomenon, the observation of which is easy, and leading to an intelligible and communicable rule of practice?

Let us suppose a case of typhus on the tenth day of fever, and presenting severe symptoms of prostration, the pulse varying from 115 to 120. Wine is exhibited, and on the first day the pulse rises to 125, on the second to 130, and if on the third day there is no diminution, we may make a bad prognosis; and thus the following rule may be laid down, that when in a case where the symptoms seem to indicate wine, the pulse either does not come down, or increases in frequency under its influence, we may expect a bad result. These facts naturally lead to the examination of the state of the heart in typhus fever, and the cases in this report are so arranged as to exhibit together the condition of the heart, and the amount of wine employed. *In this investigation we have sought for an additional rule, drawn from the state of the heart itself, to guide the inexperienced man in the exhibition of wine*, and I am not without hopes, that in the careful study of the cardiac phenomena, an indication hitherto unobserved will be obtained. In typhus fever two opposite conditions of the heart may be observed; in the one the impulse becomes extremely feeble, or altogether wanting, while the sounds are greatly diminished in intensity; while in the other, the heart's action and sounds continue vigorous throughout the whole course of the disease. These opposite states are not necessarily revealed by the state of the pulse, or the warmth of the surface. We may observe a hot skin, while the action of the heart is almost imperceptible, and on the other hand a patient may be pulseless, cold, and livid for days together, while the heart is acting with the greatest vigour. The condition of the heart must be determined by the application of the hand and stethoscope to the infra-mammary and sternal regions.

[Then follow eighteen cases, minutely detailed, with the dissection of such as proved fatal.]

In these extracts [from Laennec and Louis] I have given all that has been discovered on the subject; no series of observations on the action of the heart in typhus fever has been published; I have commenced this enquiry, and have sought to derive some important indications of treatment from the existence of the phenomena now described. In the present state of the enquiry I wish it to be understood, that my observations are to be taken as referring principally to the epidemic of last year. Further researches must be made to establish how far they may be applicable to typhus in general; but I have little doubt, from studying the researches of Louis, and connecting the facts relative to the anatomical state of the heart, with those now observed as to its vital phenomena, that my observations will be found to have a very extensive application. The epidemic of last year was marked by all the signs of putridity. Dark-coloured and abundant petechiæ, sordes of the mouth, fœtor of the surface, extreme prostration, and stupor, were the prominent features of the disease; and in many cases bronchial and gastro-enteric irritation existed to a great degree. In many of the cases the bad symptoms were developed at an unusually early period, yet though recovery by crisis was by no means common, the convalescence was generally satisfactory, and the ultimate restoration to health complete. In several instances the disease was traceable to contagion. We may thus arrange the cardiac phenomena obtained in our typhus fever:—1. Impulse and sounds remaining unaltered; the action of the heart corresponding with that of the pulse. 2. Vigorous impulse, with distinct and proportionate sounds, with absence of pulse for many days. 3. Diminution of both sounds of the heart, with absence of great diminution of the impulse (fœtal character). 4. Diminution of the first sound; with cessation or great feebleness of the impulse. 5. Complete extinction of the first sound, the second remaining clear. 6. Predominance of the first sound, the second being extremely feeble. Of these the fourth and fifth were the most common. In the great majority of cases, however, the phenomena were as follow: I. Diminished impulse. II. Diminished first sound, particularly of the left cavities. With respect to the impulse we arrived at some unexpected results. In most cases, considered through the whole progress, the diminution and return of the first sound were accompanied with the diminution and return of the impulse. So far the phenomena were what we might expect. *But in some instances, at particular perious of the case, this accordance between the impulse and sound did not exist.* . . . It is difficult, or impossible, in the present stage of the enquiry, to offer any satisfactory explanation of these apparent anomalies; but it seems certain, that under the influence of the typhoid condition, the heart may have sufficient force to give an impulse with little or no sound, on the one hand; and on the other, its contractions may be accompanied by a sound, although the impulse be absent. Whether we are to explain these facts by referring to particular states of innervation of the heart, or to organic alteration in the muscular fibres, or their connecting cellular membrane, is still to be determined.

That the cause of the want of impulse, and feebleness or cessation of the first sound, is a softening of the heart, I have no doubt. The evidences in favour of this opinion may be thus stated:—I. That softening of the heart exists in typhus fever as a local disease, and without any analogous condition of the muscles of voluntary life. II. That in our dissections in the last epidemic, we meet with this softening of the heart, in cases which during life had presented the phenomena in question. III. That the physical signs indicate a debility of the left ventricle principally, and it is this position of the organ which is most often altered in consistence. IV. Laennec has stated, that in proportion to the severity of the putrescent phenomena, is the liability to softening of the heart. And the same observation is found to be true of the physical signs now described. If this softening of the heart be one of the secondary diseases of typhus, we should, as in the case of other lesions, observe something like periodicity in its phenomena. It should appear at a certain time, and decline after its proper period had expired. I have analyzed my cases with a view to these points, and the result is, that in most instances the signs of diminished impulse and first sound were developed at or about the sixth day, and the heart seemed again healthy at or about the fourteenth day. It is difficult to determine the period of the first development of the signs in many cases, as they existed on the admission of the patient, but still taking in these cases the dates of the disappearance of the signs, we get the following general results: Average date of appearance, sixth day. Average date of cessation, fourteenth day. One case has been excluded from this analysis; the patient was admitted on the tenth day, and the heart was not reported healthy till the twentieth. We thus get, as the duration of the phenomena, a period of about eight days. It is very probable, however, that the disease begins to be developed before the sixth, and that it subsides before the fourteenth day; for, as physical signs are our only means of detecting it, it is not likely that they would be well marked in its very first development, or indicate exactly the time of its subsidence.

I am decidedly of opinion that we cannot consider the softening of the heart in typhus as the result of carditis; it seems rather to be one of that class of affections not yet sufficiently examined, in which an infiltration of some peculiar substance takes place under the influence of the typhoid condition. This occurring in the heart seems to impair its functions to a great degree; but the rapid restoration of the heart to health points out that the disease has not materially impaired its organic condition. It is obvious that we can never meet with the affection in a very advanced condition, for death by syncope would occur after the contractility of the heart had been altered up to a certain point. Finally, I would draw the particular attention of my readers to the fact, that in the great majority of these cases the use of wine was followed by the happiest effects. I may safely refer to the cases in proof of this proposition; *and I believe that in the diminished impulse, and in the feebleness or extinction of the first sound, we have a new, direct, and important indication for the use of wine in typhus fever.* In some cases the existence of these phenomena at an early period of the disease, led us to anticipate the bad symptoms, and to commence in good time the use of the great remedy; and in others, notwithstanding the existence of severe visceral irritations, the use of stimulants has been adopted with the best success, from the same indication.

The following is the number of ounces of wine given in each of ten cases: 26, 36, 42, 60, 66, 88, 144, 156, 158, 170. "In no epidemic (says Dr. Stokes) did I ever before give so much wine. I never had such success in treatment."

The following are the conclusions deduced from Dr. Stokes from his investigations on this important subject: "1. That the condition of the heart in typhus fever must be determined by the application of the hand and stethoscope, the pulse being an uncertain guide. 2. That a diminished impulse, or a complete absence of impulse, occurs in certain cases of typhus fever. 3. That in such cases we may observe a diminished first sound, or even an absence of the first sound. 4. That both these characters may exist with a distinct pulse. 5. That although in most cases the diminution of the impulse and first sound coexists, yet that impulse may exist without corresponding first sound, and conversely, that the first sound may be heard although unaccompanied by impulse. 6. That these phenomena are most evident as connected with the left side of the heart. 7. That when the impulse and first sound are lessened or lost, the return to the healthy character is observed first over the right cavities. 8. That in some cases both sounds are equally diminished. 9. That in a few cases the first sound preponderates. 10. That these phenomena indicate a debilitated state of the heart. 11. That they may occur at an early period of the disease, and thus enable us accordingly to anticipate the symptoms of general debility. 12. That the existence of these phenomena, in a case of maculated adynamic fever, may be considered as pointing out a softened state of the heart. 13. That this softening of the heart seems to be one of the secondary local lesions of typhus. 14. That the diminution or cessation of impulse, the proportionate diminution of both sounds, or the preponderance of the second sound, are direct and nearly certain indications for the use of wine in fever.

Dublin Journal of Medical Science. March, 1839

SURGERY.

Case of Laceration of the Perineum cured by Operation. By Robt. Davidson, Esq

[This case is extremely creditable to Mr. Davidson. It may admit of question whether the aperient medicines ought not to have been administered earlier.]

The subject of this case was a lady about twenty-nine years of age: the accident took place in her second accouchement. The laceration of the perineum was complete from the vulva to the anus, and extended from half to three quarters of an inch into the recto-vaginal septum; the consequence of this was an involuntary discharge of fæces, to the great distress of the patient and her friends. Feeling much anxiety about the case, from the knowledge of the want of success which had so frequently attended the operation, I referred to the cases recorded on the subject.

The able memoir by Baron Roux, read before the Académie Royale des Sciences, in January, 1834, fell under my notice, and as the result, in several cases, had proved so very satisfactory, I determined on adopting the plan he pursued,—that of the quilled suture,—with a little variation, which I will presently describe.

Having explained my views to Dr. Henry Davies, whom I had the pleasure to meet in consultation, he perfectly agreed with me on the method I proposed to adopt; and although only twelve days had elapsed since the accident, the patient was so anxious to have the operation performed, that I did not hesitate to do it, having had the bowels previously well opened by means of a purgative and enemas. On the 6th November, in company with Dr. Henry Davies, I performed the operation in the following manner: I passed deeply a strong double ligature, by means of a common curved needle, close by the edge of the rectum, and another rather more than half an inch apart from the first, towards the vagina, after which I pared the edges of the wound, which I had not previously done, that I might not be annoyed by the oozing of blood, so as to be enabled to place the ligatures more accurately. The ligatures being introduced, I employed, as cylinders, two pieces of elastic gum catheter, about an inch and a half in length, one of which was placed in the loops which the double ligature formed on one side, and the other between their separate ends, tying them firmly upon the cylinders. Baron Roux found in his cases that the use of the quilled suture caused an eversion of the edges of the wound; to remedy this he had recourse to several small sutures, at different points, between the different ligatures. To effect the same object, and also with a view of keeping the divided parts more closely and firmly in contact, I adopted the following plan, the materials for which I had prepared previous to the operation. I armed a curved needle with a piece of narrow tape, four inches long, having a knot at one end; this was passed down each end of both cylinders about half an inch, and brought outwards, the tape being prevented slipping through by the knot; the tapes then were thus placed in such a position as to be intermediate to the ligatures; this being done I turned the cylinders gently towards the edge of the wound, and tied the corresponding tapes over it, which I think rendered it much more solid than any number of small ligatures could have done. After the completion of the operation there was so little tension of the parts that we thought it quite unnecessary to make the lateral incisions so strongly recommended by M. Dieffenbach. The wound was dressed with simple dressing, and a roller applied to the knees to guard against involuntary motion during sleep. The mistura cretæ, with the liquor opii sedativus, was given daily to restrain the action of the bowels, and severe diet ordered, to such an extent that during seventeen days nothing was allowed but gruel, and that in small quantities, with occasionally a little hard biscuit. This treatment, however, the lady endured with the greatest patience and fortitude.

The urine was drawn off night and morning, which, as she lay on her side, with the thighs flexed on the body, was frequently attended with no small difficulty.

No untoward symptom occurred, and on the seventh morning the ligatures were removed, when it was found that firm union had taken place along the whole extent of the wound, with the exception of a small fistulous opening near the edge of the sphincter, having no communication with the rectum, and producing no inconvenience to the patient. After nine or ten days the urine was passed without assistance, the parts being guarded by a pledget of linen; the small fistulous opening was touched night and morning with tinct. cantharidis, and occasionally with argenti nitras, with the best possible effect.

On the seventeenth day no evacuation from the bowels having taken place, we administered a purgative draught and laxative enemas, which producing no effect, were repeated; but so impacted had the fæcal matter become that there was much difficulty in accomplishing our object, so that we were obliged to have recourse to manual assistance. We were not without our fears as to the effect of the action of the bowels on the newly-united parts, but the union was so firm as to sustain no injury. When regular action was established, she was allowed gradually to move about, and at the end of six or seven weeks she resumed her usual habits. During the whole time the infant was applied to the breasts at intervals, sufficient to relieve them, and keep up the lacteal secretion, as she was anxious to continue nursing. *Lancet. May* 4, 1839.

On the Power of the Periosteum to form new Bone. By JAMES SYME, Esq. Professor of Clinical Surgery in the University of Edinburgh.

THE question (says Mr. Syme) which I propose to consider is—whether the periosteum, or membrane that covers the surface of bones, possesses the power of forming new osseous substance, independently of the bone itself? From the experiments which are related below, and from the observation of cases of necrosis, Mr. Syme thinks that there is evidence for putting beyond all question *the power of the periosteum to form new bone, independently of any assistance from the old bone.*

EXP. I. An inch and three quarters of the radius of a dog was removed, *with* the periosteum.

EXP. II. An inch and three quarters of the radius of a dog was removed, *leaving* the periosteum.

The results of the two experiments were, that upon examination after the lapse of a considerable space of time, the bone was perfectly reformed where the periosteum *remained*, and where the latter had been *removed* the bone was not regenerated, but in its place was found a band of tough ligamentous texture.

EXP. III. The periosteum was separated from the radius of a dog; a plate of metal was placed between the periosteum and bone; upon examination six weeks afterwards, a deposition of osseous substance was found *in* the periosteum, forming a bony plate exterior to the metal, and not connected with the old bone, excepting by means of the membrane.

EXP. IV. The periosteum was removed from the surface of the radius of a dog; the denuded bone was then covered with a layer of metal; upon examina-

tion six weeks afterwards, a tough capsular covering was found, inclosing the metallic plate, but not containing any osseous substance.

Transactions of the Royal Society of Edinburgh. Vol xiv. Part I. 1839.

["It is quite an erroneous notion," says Müller,* "to imagine that one organized part may be the nutrient organ of another; for instance, that the substance of the bones is formed and nourished by the periosteum. *The osseous substance being itself organized, must itself assimilate the nutritive matters.* The idea that bones are formed by the periosteum, appears to me to be a barbarism unworthy the present state of physiology." We recommend to his notice the experiments of Mr. Syme.]

Case of Dislocation of the Left Femur on the Pubes. By JOHN DUNN, Esq. Surgeon, Scarborough.

JOHN COVERDALE, aged forty-three, was employed on the 13th February, 1839, at a quarry, in Burniston, when about one ton of earth suddenly fell upon his body and right thigh, throwing him upon his back. A second fall of earth immediately succeeded to the first, but striking with great force upon the inside of the left thigh (the right being already fixed), dislodged the femur upon the pubes.

My pupil, Mr. Best, who first saw the patient, immediately discovered the nature of the accident; for there was a very distinct bony prominence on the pubes, a great depression in the usual situation of the great trochanter, the limb everted and separated from its fellow, and about an inch shorter in length. My partner, Mr. Travis, and myself, made arrangements for the patient to be brought to Scarborough, where more efficient attentions could be secured. On calling a consultation, which was obligingly attended to by Mr. Weddell and Mr. Wilson, we found that no proper apparatus for the reduction existed in the town; that the case was of such extreme rarity as never to have been seen by any of us, either in hospital or private practice, but the course to be pursued was laid down by Sir A. Cooper, in his work on Dislocations, and we felt no difficulty in our operations.

Having first placed our patient as upright as we could, on a firm bed, he was bled to 25 oz. As he had only been poorly fed, we did not feel warranted in further depletion. Tartar-emetic was then given in divided doses, to the amount of six grains, but neither syncope nor sickness supervened. He was now laid upon his right side, the left knee bent, and given in charge to an assistant, while a strong towel, passed within the groin to fix the pelvis, was attached to an iron bar, beyond and just before his head. For want of proper pullies, we had recourse to the rather unwieldy ones used in ships, having a single purchase with two single blocks. These were attached to a staple in the floor. A good deal of time was lost at first, in adjusting the apparatus to the knee, so as to give a sufficient firmness of attachment without acting too tightly and injuriously on the skin.

Extension was now made with considerable force, downwards and backwards, for about ten minutes, when the bandage at the knee gave way. Mr. Weddell then reapplied the cords very tight, and extension was renewed in the same direction. After persevering for about ten minutes or a quarter of an hour, without any apparent success, the head of the bone at length seemed to have moved. During this evident relaxation of the muscles, I directed the extension to be desisted from, and lifting up the thigh with a towel placed round it and the back of my neck, Mr. Weddell gently turned the knee inwards, making a lever of the limb. The head of the bone returned to its place, with a slight snap. The fever after the accident was scarcely perceptible. A little sickness and griping supervened, probably from the tartar-emetic; and an aperient was all that was required during the subsequent treatment. The patient was kept in bed three days, when he got up and walked across the room. I saw him about seven weeks after, when I found him walking very well, able to rotate one limb as well as the other, and only complaining of a little weakness in his knee. The time which elapsed, from the occurrence of the accident to the reduction, was about seven or eight hours.

Medical Gazette. May 18, 1839.

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* Müller's Physiology, by Baly, page 383.

PART FOURTH.

Medical Intelligence.

MEDICAL EDUCATION AND MEDICAL REFORM.

I. *Extracts from the "Regulations of the University of London, on the subject of Examinations for Degrees in Medicine."*

1. MATRICULATION EXAMINATION.

No candidate shall be admitted to the matriculation examination unless he have produced a certificate showing that he has completed his sixteenth year.

A fee of two pounds shall be paid at matriculation. No candidate shall be admitted to the examination unless he have previously paid this fee to the registrar; and if he fail to pass the examination, the fee shall be returned to him.

The examination shall be conducted by means of printed papers; but the examiners shall not be precluded from putting any *vivâ voce* questions upon the written answers of the candidates, when they appear to require explanation.

Candidates for the matriculation examination shall be examined in the following subjects:

MATHEMATICS.

Arithmetic and Algebra.—The ordinary rules of arithmetic. Vulgar and decimal fractions. Extraction of the square root. Addition, subtraction, multiplication, and division of algebraical quantities. Proportion. Arithmetical and geometrical progression. Simple equations.

Geometry.—The first book of Euclid.

NATURAL PHILOSOPHY.

Mechanics.—Explain the composition and resolution of statical forces. Describe the simple machines (mechanical powers), and state the ratio of the power to the weight in each. Define the centre of gravity. Give the general laws of motion, and describe the chief experiments by which they may be illustrated. State the law of the motion of falling bodies.

Hydrostatics, Hydraulics, and Pneumatics.—Explain the pressure of liquids and gases, its equal diffusion, and variation with the depth. Define specific gravity, and show how the specific gravity of bodies may be ascertained. Describe and explain the barometer, the siphon, the common pump and forcing pump, and the air-pump.

Acoustics.—Describe the nature of sound.

Optics.—State the laws of reflexion and refraction. Explain the formation of images by simple lenses.

CHEMISTRY.

The component parts of the atmosphere and of water. The general characters of the different groups of elementary bodies, namely, of the supporters of combustion, the combustibles, and the metals. The influence of heat upon the bulk and states of matter.

NATURAL HISTORY.

Botany.—The characters and differences of the natural classes and principal orders of phanerogamous plants belonging to the Flora of Europe, in the botanical classification of De Candolle.

Zoology.—The characters of the primary divisions of the animal kingdom, and of the classes and orders of the vertebrate sub-kingdom, according to the system of Cuvier.

CLASSICS.

The Greek and Latin Languages.—One Greek and one Latin subject, to be selected one year previously by the Committee of the Faculty of Arts from the works of the following authors: Homer, one book; Xenophon, one book; Virgil, one book of the Georgics, or the sixth book of the Æneid; Horace, one book of the Odes; Sallust, the Conspiracy of Cataline, or the war with Jugurtha; Cæsar, the Civil War, or the fifth and sixth books of the Gallic War; Livy, one book; the treatises De Senectute and De Amicitiâ, or two of the shorter, or one of the longer orations.

The English Language.—The grammatical structure of the language. Proficiency in composition will be judged of by the style of answers generally.

Outlines of History and Geography.—History of England to the end of the seventeenth century. The papers in classics shall contain questions in history and geography.

EXAMINATION FOR HONOURS.

Any candidate who has passed, and has produced a certificate showing that he has not completed his nineteenth year, may be examined for honours in mathematics and natural philosophy, and also in classics.

In determining the relative position of candidates, the examiners shall have regard to the proficiency in mathematics evinced by the candidates at the matriculation examination. The examiners shall publish a list of the candidates in the order of proficiency; and candidates shall be bracketed together, unless the examiners are of opinion that there is a clear difference between them.*

If in the opinion of the examiners any candidate shall possess sufficient merit, the candidate who shall distinguish himself the most in mathematics and natural philosophy, and the candidate who shall distinguish himself the most in classics, shall each receive an exhibition of thirty pounds per annum for the next two years, if continuing during that period students at one of the institutions in connexion with this University.

2. BACHELOR OF MEDICINE.

Candidates for the Degree of Bachelor of Medicine shall be required, 1. To have been engaged during four years in their professional studies at one or more of the institutions or schools recognized by this University. 2. To have spent one year at least of the four in one or more of the recognized institutions or schools in the United Kingdom. 3. To pass two examinations.

FIRST EXAMINATION.

No candidate shall be admitted to this examination unless he have produced certificates to the following effect: 1. Of having completed his nineteenth year. 2. Of having taken a degree in arts in this University, or in a University the degrees granted by which are recognized by the senate of this University, or of having passed the matriculation examination. 3. Of having been a student during two years at one or more of the medical institutions or schools recognized by this University, subsequently to having taken a degree in arts, or passed the matriculation examination. 4. Of having attended a course of lectures on each of four of the subjects in the following list: Descriptive and Surgical Anatomy; General Anatomy and Physiology; Comparative Anatomy; Pathological Anatomy; Chemistry; Botany; Materia Medica and Pharmacy; General Pathology; General Therapeutics; Forensic Medicine; Hygiene; Midwifery and Diseases peculiar to Women and Infants; Surgery; Medicine. 5. Of having dissected during nine months. 6. Of having attended a course of Practical Chemistry, comprehending practical exercises in conducting the more important processes of general and pharmaceutical chemistry; in applying tests for discovering the adulteration of articles of the materia medica, and the presence and nature of poisons; and in the examination of Mineral Waters, Animal Secretions, Urinary Deposits, Calculi, &c. 7. Of having attended to Practical Pharmacy during a sufficient length of time to enable him to acquire a practical knowledge in the preparation of medicines.

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* This regulation holds good for all the other examinations.—Ed.

The fee for this examination shall be five pounds. No candidate shall be admitted to the examination unless he have previously paid this fee to the registrar; and if he fail to pass the examination the fee shall be returned to him.

Candidates shall be examined in the following subjects: Anatomy; Physiology; Chemistry; Structural and Physiological Botany; Materia Medica and Pharmacy.

EXAMINATION FOR HONOURS.

Any candidate who has been placed in the first division at the first examination, and who has produced a certificate showing that he has not completed his twenty-second year, may be examined for honours in any or all of the following subjects: Anatomy and Physiology (candidates may illustrate their answers by sketching the parts they describe); Chemistry; Materia Medica and Pharmaceutical Chemistry.

The examinations shall take place in the week following the first examination. They shall be conducted by means of printed papers; but the examiners shall not be precluded from putting *vivâ voce* questions upon the written answers of the candidates when they appear to require explanation.

If in the opinion of the examiners sufficient merit be evinced, the candidate who shall distinguish himself the most in anatomy and physiology, the candidate who shall distinguish himself the most in chemistry, and the candidate who shall distinguish himself the most in materia medica and pharmaceutical chemistry, shall each receive an exhibition of thirty pounds per annum for the next two years.

Under the same circumstances the first and second candidates in each subject shall each receive a gold medal of the value of five pounds.

SECOND EXAMINATION.

No candidate shall be admitted to this examination within two academical years of the time of his passing the first examination, nor unless he have produced certificates to the following effect:

1. Of having passed the first examination.

2. Of having, subsequently to having passed the first examination, attended a course of lectures on each of two of the subjects comprehended in the list given under the head of the First Examination, and for which the candidate had not presented certificates at the first examination.

3. Of having, subsequently to having passed the first examination, dissected during six months.

4. Of having conducted at least six labours. Certificates on this subject will be received from any legally-qualified practitioner in medicine.

5. Of having attended the surgical practice of a recognized hospital or hospitals during twelve months, and lectures on clinical surgery.

6. Of having attended the medical practice of a recognized hospital or hospitals during other twelve months, and lectures on clinical medicine.

7. Of having, subsequently to the completion of his attendance on surgical and medical hospital practice, attended to practical medicine in a recognized hospital, infirmary, or dispensary, during six months. Certificates on this subject will be received from any legally-qualified practitioner having the care of the poor of a parish.

The candidate shall also produce a certificate of moral character from a teacher in the last school or institution at which he has studied, as far as the teacher's opportunity of knowledge has extended.

The fee for this examination shall be five pounds. No candidate shall be admitted to the examination unless he have previously paid this fee to the registrar; and if he fail to pass the examination, the fee shall be returned to him.

Candidates shall be examined in the following subjects:

Physiology: the papers in physiology shall include questions in comparative anatomy. General pathology, general therapeutics, hygiene; surgery; medicine; midwifery; forensic medicine.

EXAMINATION FOR HONOURS.

Any candidate who has been placed in the First Division at the second examination, and has produced a certificate showing that he has not completed his twenty-fifth year, may be examined for honours in any or all of the following subjects—Physiology and Comparative Anatomy (candidates may illustrate their answers by sketching the parts they describe); Surgery; Medicine; Midwifery; Structural and Physiological Botany.

The examinations shall take place in the week following the second examination. They shall be conducted by means of printed papers; but the examiners shall not be precluded from putting *vivâ voce* questions upon the written answers of the candidates when they appear to require explanation.

If in the opinion of the examiners sufficient merit be evinced, the candidate who shall distinguish himself the most in Physiology and Comparative Anatomy, the candidate who shall distinguish himself the most in Surgery, and the candidate who shall distinguish himself the most in Medicine, shall each receive an exhibition of fifty pounds per annum for the next two years, with the style of University Medical Scholar.

Under the same circumstances, the first and second candidates in each of the preceding subjects shall each receive a gold medal of the value of five pounds.

Under the same circumstances the candidate who shall distinguish himself the most in Midwifery, and the candidate who shall distinguish himself the most in Structural and Physiological Botany, shall each receive a gold medal of the value of five pounds.

3. DOCTOR OF MEDICINE.

No candidate shall be admitted to this examination unless he have produced certificates to the following effect: 1. Of having taken the degree of Bachelor of Medicine in this University, or a degree in Medicine or in Surgery at a University the degrees granted by which are recognized by the Senate of this University. Those candidates who have not taken the degree in this University shall produce a certificate of having completed their twenty-third year. 2. Of having attended, subsequently to having taken one of the above degrees in Medicine—*a.* To Clinical or Practical Medicine during two years in a hospital or medical institution recognized by this University. *b.* Or, to Clinical or Practical Medicine during one year in a hospital or medical institution recognized by this University, and of having been engaged during three years in the practice of his profession. *c.* Or, if he have taken the degree of Bachelor of Medicine in this University, of having been engaged during five years in the practice of his profession. One year of attendance on Clinical or Practical Medicine, or two years of practice, will be dispensed with in the case of those candidates who at the second examination have been placed in the First Division. 3. Of moral character, signed by two persons of respectability.

The fee for the degree of Doctor of Medicine shall be ten pounds. No candidate shall be admitted to the examination unless he have previously paid this fee to the registrar; and if he fail to pass the examination the fee shall be returned to him. The examination shall be conducted by means of printed papers and *vivâ voce* interrogation. Candidates shall be examined in the following subjects: Elements of Intellectual Philosophy, Logic, and Moral Philosophy; Medicine.

If in the opinion of the examiners sufficient merit be evinced, the author of the best commentary on the Case in Medicine, the author of the best commentary on the Case in Surgery, and the author of the best commentary on the Case in Midwifery, shall each receive a gold medal of the value of five pounds.

Any candidate may present a thesis on a subject of his own choice. If in the opinion of the examiners sufficient merit be evinced, a gold medal, of the value of ten pounds, shall be given to the author of the best thesis. The examiners shall not be precluded from examining the author on the subject of his thesis.

EXAMINATION FOR HONOURS.

Any candidate who has been placed in the First Division may be examined for honours in any or all of the following subjects: Surgery, Medicine, Midwifery.

The examinations shall be conducted by means of printed papers; but the examiners shall not be precluded from putting *vivâ voce* questions upon the written answers of the candidates when they appear to require explanation.

If in the opinion of the examiners sufficient merit be evinced, the first candidate in each subject shall receive a gold medal of the value of five pounds.

INSTITUTIONS AND SCHOOLS.

No medical institution or school shall be recognized by the Senate of this University which does not possess ample means of illustrating the instruction given at it.

Regulations relating to Students who commenced their Medical Studies in or before January, 1839, *to continue in force until the year* 1842.

BACHELOR OF MEDICINE.

Previously to the year 1842, candidates who have been engaged during two years in their professional studies shall be admitted to the first examination for the degree of Bachelor of Medicine on producing certificates to the following effect: 1. Of having been engaged during two years in their professional studies. 2. Of having attended a course of lectures on each of four of the subjects comprehended in the list given under the head of the *First* Examination. 3. Of having dissected during nine months. 4. Of having attended to Practical Pharmacy during a sufficient length of time to enable them to acquire a practical knowledge in the preparation of medicines.

Candidates shall be admitted to the second examination at the expiration of two years after the first examination, on producing the certificates required at that examination.

Previously to the year 1842, candidates who have been engaged during four years in their professional studies shall be admitted to the second examination for the degree of Bachelor of Medicine, on producing certificates to the following effect: 1. Of having been engaged during four years in their professional studies. 2. Of having passed the first examination. 3. Of having attended a course of lectures on each of two of the subjects comprehended in the list given under the head of the *First* Examination. 4. Of having dissected during twelve months. 5. Of having attended to practical pharmacy during a sufficient length of time to enable the pupil to acquire a practical knowledge in the preparation of medicines. 6. Of having conducted at least six labours. 7. Of having attended the surgical practice of a recognized hospital or hospitals during twelve months. 8. Of having attended the medical practice of a recognized hospital or hospitals during other twelve months. 9. Of having completed the twenty-second year of their age. 10. Of moral character from a teacher in the last school, or institution at which they have studied, as far as the teacher's opportunity of knowledge has extended.

Candidates who have not taken a degree in Arts, or passed the Matriculation Examination in this University, will be required to translate a portion of Celsus *de Re Medica.*

Regulations relating to Practitioners in Medicine or Surgery desirous of obtaining Degrees in Medicine.

BACHELOR OF MEDICINE.

Candidates shall be admitted to the two examinations for the degree of Bachelor of Medicine on producing certificates to the following effect: 1. Of having been admitted, prior to the year 1840, members of one of the legally-constituted bodies in the United Kingdom for licensing practitioners in medicine or surgery. 2. Of having received a part of their education at a recognized institution or school, as required by the charter of the University. 3. Of moral character, signed by two persons of respectability.

DOCTOR OF MEDICINE.

Candidates who have been engaged during five years in the practice of their profession shall be admitted to the examination for this degree on producing certificates to the following effect: 1. Of having been engaged during five years in the practice of their profession. 2. Of having taken the degree of Bachelor of Medicine in this University.

II. *Extracts from "Propositions relative to the Education and Privileges of Practitioners in the several Branches of Medicine, and of Chemists and Druggists: agreed on by the Medical and Surgical Professors in the University, the Royal College of Physicians, and the Royal College of Surgeons of Edinburgh. March,* 1839."

1. That the legislature ought to fix a minimum course of education, without certificates of having accomplished which, no one should be admissible to examination for a licence to practise any of the branches of medicine.

2. That without such a licence, no one should be eligible to hold a medical or surgical appointment in any public institution.

3. That no person ought to obtain a licence entitling him to act as a general medical practitioner, who has not received a competent education in literature and science, studied in a recognized school of medicine or surgery, and undergone examination before a competent board or boards, on all the branches of medical education mentioned in the curriculum hereinafter specified.

4. That the degrees or licences granted by all public institutions which have heretofore been engaged in regulating the education, and ascertaining the qualifications, of those intended for the medical profession (or by such new boards as it may be found expedient to establish for the same purposes), should confer the right of acting as general medical practitioners, and of dispensing medicines in all parts of the British dominions: provided, first, the course of education required by these institutions or boards be not in any case less, and in the cases hereinafter provided be superior, in extent and duration, to that which shall be determined on as necessary for obtaining a licence; and, second, that the examining boards of these institutions be so constituted, as to afford a sufficient security that the members of whom they are composed possess the qualifications necessary to fit them for ascertaining, by examination, the proficiency of candidates.

5. That the time to be occupied in the minimum course of education above-mentioned, at a university or recognized school, should not be less than twenty-seven months, in which should be included three winter sessions of six months' duration each; and that the minimum age of the candidate for a diploma or licence may be advantageously fixed at twenty-one.

7. That apprenticeship should not necessarily form a part of this education; but that those who have not been apprentices should be required to bring proof of having acquired a knowledge of practical pharmacy in a laboratory or an apothecary's shop; and of having had opportunities of witnessing the treatment of diseases, for a period of not less than six months, as pupils to practitioners in dispensaries, or in public hospitals receiving out-patients, or as pupils to regularly licensed private practitioners.

8. That evidence should likewise be required from candidates for licences, who have not previously obtained the degree of A.B. or A.M., of their possessing an adequate acquaintance with Latin, and of their having received instruction in the elements of mathematics, and in natural philosophy; and that it is highly desirable these branches should be studied previously to commencing the professional education.

9. That strict sessional examinations should be enforced on all students following their course of study in universities or schools of medicine in Great Britain or Ireland, at the close of each winter and summer course; and that a record should be kept of the result of the examinations, to be produced at the final examinations for degrees and licences.

10. That the final examination for the licence to practise should be divided into at least two parts, to be held on different days; and that, in Edinburgh, these examinations may be advantageously conducted by a joint board of Fellows of the Royal Colleges of Physicians and Surgeons.

11. That no person ought to obtain a licence to act as a surgeon who has not

gone through a course of education at least equal in duration and extent to that laid down in the minimum schedule of education above stated, and undergone examination before a competent board.

12. That all persons having obtained a licence to practise surgery, in conformity with the above conditions, should be entitled to act as general medical practitioners, and to dispense medicines in all parts of the British dominions, provided they shall have been examined by a competent board or boards, on all the branches of education specified in the curriculum above stated.

13. That the course of study and the examinations for any degree in medicine granted by a university, ought to comprehend all the branches of knowledge stated above, and to imply a more extended education than is prescribed for the general medical practitioner.

14. That, on the other hand, the education for a university medical degree, should not be raised so high, by legislative enactment, above what is required of the general practitioner, as to limit, injuriously for the public, the number of those persons who, in order to obtain the honour, may be induced to take a fuller course than is necessary for a simple licence.

15. That the time to be occupied in the course of education, required for any medical degree from a university, should not be less than thirty-three months, in which should be included four winter sessions of six months' duration each; and that the minimum age of the candidate for such a degree may be advantageously fixed at twenty-two.

16. That the superiority of university medical degrees should be further secured by the course of study required for it, embracing additional branches of science connected with medicine, and by enjoining repeated attendance on the more important departments.

17. That a certain portion of the study qualifying for the honour of a medical degree should be prosecuted in some university which grants that degree, and that any such university ought to insist on attendance therein during a winter session, as preliminary and requisite to examination.

18. That all persons having obtained a medical degree from a university, in conformity with the above conditions, should be entitled to act as general medical practitioners, and to dispense medicines in all parts of the British dominions.

19. That the lectures of all teachers should be recognized as qualifying for the licence of general practitioner who are professors in universities, or fellows of the Royal Colleges of Physicians or of Surgeons in England, Scotland, or Ireland, or of the Faculty of Physicians and Surgeons of Glasgow, and who shall have been specially recognized as teachers by the bodies to which they belong, after they shall have been satisfied, by examination or otherwise, of their being duly qualified to teach.

20. That all other teachers, before their lectures are admitted to the same privilege, should, besides being in possession of a medical degree from a university, or a diploma or licence from one of the Royal Colleges of Physicians or of Surgeons, or of the Faculty of Physicians and Surgeons of Glasgow, or other legally-constituted licensing body, undergo an examination on the branch they propose to teach.

23. That the lectures of teachers, not being professors in universities, but qualified as above stated, ought to be recognized in such extra academical education as may be allowed to qualify for university degrees.

24. That professors and other recognized teachers ought to give courses on the respective subjects of their lectures, of such extent and duration as may be deemed sufficient by the legislature to embrace the full consideration of these subjects.

25. That the lectures of no professor or lecturer who teaches within the same year more than one of the branches required, ought to be recognized; but that, in reference to this regulation, anatomy with practical anatomy, and chemistry with practical chemistry, might be considered as one branch respectively; while clinical medicine and clinical surgery might be taught, in addition to any of the other branches, by professors, physicians, and surgeons, to whom is intrusted the charge of recognized hospitals.

26. That provision should be made in regard to chemists and druggists for their

being found sufficiently qualified to compound, prepare, and dispense medicines; and that no persons ought to obtain licences to act as such who have not, first, attended at least one full course of lectures on each of the following subjects, viz. chemistry, botany, and materia medica, and a course of practical chemistry, by recognized teachers; second, been employed for three years in compounding and preparing medicines under a licensed general practitioner, or licensed chemist and druggist; and, third, attained the age of twenty-one years.

27. That, previously to obtaining such licences, the candidates should undergo an examination, before a competent board, on chemistry, botany, and materia medica, and as to their knowledge of the Latin language, and of the forms of prescription.

28. That those persons only who have gone through such education and examination, should be entitled to the name of licensed or approved chemists and druggists, or to such other designation as may imply their qualification; that the licence granted to them should infer no right to exercise the duties of general practitioner, but should entitle them to act as chemists and druggists in any part of the British dominions.

III. *Extract from "The Petition of a Meeting of Physicians, Surgeons, and Apothecaries, practising in Ulster, convened by public advertisement, and held in the Belfast Hospital on Wednesday, the 24th of January,* 1838."

That your petitioners humbly and respectfully beg to submit the annexed outline of a plan for the better regulation and government of the Medical Profession in the British dominions, to the consideration and wisdom of your Honorable House; feeling assured that such outline will be fully sustained by the evidence taken before the Committee of your Honorable House; and if adopted, will tend materially to lessen or remove altogether the anomalies, abuses, and mischief, at present existing; while at the same time, it will improve and elevate a profession whose high and peculiar characteristic is to minister to and alleviate the sufferings of mankind.

1st. That a Central Board of Examiners be formed in each of the metropolitan cities—in London, Dublin, and Edinburgh, who shall possess the power of conferring a diploma in medicine, which will entitle the possessor to practise in any or all of the branches, and in any part of the British dominions; that the persons composing these boards be selected from the most distinguished members of the profession in the three kingdoms; shall receive a *stated* annual salary for their services as examiners, and be totally unconnected with any medical school; that they be authorized to frame ONE *curriculum of education*, on an enlarged and scientific scale, for the entire medical profession, which all medical teachers and students shall be strictly bound to observe.

2d. That they be intrusted with the power of recognizing medical schools and teachers; and of punishing, on summary jurisdiction, all persons who presume to teach or practise illegally the profession in any one of its departments. Further, that they be instructed to frame ONE *Pharmacopœia* for the whole empire; and thus remove the inconveniences and risk at present so generally experienced by a want of uniformity in the preparation of several most important and dangerous remedies.

3d. That the existing Universities, Colleges, and Halls be deprived merely of the privilege of conferring medical diplomas, and that they be permitted to exist as schools; retaining all the funds, museums, and other properties they may be already possessed of.*

IV. *Extracts from a Statement made to the Home Secretary, by a Deputation from the British Medical Association, June* 2, 1838.

Some of the evils under which the profession labours were briefly enumerated to his Lordship.

First. There were the unreformed medical corporations, eight in number, namely, 1, the Royal College of Physicians; 2, the Royal College of Surgeons;

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* Lancet, June 23, 1838.

3, the Society of Apothecaries, in London: 4, the Royal College of Physicians, and 5, the Royal College of Surgeons, in Edinburgh: 6, the College of Physicians; 7, the Royal College of Surgeons; and 8, the Apothecaries' Company, in Dublin: which are evils in themselves, inasmuch as they are nearly all obnoxious to the abuses and defects of the unreformed municipal corporations, which no party now dares to defend.

Secondly. There were the councils, or governing bodies, of those corporate institutions, which are self-elective or self-perpetuating, and the members of which are elected for life.

Thirdly. There was the manner in which the proceedings of those corporate bodies were carried on, than which nothing could be more inconsistent with the principles of the free constitution of this country, inasmuch as they were always carried on *in secret;* and inasmuch, also, as a full statement of the *accounts*, or of the appropriation of the money which has been levied on the members, and has accumulated, is never published.

Fourthly. There was the irresponsibility of those governing bodies, whose councillors considered themselves as being neither amenable to the members at large of their own body, nor to the public, nor to the government of the country; the injurious effect of which practice was to be found in the working of the innumerable by-laws they had each of them (like the rest of the corporations now reformed) enacted. Those by-laws prescribed onerous injunctions on the meritorious student, levied money in hospitals and schools, and promoted private views at the public expense, whereby medical knowledge and medical improvement were checked instead of being promoted.*

V. *Extract from a Letter of* Dr. Webster, *President of the British Medical Association, addressed to the Secretary of the Glasgow Medical Association. July* 3, 1838.

The prominent object of the British Medical Association is, 1st. The establishment of a "National Faculty of Medicine," (of which a branch might exist in each capital,) which should comprehend all the legally-qualified practitioners in the kingdom. 2dly. That a senatus or representative council be elected by the faculty, which shall manage the affairs of the profession, and regulate all matters relating to the education, examination, and admission of future members. 3dly. That the members of the faculty shall all possess the same rights, privileges, immunities, and be able to practise in all parts of her Majesty's dominions without let or hinderance. 4th. That the members of the faculty be recognized by the title of Doctor; or they might be divided into two classes, Doctor in Medicine and Doctor in Surgery; and that they shall be at liberty to practise the whole or any branch of the healing art, and write prescriptions only, or dispense their own medicines, as may be found most eligible or convenient. 5thly. That chemists and druggists shall be examined as to their knowledge of chemistry, materia medica, and pharmacy; and be licensed to dispense medicines only, but not to practise any branch of the profession. 6thly. That there be a British Pharmacopœia for the whole empire. 7thly. That all the members of the faculty be registered; and that any person practising without being so registered or holding a diploma or licence from the senatus, shall be summarily punished.†

VI. *Extract from a "Report of the Medical and Surgical Society of Newcastle-upon-Tyne, dated Nov.* 27, 1838."

There are in the United Kingdom of Great Britain and Ireland not fewer than sixteen corporations having power to grant degrees in medicine and surgery, and differing essentially in the extent and duration of the curricula they enjoin. The Society is of opinion that all of these might be advantageously superseded by one institution being placed at the head of the profession in each division of the empire, whose privileges should be reciprocal, and whose executive officers, elected by the members at large, should hold periodical conferences, for the purpose of establishing uniformity of operation; that such institutions should have entire control over education, and the granting of degrees and licences to practise, together with all other matters relating to the medical profession; that the course of instruction and test of qualification should be the same in each; that, from one or other of them, all persons engaged in the practice of medicine and surgery should be required to possess a diploma or licence; that, to individuals thus authorized, the law should extend a suitable protection; and that proper measures should be enforced for the suppression of unqualified practitioners.

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* Lancet, June 16, 1838.

† Ibid. July 28, 1839.

The Society would suggest, as a means of effecting the last-named object, that every person, before commencing practice in any town or other locality, should be required to obtain a certificate from a magistrate, giving him permission to that effect, which should be granted on the production of satisfactory testimonials of qualification, and that, after having been thus authorized, his name should be duly registered. Persons presuming to practise, whose names have not been so registered, should be subjected to a penalty on summary conviction before a justice of the peace.

The rapid progress of science during the present century, in conjunction with increased facilities for the attainment of medical and surgical knowledge, have fully proved that any attempt to constitute an arbitrary division of diseases, and to consign the treatment of them to different classes of practitioners, according as they affect the external or internal parts of the body, is not only unscientific, but impracticable; and, as the physician and the surgeon must be guided by similar principles in combating disease, whether involving the surface or the interior of the human frame, the education of all practitioners should, in the opinion of this Society, be regulated by one common standard. Those distinctions in rank which have hitherto subsisted (not, perhaps, without good effect,) would thus be rendered unnecessary, since there could be no longer any rational ground for separating into different grades men who would be identified not less in education than in the nature and object of their pursuits. Such uniformity, if established in this as in other countries, would place practitioners, whether of medicine or of surgery, on an equal footing; but would not, in the least degree, prevent individuals devoting their energies to the prosecution of any particular department of professional duty, which inclination or other circumstances, might lead them to adopt in preference to another.

The task of preparing the medicines prescribed by the general practitioner, devolves, almost universally, on the apprentice of the latter. That material benefit might accrue from a well-devised scheme of pupilage, there can be no question; but apprenticeships, as at present conducted, have ever been productive of unhappy results; and in no respect does the unfavorable tendency of this system appear more conspicuous than when viewed as an instrument for executing the responsible duty of dispensing. The abolition of apprenticeships, so far, at least, as this department is concerned, would be highly expedient. The Society is of opinion, that a charge so important might, with greater safety, be confided to an apothecary or dispenser, who had been examined in pharmacy, and had obtained a specific licence for the purpose in question. Such substitute for the apprentice would, the Society believes, be most desirable, not less for the comfort and convenience of the practitioner than for the welfare and security of his patients.

This proposition, if acted upon, might have an additional good effect in terminating the absurd method by which at present the majority of general practitioners seek to be remunerated, viz., by a profit on the medicines they supply.

The foregoing representations suggest the desirableness of obtaining,—

1. An improved system of education.
2. A more efficient method of examination.
3. One governing body to preside over the whole profession in England, Scotland, and Ireland.
4. Uniformity of education and of grade among practitioners.
5. Adequate protection for legally-authorized practitioners.
6. The prevention of unqualified persons.
7. The suppression of quackery.

8. The separation of the practitioner and the dispenser in the same individual.
9. The abolition of apprenticeships as at present constituted.
10. The institution of licensed dispensers.

VII. *Resolutions agreed to at a Meeting of the Royal College of Surgeons in Ireland, held on Monday, April 22,* 1839.

1. That the College are willing and anxious to adopt such steps as may be considered practicable and expedient, having for their object the incorporation of the whole body of practitioners in Ireland into a firm and powerful union.

2. That, in effecting such a measure, the College conceive that those only can be properly considered as medical practitioners who pursue the healing art as a profession; and that, consequently, persons following the business of retail druggists ought not to be admissible into the proposed union.

3. That in making this restriction, however, the College by no means wish to undervalue the occupation of a druggist or apothecary; but that they consider its pursuit by an individual, who at the same time practises medicine, to be alike injurious to the public and to the parties concerned.

4. That the College are prepared to go the length of seeking a new charter of incorporation, if the proposed union cannot be satisfactorily effected by any other mode.

5. That, inasmuch as some difference of opinion may at present exist, as to the mode of effecting the objects referred to in the resolutions now presented, the College conceive that a general congress of the profession ought to be immediately held; and they now declare that they will cordially assist in the proceedings of such congress, if its assemblage be thought advisable by the mass of the practitioners of Ireland.

6. That it is the opinion of the College that this union of the profession should include both physicians and surgeons, with equal privileges.

7. That, in order to carry into effect the suggestions contained in the report now agreed to, it is resolved, That the College, seeing the utility of a union of all the physicians and surgeons of Ireland, not engaged in the practice of pharmacy, willingly consents to become the centre of such a union, and to afford every facility in its power for the attainment of the important objects contemplated by those who have proposed it.

8. That, as the efficiency of such a union depends upon its being established upon just, sound, and liberal principles, deputies should be sent from every county in Ireland, to represent the opinions and wishes of their respective constituents, or those who are unable to attend, at each general meeting of the society.

9. That the first meeting of a society thus formed, which may be aptly termed 'The Medical Union of Ireland," shall be holden in this College on the 29th day of May next, to which all the physicians and surgeons of Ireland, not practising as apothecaries, shall be invited by public notice in the newspapers to attend.

10. That any officers appointed to carry these purposes into effect shall be merely esteemed *pro tempore* until arrangements are made by the first general meeting. That the committee be authorized to give notice by advertisement of these resolutions, and make such arrangements as may be necessary to carry them into effect.

11. That the proposed meeting of the profession is to be held for the purpose of determining upon the best means of forming a union of the profession, and not for the purpose of considering whether such a union is or is not expedient.

12. That the president and secretaries, with the committee of correspondence, be deputed to meet the President and Fellows of the College of Physicians to submit the foregoing resolutions to them, and ask the co-operation of their College.

VIII. *Resolutions passed at the Meeting of the Physicians and Surgeons of Ireland, called in accordance with the foregoing Resolutions, and held in Dublin, on Wednesday, the 29th May,* 1839, *partly at the Royal College of Surgeons, and partly (owing to want of room in the former) at the Theatre.*

1. That we, the physicians and surgeons of Ireland, having expended large

sums of money and much time and labour in the acquisition of professional knowledge, and many of us being engaged in the performance of important duties to the public, feel that it is not unreasonable for us to expect from the government and the legislature protection equal to that afforded to the members of other liberal professions.

2. That it is the opinion of this meeting that we are not protected, inasmuch as while we have voluntarily submitted to a lengthened course of medical study, and trying examinations; and while we have contributed large sums to the revenue in the form of stamp duties, yet our services are constantly enforced in the administration of public justice without any or with insufficient provision being made for our remuneration.

3. That it is the opinion of this meeting that the cause of all these evils is to be traced to the existence of divisions and separate interests among the members of the medical profession; and that their effectual remedy is to be sought for in a permanent union.

4. That the true basis of such union should be a similarity of studies, pursuits, and interests; and that we, the physicians and surgeons of Ireland now assembled, do declare that medicine and surgery are one science and one profession, and that the separate practice of certain departments by distinct individuals is merely an expedient division of labour, having no reference to education or to general professional interests.

5. That it is therefore our opinion that a legislative measure should be sought for by us to unite the medical profession of Ireland into a corporation upon such principles as shall constitute them one national faculty, and thereby identify in feeling and interests the great mass of provincial practitioners with their metropolitan brethren.

6. That, to promote so desirable an end, steps be at once taken to form district medical associations throughout Ireland, which shall be composed of all practitioners in medicine holding degrees or diplomas in medicine or surgery from any of the colleges, corporations, or universities, at present legally authorized to grant the same, who are of irreproachable moral and professional character; and who are not engaged in the business of retailing drugs, or compounding the prescriptions of others.

7. That the members of such district associations shall elect from among themselves a president, vice-president, secretary, and committee; that they shall have the power to nominate delegates to represent their interests at the general meetings of the profession, and that the duties of such associations shall be to obtain local information regarding all matters of medical police; to settle disputes among their own members; to watch over their local interests, and to communicate with a central metropolitan council.

8. That the following are the principles which should be held in view in constructing the constitution of the new College, which it is proposed to establish: *a.* Fundamental regulations with regard to the education of all persons proposing to enter the profession of medicine, to be permanently established. *b.* Compliance with such fundamental regulations to be rewarded with the licence of the college, conferring free and equal right to all professional practice, office, and emoluments. *c.* All persons so licensed to be enrolled as members of the corporation, upon the expiration of a certain period of probation, provided only they can produce evidence of an irreproachable moral and professional character. *d.* Every person so enrolled, to be thenceforward free to vote and act in the college in every corporate capacity whatsoever. *e.* A general meeting of the whole college to be held on the last Wednesday in May in each year (being the anniversary of the present congress) to which it may be lawful for the district associations to send representatives, the business of such meeting being to elect a president, secretaries, and an executive council, which shall carry on the government of the college during the ensuing year; but shall at all times sit, deliberate, and vote with open doors. *f.* A general meeting to be called at any time by the council, or upon a requisition signed by at least twenty members.

The title of this college shall be the Royal College of Medicine and Surgery in

Ireland, and it shall be to all intents and purposes a union of the two branches of medicine and surgery into one faculty.

9. That at the first formation of the new College, all persons holding degrees or diplomas in medicine or surgery from any of the Colleges or Universities at present legally authorized to grant the same, who have been five years in the practice of their profession, not following the business or profession of a retail druggist or apothecary, and who can produce evidence of irreproachable moral and professional character, shall be enrolled as members of the corporation, upon payment of a sum not exceeding 20 guineas.

10. That persons similarly circumstanced, but who have not been five years n practice, shall, on the payment of a like sum, not exceeding 20 guineas, be nrolled as licentiates of the college, and shall be held qualified to be enrolled as nembers of the corporation, upon the completion of the full term of five years rom the date of their original degree or diploma, without further expense.

11. That members and licentiates of the College of Surgeons, and fellows and icentiates of the College of Physicians (provided the latter body join the union) hall be exempted from any such payment, they having already paid large sums, nd now possessing exclusive rights in the present colleges.

12. That persons now enrolling their names, and pledging themselves to upport the foregoing resolutions, shall, together with the members and licentiates f the College of Surgeons, and of the College of Physicians (in case that body in in the union) constitute an association to continue in existence until the new harter or act of incorporation shall have been obtained, and that the provisional ody so framed shall be constituted as to its officers, meetings, &c., as nearly as ossible upon the principles already laid down for the constitution of the new college.

13. That a provisional council, secretaries, and president be now elected, and at the business of organization be at once commenced.

14. That Richard Carmichael, Esq. be president of the provisional council, at Dr. Maunsell shall be secretary, and that the council shall consist of the comittee of correspondence of the College of Surgeons, with the deputies of the local sociations now existing, or hereafter to be appointed, with power to add to their umber, and to communicate with medical associations in the sister kingdoms.

15. That it is our firm conviction that the interests of the public, and of the edical profession, require that encouragement should be given to a class of ientific apothecaries, whose time and attention would be exclusively devoted to e preparation and compounding of medicines, and who would thus have an pportunity of raising the profession of pharmacy from its present degraded conition in Ireland to a level with that which it occupies in France and Germany; nd that we think such encouragement would be best afforded by the establishent of a College of Pharmacy; by the prevention of medical practitioners from eeping shops for the sale of drugs, or compounding the prescriptions of others, nd by affording to regularly-educated apothecaries protection, by giving them an xclusive right of dealing in medicinal articles, and such other advantages as ould fairly be granted to them by the legislature.

16. That we disclaim all intention of interfering by any of our proceedings ith vested rights now enjoyed by any individuals.

17. That the establishment of a relief and widows' fund be recommended to he attention of the provisional council.

18. That the best thanks of the meeting be given to Mr. Carmichael for his dmirable conduct in the chair.

REMARKS.

We have been waiting, quarter after quarter, we may indeed say year after year, for the publication of the Report of the Parliamentary Committee of Enquiry into the state of the medical profession, in order that we might call the attention of our readers to a subject of such surpassing interest, and state our own opinions and views respecting it. As, however, such Report has not made its appearance, and, we fear, is not likely soon to do so, and as, consequently, the prospect of any legislative enactment to be derived from this source seems nowise probable, we are compelled by the magnitude and urgency of the question at issue, to enter upon it with such help as we are able to derive from the exertions and demonstrations made by the profession itself since the institution of that enquiry. On the present occasion, however, it is our purpose merely to open the subject, reserving for another and we hope an early opportunity the ample discussion which it requires, and to which it is so well entitled.

The preceding documents, and we might have added others of a similar character from the same sources, sufficiently show how general and deep is the impression among the members of the profession of the necessity of medical reform. We have reason to believe that this impression will be still further strengthened and extended by the proceedings of the Provincial Medical and Surgical Association, at the next meeting of the members, to be held at Liverpool on the 24th of the present month. We regret that we are not at liberty to publish the Report of a Committee appointed by the Association for the purpose of watching over the interests of the profession, which will be laid before the members at this meeting. We may, however, state that this Report strongly recommends the same consolidation and union advocated in the preceding documents; and differs only from these in giving the subject of EDUCATION as the basis of the reform, a more prominent place and character. It is because we so perfectly accord in this view of the subject, that we have united, under one head, in the preceding pages, the documents relating exclusively to education, and those bearing chiefly on the external arrangements of the profession and the privileges of its members. It must be manifest to every one qualified to judge, and who takes a proper view of the subject, that a sufficient and well-arranged education, as well preliminary as professional, must lie at the root, and indeed must form the indispensable basis of all rational and fructifying reform. This truth was fully admitted by several of the distinguished speakers at the great Dublin meeting, a circumstance which we are the more anxious to point out, as, although it is recognized in the resolutions, it certainly occupies a less prominent place in them than is desirable. "It was proposed (Dr. Maunsell said) that the road of entrance to the medical profession should be that of education alone; that there should be the highest possible standard of education established; and under no other circumstances would any man be permitted to enter. This was one of the grand objects of the union, and one, which necessary as it was to the public, and the medical profession, could yet never be effected so long as sixteen or seventeen distinct and rival corporations existed and competed with each other, not in generous efforts to raise the standard of medical education, but in a Dutch auction, as to which of them should give the licence to practise on the cheapest and easiest terms." "The resolutions (said Dr. Bullen) called upon the legislature to enable them to obtain an act—for what? Why, to effect medical educational reform: for the first great principle upon which the act must be framed, must be to elevate the medical profession to the highest standard of eminence. It was only by elevating the masses that they could raise the units, and there was no man whose interest would not be advanced by every point in which they could raise the general medical education of the practitioners of Ireland." It is certainly true that many advocates of medical reform who have evinced a strong desire to regulate the outer and grosser mechanism of the profession, have paid but slight regard to the finer, less visible, but much more important springs within. But this, unquestionably, is beginning reform at the wrong end. We, on the other hand, are so impressed with the paramount importance of the education, that we desire to see it take precedence in all deliberations respecting the improvement of our profession; and this must be our excuse for confining our remarks, on the present occasion, so much to its consideration.

It is but justice to almost all our examining bodies to state, that, of late years, they have much raised the standard of qualification in the candidates for their licences, by insisting on more extended studies. In the "Propositions" from Edinburgh, given above, we see further indications of a greatly improved system of education. It is, however, in the document placed first in our series, the "Regulations" of the University of London, that we recognize, for the first time, all the essential elements of a complete medical education; and we do not hesitate to avow our belief, that whatever other improvements may be the result of the medical agitation now on foot in Great Britain and Ireland, they will be imperfect unless they contain provisions for enforcing an education, preliminary and professional, as liberal, at least, if not precisely the same. Indeed, we do not know that much more is wanting to satisfy intelligent reformers than the establishment in each of the other divisions of the empire of an institution like the University of London, only constructed on a more enlarged and liberal basis and endowed with higher powers and privileges. The first great desideratum for the improvement of the profession is, unquestionably, to have established, by legislative enactment, a *minimum* of preliminary and medical education, which shall duly qualify for successful practice; and it appears to us that the course of instruction prescribed in the regulations of the University of London, is a near approximation to this. Without being so complete, perhaps, as is desirable, it is, we think, as good as can be obtained under the defective system of teaching, which still prevails in our schools generally. In our opinion, the University requires nothing in their "Examinations in Arts," which may not easily be acquired by a youth of ordinary abilities, of seventeen or eighteen years of age; and we believe that the amount of professional knowledge required of the candidate for the first medical degree may be easily obtained during the five years subsequent to such preliminary training.*

Now, let us suppose that the London University, in addition to its present powers, were legally constituted the sole examining and licensing body for England; that similar bodies were established in Edinburgh and Dublin, with like authority for Scotland and Ireland respectively; and that the degree of *Bachelor of Medicine* were made the essential qualification for the practice of medicine and surgery; that, in fact, the Bachelors of Medicine, (or by whatever other name they might be recognized) should constitute the class of *General Practitioners;* we have thus, at once, the elements of the only reform that can be required or ought to be received by the members of an enlightened profession—a reform based on improved education, leading to improved knowledge and skill, and enhancing professional respectability and honour. According to this arrangement, the Bachelors of Medicine being the general practitioners, the Doctors of Medicine would constitute the higher ranks of the profession, and fill the offices now usually filled by physicians and surgeons, (or *pure* surgeons, as they are sometimes called.)

This arrangement, while recognizing the unity of the profession and the right of all to practise any of its branches, would still admit of those divisions into classes and those differences of rank, the convenience and utility of which have been sanctioned by experience. The equality of all the members of the profession would only be broken by the legitimate claims of superior talents, higher studies, or more enlarged experience.

Such a great change in the state of the medical profession as that contemplated in these observations and in the documents which precede, cannot, of course, be effected without the interference of the legislature; and is, no doubt, attended with great difficulties. But the necessity of reform is now so apparent, and the voice of the profession has been at length raised with such earnestness and energy, that government, we think, cannot help giving the subject their early and serious consideration. The simple fact, stated in one of the preceding documents, that there exist at this moment sixteen different examining and licensing bodies in Great Britain, each requiring a different amount of education, is sufficient to show the absolute necessity of legislative interference to put an end to a state of things which is little less than disgraceful in a civilized country. To carry such a scheme into effect much time, much patience and labour on the part of its advocates, and not a few sacrifices on the part of many of the existing public bodies, will be requisite; but we, for our parts, see nothing impracticable in it; and we regard the great ends to be obtained by its successful prosecution as justly claiming the utmost exertion of every individual who approves of it, and justifying the many and great sacrifices necessary for its accomplishment.

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* The only doubts we entertain on the subject are—1. Whether the prescribed education may not be found too expensive for the means of the present class of medical students; 2. Whether the means of the inhabitants of certain localities, particularly in country districts, may not be found inadequate to supply a fair remuneration to men who have had so expensive an education. It is to be recollected, however, that the abolition of apprenticeships, which must form part of the plan of reform, would cut off an important source of expenditure of money, as well as time.

It is not our intention, at present, to notice in detail, either of the two curricula given among the preceding documents; but we cannot pass by two most important improvements. In the Edinburgh "Propositions," it is recommended, "that a portion of the study should be allowed to be prosecuted under extra-academical teachers;" and it forms a characteristic of the London Curriculum, that the students have, in a very considerable degree, the option of choosing the particular Lectures they shall attend. This regulation, if generally adopted, will have the effect of removing all bad teachers from Universities and medical schools, by the very simple process of leaving their benches empty.

To our brethren in England and Scotland we would hold up the proceedings of their fellow-labourers in Ireland as worthy of all approbation and imitation. In their operations hitherto, crowned by the great congressional meeting in Dublin, they have shown so much zeal, energy, unanimity, and disinterestedness, as not only deserves but must obtain success. If they proceed, as we doubt not they will, in the course they have begun, they may depend on the warm sympathy and active co-operation of their brethren in England. Reform, to be successful in any one division of the empire, must be claimed by all—it must be general, not partial; and we are glad to observe that this is the view taken of it by the meeting in Dublin. "Although," said Dr. Bullen, "they attempted to found a national Irish faculty, they could not stand insulated as a national body. They had the medical institutions of England and Scotland, and they never could, and never ought, to attempt to carry any measure which would exclude the regularly-educated graduates of other British schools. It would ill become them, having entered within those walls, that their first step should be to carry out that principle of exclusion which they so unanimously condemned. Therefore, the movement that day was not exclusively an Irish one, but one that would be responded to by all the schools of England and Scotland. The principle which the Irish and other schools advocated, was to elevate and advance the medical profession by elevating and advancing medical education. It might be apprehended that they were only endeavouring to cloak another monopoly by the proceeding which they had undertaken, and the only way to avoid such a suspicion was to put forward in every way that they could, that the only test required of a man's competency was, that when the individual was to receive the degree giving him a licence to practise any of the departments of the medical profession, that degree should be a *boná fide* test of his intellectual capacity and educational acquirements. If they adopted this principle, no government that valued education in man or recognized it as important to the great interests of society could refrain conscientiously and consistently from uniting with them in saying that a cry for medical educational reform had been raised which should be carried out."

But, moreover, we think it essential to the success of this great measure, not merely that the reform shall be founded on an improved education and higher qualifications in the candidates for licences to practise, but that the body which regulates the education, tests its results, and grants the licences, shall be restricted to the exercise of these duties, having nothing whatever to do either with the direct communication of instruction to students, or with the conduct or proceedings of candidates after they have received their degrees or licences. Other bodies of course, whether under the name of simple schools or chartered colleges, must exist for the purpose of imparting instruction; others, again, for purposes of a scientific or merely professional kind, like our present colleges of physicians and surgeons, the college of pharmacy proposed in the Dublin resolutions, &c. &c., but the all-important institution, and the establishment of which must be looked upon as the great object of the efforts now making, is, as Dr. Henry well expressed it at the Dublin meeting, "a UNIVERSITY rather than a College,—not a teaching, but a licensing body, that should have a relation to the existing colleges, somewhat similar to that which exists between the university of Oxford and its component colleges." It is to the establishment of such a body as this in Dublin and Edinburgh, with exclusive powers of regulating medical education, examining candidates, granting degrees, and licensing to practise; that the chief efforts of the profession should, we conceive, be now directed in Ireland and Scotland; and it is for the addition of the exclusive licensing power to those powers already possessed by the new university of London that the reformers of England should mainly strive. For conferring these powers on the three metropolitan institutions an act of the legislature is essential; all the other arrangements of the profession as a body might, we think, be carried into effect afterwards by the efforts of the members themselves, or, at least, without any legislative enactment. In our view of the subject, all authority of the universities should cease with the act of licensing, and then only should the authority of such bodies as colleges of physicians and surgeons begin. The objects of such colleges should be altogether different from those of the universities. They would be reunions of the members of the profession for scientific and professional purposes, to which all ought to have equal access, and in which all should have the same privileges. They would constitute, as it were, the parliament of the profession in their respective divisions of the kingdom. They would be the guardians of the rights of the members, as well as of their honour and respectability; they would promote the cultivation of science by their countenance, by their funds, and by their honorary distinctions; they would be the centre and medium of professional union; in a word, they would protect and promote, in every way, the interests of a profession composed of men all liberally educated and all possessing an equality of rights. For the better accomplishment of these and many other most important objects, it would, no doubt, be highly desirable that the present colleges of physicians and surgeons should be united into one institution, in each of the three capitals. In these the members might still divide themselves into sections, for the cultivation of different departments of medical science, although it is obvious that the science itself can only be divided into different parts by artificial violence.

PROVINCIAL MEDICAL AND SURGICAL ASSOCIATION.

It is gratifying to observe the progressive growth, in extent and usefulness, of this excellent society. The members now amount to 1,200. It comprehends six district branches, all of which are in a flourishing condition. These district branches are local *reunions*, established for the purpose of promoting the general objects of the Association, and furnishing some substitute for the general meetings to those members who cannot follow the latter in their annual migrations. The *Bath District Branch* held its third annual meeting at Bath, on the 6th of June, and was well attended. Mr. Soden was president. The members met in the forenoon, at the Literary and Scientific Institution, and afterwards dined together. Dr. Barlow, the secretary, read a very satisfactory report, from which it appeared that the number of members amounts to 84. The *Southern District Branch* held its annual meeting at Portsmouth, on the 13th of June, and was attended by members from Salisbury, Winchester, Southampton, Chichester, and all the other towns within the district; the number present being upwards of 80, including several medical gentlemen, not members. Dr. Quarrier was president. The meeting was held in the Guildhall, and the members afterwards dined together. Several interesting communications were made to the society, and a most admirable report on the state of surgery, during the past year, was read by Mr. Newnham, of Farnham, and which he was unanimously requested to publish, for the benefit of the profession generally. This request, we are happy to say, Mr. Newnham and his coadjutors, Mr. Wickham, of Winchester, and Mr. Salter, of Poole, complied with; and the publication of the report has accordingly taken place: the profits to be given to *The Benevolent Fund* of the Association. The next meeting of the branch was fixed for Chichester, and Dr. Forbes was appointed president.

The seventh anniversary meeting of the Association will be held at Liverpool, on the 24th and 25th of the present month (July), under the presidency of Dr. Jeffreys. It is expected to be the fullest that has yet taken place, not merely from the increased number of the members of the Association, and populousness of the place of meeting, but from the great importance of some of the subjects to be discussed; more particularly those of *vaccination* and *medical reform*. This meeting will also possess an attraction of a peculiar kind, which, although having more of a personal than a professional character, cannot fail to interest every one belonging to the association; we may indeed say, every lover of his profession, and every admirer of what is excellent and honorable in its members. We refer to the presentation to his family of a Portrait of Dr. Hastings, by the members of the association. We cannot better advert to this subject than by using the words of Dr. Barlow, at the late meeting at Bath. "The District Council (he said) wish to advert to the slight tribute of gratitude offered to the respected and estimable founder of the association, Dr. Hastings. The portrait of him designed as a present to his family, is completed; and the engravings are expected to be ready for delivery to the subscribers at the ensuing general meeting. If the claims of Dr. Hastings on the gratitude of the association were to be estimated at their real amount, no recompence could adequately remunerate the services which he has rendered, and continues to render. For these, his best reward must be derived from the heart-cheering consciousness of talents and energies ably and zealously devoted to a most praiseworthy and beneficial end. To those for whom he has laboured, it cannot but be gratifying to possess a memorial of one to whom they are so deeply indebted; and no memento could be more appropriate or interesting than one which will keep continually in view the intelligent and expressive countenance of their benefactor and friend."

EXAMINERS OF THE LONDON UNIVERSITY.

In a preceding article, on Medical Education and Reform, we have given strong proofs of our being sincere and zealous friends of the University of London; and we have now much pleasure in adding that we think the medical members of the senate are entitled to the gratitude of the profession for the admirable curriculum of education, preliminary and professional, which they have established for the candidates for university degrees. We need hardly say, then, that it is with extreme regret that we have to express our conviction that the University, in one of its first steps towards carrying its regulations into effect, has committed a serious mistake. As the terms of the curriculum leave it, in a very considerable degree, optional with the candidates to obtain the requisite knowledge where and in what manner they please, it follows, as a matter of course, that, in order to stamp the degrees with anything like distinction, or even with a current value, the Examinations should possess a high character for extensiveness, strictness, and impartiality, and the Examiners should be men of acknowledged eminence. Now, we presume to doubt if the senate have kept sufficiently in view these important truths, in the recent election of examiners, more particularly in the departments of Medicine and Surgery. While freely admitting the unexceptional personal and professional character of the gentlemen chosen in these departments, we refuse to acknowledge them as possessing that degree of eminence in either literature or science which can give any particular distinction to the tribunal in which they are judges. We are glad, for the sake of the University, that these appointments are only for a single year; and we warn the Senate that, if the golden rule of *Detur Optimo* be not rigidly adhered to in all their future elections, the glory of the Institution will be eclipsed in its very dawn.

FAREWELL DINNER TO DR. CONOLLY,

ON HIS LEAVING WARWICKSHIRE.

It is always an agreeable duty with us to record proceedings of a public character which do honour to our profession or to its members; this duty is doubly pleasant when, as on the present occasion, such proceedings have reference to a most dear and much valued friend. The official relation in which we have so long stood to Dr. Conolly being broken by his appointment to Hanwell, we might be supposed at liberty to indulge our feelings by stating how much we have profited by his kind assistance during our long and happy association, and how deeply we prize the friendship which we trust no change of place or of fortune will ever interrupt; but we are restrained by an apprehension of giving pain to that modesty which forms as marked a feature in the character of our friend as the great talents and virtues by which it is accompanied. Our regret at losing the cooperation of Dr. Conolly in the editorial department of this Journal—and we are happy in believing, in the *editorial* department only—is counterbalanced by the conviction that he is at length placed in a situation peculiarly suited to his tastes and talents.

and by the confident belief that that most important department of medicine to which his attention will be now almost exclusively devoted, will derive very great advantages from his future labours.

We extract the following account literally from a Warwickshire paper, and regret that our limits permit us to insert only a small part of the original Report:

> We have very great pleasure in publishing a condensed report of the proceedings of this interesting meeting, which took place at the Town Hall, Stratford-on-Avon, on Monday last. The entertainment originated with the members of the Shakspearean Club, who were anxious, without any formal or public notification, to testify their deep sense of Dr. Conolly's zealous attention to the interests of that society. We were pleased to observe but one feeling pervading the assembly, viz., that of satisfaction that so honorable an appointment as that of Resident Physician of the Middlesex Lunatic Asylum, should have devolved upon one, whose accomplishments as a scholar and whose virtues as a gentleman had long since endeared him to a numerous circle of friends and admirers. It will afford some satisfaction to every true Shakspearean resident in this neighbourhood to know that, although removed from the sphere of his early and active exertions in its behalf, the society may encourage a hope of occasionally finding Dr. Conolly a participator in their annual tributary honours to the memory of Shakspeare."
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> After the usual toasts the Chairman, W. J. Harding, Esq., of Baraset House, observed, "that the next toast was intimately connected with the object for which they were assembled. Having been requested by the members of the Shakspearean Club to preside on that occasion, he must entreat their indulgence for any inadequacy which might appear in the performance of the duty he had undertaken. There were many members present who would address the meeting, and do that justice to the subject of the toast which he candidly acknowledged he felt himself incompetent to perform. He would invite them to drink the health of their worthy and talented guest, Dr. Conolly. Filling the situation which he then occupied, he felt himself called upon to express, however inadequately, the sentiments of the Shakspearean Club towards that most excellent and respected man; and, although he might fail in expressing them in full force, it was not difficult to know what they were; they were to be found in one common feeling of admiration, gratitude, and respect. (Great applause.) Had that meeting been considered a public one, neither that room nor any other in the neighbourhood would have been sufficiently extensive to admit the numerous assemblage of friends and admirers of their respected guest, who would have dined with the society on such an occasion; but the meeting was confined to the members of the Shakspearean Club, and a few of their friends, resident in Stratford or its immediate vicinity, who were anxious to do honour to the public and private virtues of Dr. Conolly: and was to be regarded rather as the expression of the social and kind feelings of the club, than any public demonstration of that respect which would have awaited Dr. Conolly, had the assembly assumed a more extended character. There was no gentleman merited better the honour then conferred upon him; which, from the modesty which had ever been his greatest characteristic, would be more agreeable to him than any more ostentatious mode of expressing their feelings towards him. Notwithstanding they all regretted that the present occasion was to be regarded as a leave-taking, they had the satisfaction of knowing that Dr. Conolly had recently received an appointment in every way suitable to his wishes, and which might, indeed, be looked upon as a fitting reward for his talents, and a public recognition of those merits which had so long and so highly been estimated by his friends. The gentlemen with whom the appointment rested, viz., the Magistracy of Middlesex, had, in his (the Chairman's) opinion, conferred great honour upon themselves in selecting a gentleman who had devoted, during a long series of years, so much attention to that particular branch of medical science, and was, in every way, eminently qualified to protect and relieve the unfortunate individuals whose distressing maladies might place them under his care. In reference to the Shakspearean Club, they could not forget how much they were indebted to the residence of Dr. Conolly amongst them for a promotion of that and other institutions upon which they now set a high value. During his residence in the town of Stratford, he became universally beloved by all classes of society and the inhabitants, of which there could be no more gratifying proof than the presentation of a piece of plate by Sir Gray Skipwith, in the name of his townsmen and friends. He (the Chairman) was not in the kingdom at that time, but he recollected that, on his return, he universally heard Dr. Conolly spoken of as a skilful and benevolent physician, and an indefatigable friend to the poor and afflicted. He afterwards witnessed the Dr.'s great and assiduous attention in promoting the adornment of the Chancel, and the preservation of the remains of Shakspeare and his family. Nothing could exceed his constant attention at the meetings of the Committee, where his ser-

vices were of the most invaluable character. Dr. Conolly had left Stratford with the regret of all classes, and at Warwick and Birmingham, where he had since resided, he was distinguished for his ardent promotion of similar objects, and was equally and generally beloved. Indeed, he was a man of such commanding talent and philosophic character, of so much modesty of demeanour and amiability of disposition, that it was impossible that he could be otherwise than admired, respected, and beloved. In fact, whether as a public man or as a private individual, few could approach him. (Great cheering.) In these times of improvement, when time and space were so much abridged, he could not but cherish the hope that they should often see Dr. Conolly at their annual celebration of the Poet's birthday. How often had they been rendered happy and delighted, when the Doctor had, from that chair, expatiated on the character and writings of the Immortal Bard, with all his admirable taste and eloquence of language; and he could not think of those occasions without still wishing that they might often have the opportunity of seeing him amongst them on the 23d of April. (Cheers.) He proposed the health of Dr. Conolly, with three times three: might he live long to enjoy an appointment for the duties of which he was so admirably qualified."—The toast was drank amidst the most deafening applause.

"Dr. Conolly rose to return thanks for the honour done to him. When he received the kind invitation of the club, his reply had been that he had known the members too long, and had experienced their kindness too often, to require any formal manifestation of it. But he must confess that the manner in which they met him that day was deeply gratifying to him. They had known him long and intimately; through good and evil report they had been his steadfast friends; he had come among them, many years ago, a stranger to them all; the best, the happiest, the most energetic of his years had been passed among or near them; his hair (he might literally say) had grown gray among them: his children had grown up among them; and well might he prize the cordial and honest testimony of those to whom so much of his life and conduct was necessarily known. And, he must add, no man could practise the profession of medicine in any community for many years, without witnessing such changes and chances in the families of his friends and neighbours, and so many scenes calculated to call forth the deepest and strongest affections, as to awaken and daily strengthen in his mind those bonds of peace and charity which were the most suitable to those engaged in the same pilgrimage. Therefore it became doubly grateful to him to hear the kind accents in which they were pleased to mention his humble name and services, particularly as expressed so cordially, so courteously, but he feared, as regarded the object of them, so undeservedly, by their esteemed chairman. In that very room, sixteen years ago, he had met several of the inhabitants of the town and neighbourhood, for the purpose of instituting the dispensary. Many of its original supporters now sleep in the grave; but it was cheering to see that, although the individual actors in charitable, as in other works, must fade and decay, the work of charity itself went on and prospered. The dispensary had grown into an infirmary, which would be the means of affording important aid to the sick poor of the town and neighbourhood for ages to come. On another occasion, eleven years since, he had with much satisfaction assisted in that room in forming a Society for Reading and Lectures, which he knew had already been useful, and which he hoped would hereafter become much more so. In a neighbouring town he had had the gratification of being one of those who assisted to form the Warwickshire Natural History Society, the great success of which was to be ascribed to the extraordinary exertions of a few scientific individuals, to whom he should always feel very grateful, aided by the general interest and patronage of the inhabitants of the county. The pleasure he derived from contemplating these and other provincial institutions with which he had been accidentally connected arose not so much from their actual condition as from a belief that they were only rudimental fragments of future provincial academies or colleges, the advantages of which, unknown in the provinces of England, had long been enjoyed in other countries, by no means rivalling this country in other particulars. The first humble labourers in that rich field might then be forgotten, but the seed they planted would grow and flourish, and diffuse precious fruit throughout the land. (Cheering.) Again, looking back on Stratford recollections, he was reminded how many times he had in that room met many of them, deeply interested like himself, in devising the means of preserving the venerable fabric wherein reposed the sacred ashes of Shakspeare; to possess which was the boast of Stratford, and might well be the envy of the world. (Renewed cheering.) It delighted him to reflect that out of those efforts sprung the great effort now making by the inhabitants to preserve the whole of their beautiful parish church from decay; and within its walls, for countless centuries, he trusted their descendants would assemble to offer a pure worship, passing by, not ungratefully, the tombs of those who prevented the whole building falling into ruins. He never forgot that these designs began and were fostered in the club; and it was because the club had ever entertained such useful and liberal designs and had ever promoted friendly feelings among the townspeople, and encouraged a temperate and honest conviviality, that he had at all times felt pleasure in meeting the members and contributing his best assistance to them. When now reviewing, as all must occasionally do, a distinct portion of his life, and asking, as all must occasionally ask, the meaning of this swiftly-passing dream, feeling that one passage of it was closing with this evening, and wishing to close it candidly and fairly with them, and peaceably with his own conscience, he felt that to one charge they might possibly think his conduct open; for if he had been so kindly treated in Warwickshire, it might be thought he ought to have made a fortune, and remained among them. His answer to this was, that to make a fortune, a physician required not only a larger population around him, but a habit of pursuing his profession a little more eagerly than he had perhaps been disposed to do; for he very early found out that much consideration was due from a physician not only to the squalid poor, but to many persons of decent and respectable station, to whom sickness brought many sacrifices, seldom avowed, but too clearly seen by a humane observer. He must add that he had a firm, although not a presumptuous trust in Providence; and if he could leave his children the legacy of an unblemished fame, he should have a better hope of their welfare and happiness than if he left them the largest fortune that grasping avarice could extort from suffering poverty. (Great applause.) And this subject reminded him that there was one topic, never touched upon before by him, because he felt that he could not heretofore touch upon it with propriety and dignity, and yet on which, now that it could no longer affect his interest, he would beg leave to say a few, a *very* few words. He knew that a belief had prevailed in the neighbourhood, how originating and how spread about he neither knew nor heeded, that on leaving Stratford he had made his friends there the subject of a gainful bargain, and on returning to Warwickshire had contravened the terms of that bargain. All the evil which this report could do him, or was intended to do him, was now done. It would no more affect him. He would only say that *there was not one word of truth in it from beginning to end.* Those around him well *knew* that it was not true; that it *could not* be true; that such conduct was abhorrent to his nature, and contradicted by his whole life. (Applause.) He wished to make no accusations; he would utter no reproaches; he fully and sincerely forgave those who had done him undeserved injury; and he never meant to allude to the subject more: but if such had been his conduct, he should have ill deserved, and could not have obtained, the favour and good opinion of his own profession, which he felt had ever attended him, and for which, strongly and widely expressed, and without his soliciation, he was solely indebted for the appointment which was the occasion of his leaving them. To possess and to deserve the esteem of the members of the medical profession, and to be an honorable member of it, had ever been, and would ever be, the first worldly object of his care, and the highest object of his pride. By the diligent exercise of the duties and virtues of that profession, as far as was in his power, he should ever seek his chief happiness, and must hope to secure honorable reputation." &c.

BOOKS RECEIVED FOR REVIEW.

1. Elements of Medical Jurisprudence. By Theodore R. Beck, M.D. and John B. Beck, M.D. Sixth Edition.—Philadelphia, 1838. 2 vols. 8vo, pp. 670, 743.

2. A System of Anatomy for the use of Students of Medicine. By Caspar Wistar, M.D., late Professor of Anatomy in the University of Pennsylvania. With Notes and Additions by W. E. Horner, M.D. Seventh Edition, entirely remodelled and illustrated by numerous engravings, by J. Pancoast, M.D., Lecturer on Anatomy and Surgery, &c.—Philadelphia, 1839. 2 vols. 8vo, pp. 491, 560.

3. Political Medicine; being the substance of a Discourse lately delivered before the Royal College of Surgeons in Ireland, on Medicine, considered in its relations to Government and Legislation. By H. Maunsell, M.D. — Dublin, 1839. 8vo, pp. 45.

4. A Letter to the Secretary-at-War, on Sickness and Mortality in the West Indies; being a Review of Capt. Tulloch's Statistical Report. By Sir Andrew Halliday, M.D. F.R.S.E., Dep. Inspector of Hospitals. London, 1839. 8vo, pp. 63. 2s.

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CONTENTS OF No. XVI.

OF THE

British and Foreign Medical Review.

OCTOBER, 1839.

PART FIRST.—Analytical and Critical Reviews.

PAGE

ART. I.—Leçons sur les Phénomènes Physiques de la Vie, professées au Collége de France. Par M. Magendie. Recueillies par C. James et G. Funel . 309
Lectures on the Physical Phenomena of Life, delivered at the College of France. By M. Magendie, and collected by C. James and G. Funel.

ART. II.—1. Lehrbuch der Arzneimittellehre. Von Dr. C. G. Mitscherlich . 353
A Manual of Materia Medica. By Dr. C. G. Mitscherlich.
2. The Elements of the Materia Medica; comprehending the Natural History, Preparation, Properties, &c. of Medicines. By Jonathan Pereira, F.R.S. ib.
3. Die Neuesten Entdeckungen in der Materia Medica. Von Dr. J. H. Dierbach ib.
The latest Discoveries in the Materia Medica. By Dr. J. H. Dierbach.
4. Von dem Wirkungen der Gebräuchlichen Metalle auf den menschlichen Organismus überhaupt, und als Heilmittel; und dem Kupfersalmiak Liquor, und andern Kupferpräparaten als solcheninsbesondere. Von Dr. Köchlin ib.
Of the Effects of the Metals in common use upon the Human Organism generally, and as Remedial Agents; and of the Cupro-Ammoniacal Liquor, and other preparations of Copper, as such, in particular. By Dr. J. R. Köchlin.
5. Physiologisch-Therapeutische Untersuchungen überdas Veratrin. Von Dr. F. A. Forcke . . . . ib.
Physiological and Therapeutical Researches concerning Veratrine. By Dr. Forcke.

ART. III.—Die geburtshülfliche Auscultation. Von Dr. Herm. Franz Naegele 365
Obstetric Auscultation. By Herman Francis Naegele, M.D.

ART. IV.—Das Hirn des Negers, mit dem des Europäers und Orang-Outangs verglichen. Von Dr. F. Tiedemann 374
The Brain of the Negro compared with that of the European and the Orang-Outang. By Dr. F. Tiedemann.

ART. V.—1. Beiträge zur operativen Orthopädik, oder Erfahrungen über die subcutane Durchschneidung verkürzter Muskeln und deren Sehnen. Von Dr. Stromeyer 384
Contributions to Operative Orthopædia; on the Subcutaneous Division of Contracted Muscles and their Tendons. By Dr. Louis Stromeyer.
2. Handbuch der Plastischen Chirurgie. Von Dr. Eduard Zeis . . ib.
Manual of Plastic Surgery. By Dr. Edward Zeis, of Dresden.
3. Mémoire sur la Section du Tendon d'Achille dans le Traitement des Pieds-bots. Par M. Bouvier 385
Memoir on the Section of the Tendo-Achillis in the treatment of Club-foot. By M. Bouvier.
4. A Treatise on the Nature of Club-foot, and analogous Distortions, including their Treatment. By W. J. Little, M.D. . . . . . ib.

ART. VI.—Recherches Medico-physiologiques sur l'Electricité Animale. Par J. F. Coudret, M.D. 406
Medico-physiological Researches on Animal Electricity. By J. F. Coudret, M.D.

ART. VII.—1. Handwörterbuch der gesammten Chirurgie und Augenheilkunde. Herausgegeben von Ernst Blasius 413
Dictionary of General and Ophthalmic Surgery. Edited by Dr. E. Blasius.
2. The Cyclopædia of Practical Surgery. Edited by William B. Costello, M.D. (Art. Aneurism, by James Wardrop, M.D.) . . . . ib.

ART. VIII.—1. Diagnostisch-praktische Abhandlungen aus dem Gebiete der Medicin und Chirurgie; durch Krankheitsfälle erläutert. Von Dr. Löwenhardt . 419
Diagnostic and practical Essays on Medical and Surgical Subjects. By Dr. Löwenhardt. Part II.
2. Versuche für die praktische Heilkunde. Von Ferdinand Jahn . . ib.
Essays on Practical Medicine. By Ferdinand Jahn. Part I.

§

PAGE

ART. IX.—1. Deux Mémoires, en réponse à cette question—"Faire connoitre les Analogies et les Differences qui existent entre le Typhus et la Fièvre Typhoide." Par MM. C. E. S. G. DE CLAUBRY et J. J. H. MONTAULT . 425
On the Analogies and Differences between Typhus and Typhoid Fever. By MM. GAULTIER DE CLAUBRY and MONTAULT.
2. A Report founded on the cases of Typhoid Fever, or the common continued Fever of New England. By JAMES JACKSON, M.D. . . . ib.
3. A Short Treatise on Typhus Fever. By G. L. ROUPELL, M.D. . . ib.
4. Etudes Cliniques sur divers points de l'Histoire des Fièvres Bilieuses et Typhoides. Par le Docteur H. C. LOMBARD . . . . ib.
Clinical Remarks on certain points in the History of Bilious and Typhoid Fevers. By Dr. LOMBARD.
5. Der Typhus und dessen Erscheinungen, oder die Typhoseptosen, pathogenetisch und therapeutisch erläutert. Von L. BUZORINI, M.D. . . ib.
Typhus and its Phenomena, or the Typhoseptoses; pathologically and therapeutically illustrated. By L. BUZORINI, M.D.
6. Das Blutfieber, vorzuglich in seiner Verbindung mit einigen Krankheiten des Darmkanals; pathogenetisch und therapeutisch erläutert. Von F. KEHRER. ib.
Blood-Fever, especially in its Connexion with certain Diseases of the Alimentary Canal; pathologically and therapeutically illustrated. By Dr. KEHRER.

ART. X.—A Treatise on the Diseases of the Heart and Great Vessels, and on the Affections which may be mistaken for them; comprising the Author's View of the Physiology of the Heart's Action and Sounds. By J. HOPE, M.D. F.R.S. 445

ART. XI.—The Accoucheur; a Treatise on Protracted Natural Labours, Suspended Animation in new-born Infants, Uterine Hemorrhage, &c. By JOHN CRAIG 467

ART. XII.—1. Lehrbuch der Physiologie des Menschen. Von Dr. F. ARNOLD . 476
System of Human Physiology. By Dr. FREDERIC ARNOLD.
2. Lehrbuch der pathologischen Physiologie. Von Dr. WILHELM ARNOLD . ib.
System of Pathological Physiology. By Dr. W. ARNOLD.

ART. XIII.—Medical Notes and Reflections. By HENRY HOLLAND, M.D. F.R.S., &c. 482

ART. XIV.—On the Physiological Inferences to be deduced from the Structure of the Nervous System in the Invertebrated Classes. By W. B. CARPENTER, M.D. 508

ART. XV.—1. Selections in Pathology and Surgery; or, an Exposition of the Nature and Treatment of Local Disease, exhibiting new pathological views, and pointing out an important practical improvement. By JOHN DAVIES . 512
2. Practical Remarks on the use of Iodine locally applied in various Surgical Diseases and External Injuries. By JOHN DAVIES, &c. &c. . . ib.
ART. XVI.—An Enquiry into the Morbid Effects of Deficiency of Food, chiefly with reference to their occurrence among the Destitute Poor; also Practical Observations on the Treatment of such cases. By R. B. HOLLAND, M.D. . 524

PART SECOND.—Bibliographical Notices.

ART. I.—Practical Observations on Diseases of Women. By WILLIAM JONES, M.R.C.S. 530

ART. II.—The Unity of Disease analytically and synthetically proved: with Facts and Cases subversive of the received Practice of Physic. By S. DICKSON, M.D. 531

ART. III.—First Annual Report of the Registrar-General of Births, &c. in England 532

ART. IV.—Physiology, for the Use of Elementary Schools. By C. A. LEE, M.D. 533

ART. V.—Six Mois de Séjour en Angleterre. Par SIRUS PIRONDI, D.M. . 534
Six Months' Residence in England in 1836. By SIRUS PIRONDI, M.D.

ART. VI.—The Surgeon's Vade Mecum: a Hand-book of the Principles and Practice of Surgery. By ROBERT DRUITT, M.R.C.S. . . . 536

ART. VII.—1. Essay on Cruelty to Animals. By JAMES MACAULAY, M.A. . ib.
2. The Animals' Friend; or, the Progress of Humanity. No. VII. . . ib.

ART. VIII.—On the Enlisting, Discharging and Pensioning of Soldiers, with the Official Documents. By HENRY MARSHALL, F.R.S.E. . . 537

ART. IX.—School Botany; or an Explanation of the Characters and Differences of the principal Natural Classes and Orders of Plants belonging to the Flora of Europe. By JOHN LINDLEY, PH.D. F.R.S. 538

PAGE

Art. X.—The Surgical Anatomy of the Arteries, and Descriptive Anatomy of the Heart; together with the Physiology of the Circulation in Man and Inferior Animals. By Valentine Flood, A.M. M.D. T.C.D. &c. . 538

Art. XI.—The American Medical Library. Edited by R. Dunglison, M.D. . 539

Art. XII.—An Experimental Enquiry concerning the presence of Alcohol in the Ventricles of the Brain, after Poisoning with that liquid; together with Experiments, illustrative of the Action of Alcohol. By John Percy, M.D. ib.

Art. XIII.—Illustrations of Cutaneous Diseases. By Robert Willis, M.D. . 541

Art. XIV.—1. A Series of Anatomical Sketches and Diagrams, with Descriptions and References. By Thomas Wormald and Andrew M. M'Whinnie . 542
2. The Surgical Anatomy of the Groin, the Femoral and Popliteal Regions. By Thomas Morton . . . . ib.

Art. XV.—Observations on the Disorder of the General Health of Females called Chlorosis, showing the true cause to be entirely independent of Peculiarities of Sex. By Samuel Fox . . . . . . ib.

Art. XVI.—The Collected Works of Sir Humphry Davy, Bart., LL.D. F.R.S. Edited by his Brother, John Davy, M.D. F.R.S. Vol. I. . . 545

Art. XVII.—Retrospect of the Progress of Surgical Literature for the Year 1838-9. By Messrs. Newnham, W. Wickham, and Salter . . . 546

Art. XVIII.—Political Medicine; the substance of a Discourse on Medicine, considered in its relations to Government and Legislation. By H. Maunsell, M.D. 547

Art. XIX.—Medical Lexicon; a new Dictionary of Medical Science, containing a concise Account of the various Subjects and Terms; with a Vocabulary of Synonymes, &c. By Robley Dunglison, M.D. &c. . . 548

Part Third.—Selections from the British and Foreign Journals.

I. THE FOREIGN JOURNALS.

ANATOMY AND PHYSIOLOGY.

M. Simon on the Physiology of Nutrition 549
Dr. Marchand on the Production of Urea in the Animal Body . . 550
Dr. Bulard's Observations on the Plague . . . . . ib.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Dr. Otto's Case of Apoplexia Cutanea 552
M. Lenepveu's Case of Chronic Farcy terminating in Acute Glanders . . ib.
M. Gouzee on Cod-liver Oil and Ray-liver Oil in Scrofulous Affections . 554
Dr. Albers on Fatty Disease of the Liver (Pimelosis Hepatica) . . ib.
Dr. Schlesier on the Medical Use of Alum, in Diseases of the Heart . . 555
M. Bonetti on Arteritis 556
Dr. Toulmouche's Experiments on Sulphuret of Antimony . . ib.
Dr. Malin's Contribution to Psychical Anthropology . . . 557

SURGERY.

Dr. Guerin on the Treatment of Lateral Curvatures of the Spine . . 557
Statistics of Tracheotomy 558
Dr. Defer on Gangrene resulting from the employment of a Starched Bandage . ib.
Dr. Roux on the Superiority of M. Reynaud's Operation for Circocele . . ib.
Dr. Remak on Torsion of the Arteries 559
M. Bouvier on Congenital Dislocations of the Femur . . . ib.
Dr. Löwenhardt on Chronic Hydrocephalus cured by Compression of the Head . 560
M. Malgaigne on the Treatment of Inguinal Hernia by Trusses . . 561

MIDWIFERY.

Singular Case of a Woman delivered of Five Children . . . 564
Drs. Bunsen and Kyll on Morbid Accumulation of the Liquor Amnii . ib.
Prof. Kilian on the Galvanic Obstetric Forceps 566
Singular Case of Malformation . . . . . ib.
Dr. Schwabe's Case of Mole Pregnancy with Spontaneous Amputation . 567
Dr. Carganico's Case of Spontaneous Evolution . . . . ib.
Case of Difficult Delivery of Twins which were united by the Breast . . 568

PAGE

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

Dr. **Lachese**, Jun. on the Action of Arsenious Acid . . . 568
M. **Devergie** on some New Signs of Suspension having taken place during Life . 572
Dr. **Malle's** New Process for the Separation of Arsenic in Organic Mixtures . 573
On Hydrated Peroxyde of Iron as an Antidote to Arsenic . . . ib.

MEDICAL STATISTICS.

Dr. **Hoffman** on Longevity and Mortality in Prussia . , . 575

II. THE AMERICAN AND COLONIAL JOURNALS.

ANATOMY AND PHYSIOLOGY.

Dr. **Fisher's** Contributions Illustrative of the Functions of the Cerebellum . 576
Dr. **Bolton** on the Physiology of the Iris 580
Prof. **Dunglison** on the Pulsations of the Fœtal Heart and the Umbilical Cord . 581

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Dr. **Somervail** on the Therapeutic effects of Tobacco applied externally . 582
Dr. **Evans's** Account of the Asylum near Frankford, Pennsylvania . . 583

SURGERY.

Dr. **Hosack's** Cases of Sensitive Tumours of the Female Urethra, with Remarks . 585
Dr. **Barton's** Views and Teatment of an important Injury of the Wrist . 588
Dr. **Billingslea** on the Use of Iodine in Herpes . . . 591

MIDWIFERY.

Prof. **Dunglison** on the Temperature of the Vagina and Os Uteri during Labour ib.

MEDICAL STATISTICS.

Classification of Fracture Cases treated at the Native Hospital, Calcutta . 592
Diseases of House-Patients treated at the Native Hospital, Calcutta . . ib.

III. THE BRITISH JOURNALS.

ANATOMY AND PHYSIOLOGY.

Dr. **Hake** on the Structure and Functions of the Spleen . : . 593
Dr. **Alderson** on the Difference of Colour in different parts of the Bodies of Animals ib.
Mr. **Paget** on the Blood-vessels of Tendinous Tissues . . . 594
Dr. **Reid** on the Effects of Lesion of the Trunk of the Ganglionic System of Nerves ib.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Dr. **Simpson** on the Origin of Hernia and Malformation in the Fœtus . . 594
Dr. **Henderson** on the Result of Experience in Sea-Scurvy . . . 595
Dr. **Cahill's** Observations on the Treatment of Delirium Tremens without Opium ib.
Mr. **Dewar's** History of a Singular Convulsive Disease . . . ib.
On the Treatment of Strangulated Hernia on the plan of Dr. O'**Beirne** . 596
Mr. **Phillips's** Case of Section of the Hamstring Tendons . . ib.
On the Treatment of Prolapsus Uteri by means of the Actual Cautery and Caustics.
  1. Mr. **Phillips** on a new and successful Method of treating Prolapsus Uteri . ib.
  2. Letter from Dr. **Evory Kennedy** on the use of Caustics in Prolapsus Uteri · ib.

MIDWIFERY.

Mr. **Terry's** Case of Restoration of Animation in a Still-born Child . . 596
Dr. **Churchill's** Researches in Operative Midwifery . . . 597

MATERIA MEDICA AND CHEMISTRY.

Mr. **Nichols's** Economic Formula for Hydriodate of Potash . . . 597

TOXICOLOGY.

Experiments with the Wourali Poison. With Dr. Clanny's Remarks . . 597

PART FOURTH.—Medical Intelligence.

Provincial Medical Association 603
British Association for the Advancement of Science . . . 610
BOOKS RECEIVED FOR REVIEW. 617

THE
BRITISH AND FOREIGN
MEDICAL REVIEW,

FOR OCTOBER, 1839.

PART FIRST.

Analytical and Critical Reviews.

Art. I.

Leçons sur les Phénomènes Physiques de la Vie, professées au Collége de France. Par M. Magendie. Recueillies par C. James et G. Funel. —*Paris*, 1836-8. 4 Tomes, 8vo, pp. 310, 376, 471, 400.

Lectures on the Physical Phenomena of Life, delivered at the College of France. By M. Magendie, and collected by C. James and G. Funel.—*Paris*, 1836-8. Four Vols. 8vo.

In the writings of most physiologists, from the earliest times to the present, one of two very opposite but equally erroneous tendencies may be observed. While some have regarded the phenomena of life as resulting, in all instances, from the operation of laws entirely different from those which influence inert matter, others have as exclusively resorted to the various branches of physical science for the solution of phenomena which these are utterly inadequate to explain. Now it requires but a moderate degree of observation to become convinced that, although there are powers at work in the living organism, which produce results quite different from any presented in the inorganic kingdom, and which we may therefore reasonably presume to be distinct and peculiar in their nature—many processes are nevertheless carried on, which, though conducive to vital ends, and under the control of the hidden powers of life, are, in their immediate nature, purely physical. However obvious this truth may appear when clearly stated, it has in general been absent from the speculations of physiologists; so that although a host of ingenious minds have been occupied in bringing physical laws to bear on vital phenomena, which such laws cannot explain, few have cared to apply them to a large range of phenomena in the living body, which they can explain, and which in truth are explicable in no other way.

To distinguish the *physical* phenomena of living bodies from the *vital*, and to illustrate the former, and refer them to the laws from which they result, are the professed objects of M. Magendie in the course of lectures before us; and these objects are stated, at the commencement, with sufficient precision. Judging from the author's reputation as a physiologist, the knowledge of chemistry evinced in some of his writings, and his experience in the practice of medicine, it might be thought that the subject could not have fallen into better hands. In the progress of the present work, however, we meet with too many proofs that a man may possess great ingenuity and considerable acquirements, with very small capacity —we had almost said, a total incapacity—of sustained reasoning. Led astray by a predominating idea, M. Magendie continually loses sight of the distinction which he admits as the basis of the enquiry, and deviates from the path he has himself prescribed. Instead of carefully separating the physical phenomena of life from the vital ones with which they are so intimately associated, his chief anxiety seems to be to make out as many physical and as few vital as possible.

The mode of investigation he adopts is much better calculated to strike an audience with momentary conviction than to enlarge the real limits of science. It consists chiefly in experiments, by which it is made evident to the senses that certain phenomena take place under certain circumstances; but little pains are bestowed to ascertain what would have taken place under other circumstances. The fact auspicious to the preconceived view is seized upon, and a hasty generalization set on foot. Indeed, we find ourselves obliged to declare, that we know not, in the whole range of modern scientific literature, a work which contains so frequent violations of the inductive principle, or such rash generalizations from isolated facts, as that before us.

Independently, also, of the unphilosophical manner in which the inferences are drawn, we must express our distrust of the source from which a great many of the data are derived; namely, destructive experiments on living animals. We feel convinced that an animal mortally injured, in which every action of life is fast ebbing and the organ immediately experimented upon placed *suddenly* in circumstances incompatible with the continuance of its functions, can, generally speaking, afford no just solution of the phenomena of life, whether vital or physical. If this objection be deemed too vague and general, as we are aware it may, at first sight, we appeal at once to experience, as to the results of such methods of enquiry; and we ask how it is, if they be worthy of the confidence which some have reposed in them, that the ablest physiologists are continually at variance as to the *facts* elicited from them, one thing being observed by one experimenter, and something quite different, or diametrically opposite, by another? We do not at all deny that there are instances in which such experiments are of the utmost utility, and afford the only means of determining a question; but, as a general *basis* of physiological research, we believe them to be entirely delusive. Again, no respect is paid by M. Magendie to the physiological differences of animals; and an inference from the changes which ensue in the body of one animal is made applicable to the whole class, as well as to man.

The perusal of these volumes, indeed, confirms an opinion which we had formed from a knowledge of the previous labours of this physiologist; that, whatever tact he may possess in performing experiments, he is not well qualified to draw conclusions from the results. Thus we find him repeatedly deducing an inference from certain appearances in one lecture, expressing a dogmatical opinion off-hand, and coolly retracting that opinion in a subsequent lecture. There may be candour, in thus publicly acknowledging an error, but we must contend that such hasty conclusions betray a great want of judgment, and serve to create distrust in the opinions of the experimentalist. Many years ago, M. Magendie startled all sober physiologists by the assertion that the grand sympathetic was not to be considered as a part of the nervous system of animals; and that he had removed the cervical and superior thoracic ganglia, without causing the least disturbance in the animal functions. (*Précis Elément. de Phys.* tom. i. p. 172, 1825.) In the work before us we see the same spirit displayed in numerous instances. The same want of caution, the same hasty generalization, the same indifference to strictly inductive reasoning, exist—the subject only is changed, not the mode of investigation. The result is what might be anticipated.

We have premised these general remarks, in order to avoid needless repetition, and now proceed to consider the contents of these lectures in detail. We shall, as much as possible, take up the different subjects in the succession in which we find them; but as there is throughout a total want of logical arrangement, with its necessary consequence of frequent iteration, we shall not hesitate to make such transpositions as perspicuity or condensation may demand.

Porosity and Imbibition. Every substance in nature is more or less porous, and the organic textures are endowed with porosity in various degrees. All porous bodies present the phenomenon of capillary attraction, and to this, as manifested in the organic textures, M. Magendie gives the name of *imbibition.* Our author makes imbibition the key to the whole doctrine of absorption. The following is a summary of the conclusions, with reference to this function, which he assumes as established by his former researches, or deduces from experiments presently to be detailed.

In every instance of absorption there are two distinct phenomena, which we should be careful not to confound. 1st. The introduction of a liquid by imbibition. 2d. Its conveyance into the current of the circulation. The lymphatic vessels have nothing to do with absorption, which is exercised solely by the veins, except in the instance of the absorption of chyle, which is effected by the lacteals. M. Magendie contends against the opinion that the absorbing surfaces of living bodies select the substances fit to be absorbed, and reject the rest: he maintains that they take up substances on the simple principle of capillary attraction, and hence that the facility with which substances are imbibed, depends entirely on their physical properties—soluble substances being more readily taken up than insoluble ones, and volatile substances more readily than those which are fixed: from all which it would appear, that the vital function of absorption, as hitherto regarded by physiologists, has no existence; the phenomena referred to it being purely physical. Imbibition exists in various degrees, in the different organic textures. It is greatest in the serous and cellular membranes, less in the blood-vessels, less still in the mucous membranes, and least in the dermoid textures. The epidermis, under ordinary circumstances, may be said not to imbibe at all; nevertheless, if a substance apt for imbibition be applied to it for a length of time, it is absorbed. Though the imbibing power of any two surfaces be equal, the transit of substances through them into the system will be more or less speedy, according to their degree of vascularity; thus the pleura and peritoneum are both serous membranes, and as such may be supposed to imbibe in an equal degree; but the pleura is the more vascular, and hence a poison injected into the cavity of the thorax will act on the system more rapidly than if it were injected into the cavity of the abdomen. Active substances do not affect the system through the medium of the nerves, but by entering the circulation; the rapidity, therefore, with which such substances produce their effects, is exactly proportioned to the facility with which they are imbibed, and the vascularity of the imbibing organ. The following are apt illustrations of imbibition.

If we put a drop of an aqueous solution of iodine on a sheet of paper, the colouring matter remains in the centre, and the more liquid part is diffused towards the circumference. Suppose, now, that a man has received a severe blow on his arm; blood is effused, and an ecchymosis formed, occupying a limited space. What do we observe the day after the accident? The contused point is black, while the neighbouring integuments are of a yellow colour, which often extends as far as the shoulder and fore-arm. In fact, the black part of the blood remains where it was first effused, and the serum, charged with yellow colouring matter, is infiltrated into the textures. What is there of vital in all this? We cannot suppose that the absorbents have conveyed the colouring matter; for, since the imbibition takes place below as well as above the injured part, their action would then be retrograde. (Vol. i. p. 20.)

Again, an aqueous solution of the ioduretted iodide of potassium is injected into the abdominal cavity of a rabbit. On laying open the abdomen, the serous coat of the intestine is found to be penetrated and coloured by the injection, the liquid having been imbibed through the parietes of the vessels. We must not wait too long before opening the abdomen, or the whole of the liquid will have disappeared, and been taken into the circulation. If a few drops of the liquid be placed upon the stomach, a similar phenomenon is observed. (Vol. i. p. 21.)

Before going further, we must enter our protest against the arbitrary manner in which M. Magendie assumes it as proved that the lymphatics have no part in the function of absorption. This is one of many instances in which, having convinced himself, he thinks that every one else ought to be convinced also. It would be out of place here to enter into a discussion of so disputed a point, but we may be allowed to ask, if M. Magendie's experiments be so decisive, how is it that his conclusions are not generally received? He must be aware that the greater number of contemporary physiologists, while they fully admit that he has established the fact of absorption by the veins, still believe the lymphatics to bear a part, and a principal part, in the function in question. While it is the attribute of genius to conceive new ideas, and to devise new modes of research, the validity of the deductions derived must ever be determined by the impartial judgment of others.

We do not positively assert that M. Magendie's doctrine is erroneous, though we believe it to be so; but we affirm that, in the present state of our knowledge, M. Magendie has no right to assume, as a basis of physiological reasoning, that the veins are the sole absorbents. Neither can we admit that the function of absorption is, in all instances, dependent on the common laws of physics, although M. Magendie has shown that it is so in some cases, where it might formerly have been regarded as a vital process.

We may adduce one or two examples of the inapplicability of M.

Magendie's hypothesis. It is well known that the cavity of a circumscribed acute abscess is lined with a dense membranous substance, much resembling a layer of organized lymph; and it is further well known that abscesses have a remarkable and salutary tendency to open on the surface of the body, without which they would always endanger life, when situated near any of the great cavities: now, can M. Magendie tell us what change is effected in the physical properties of the more superficial portion of the sac that should render it more easy of absorption than any other portion? In the course of these lectures, M. Magendie remarks on the bursting of an abscess, that it is partly by the successive imbibition of the pus into the cellular tissue, that the skin is penetrated throughout its layers, grows thin, and finally gives way, (vol. i. p. 106;) but he does not inform us why the pus at first made its way through the firm parietes of the abscess at one point rather than another. We suspect this looks very like an increased action in the absorbent vessels themselves, set up by the instinctive power of the living organism. Again, when sphacelus has taken place in a part, and a line of separation is drawn between it and the living parts, and the intervening substance is absorbed so as completely to detach the slough, can M. Magendie show us any physical change in the particles taken up which renders them more apt for imbibition than they were before? or can he tell us why this separation of the mortified part happens in one case and not in another, the physical condition of the dead and living parts being, in the two cases, to all appearance exactly analogous? While we are unsatisfied on these heads we must remain under the conviction that the absorbents do actually exercise a vital selection, and that the reason why a mortified part is separated in one case and not in another is that the vital powers are sometimes equal to the effort, and sometimes not.

A third point on which we must differ entirely from M. Magendie is that of cutaneous absorption. He denies that any absorption takes place through the cuticle under ordinary circumstances. Now, considering the direct evidence in favour of cutaneous absorption to be found in the experiments of various physiologists, and especially in those of Dr. Milne Edwards, which are generally thought to be conclusive,* we expected some new and very striking facts from M. Magendie on the opposite side of the question. Nothing, however, is adduced, but that he has often received on his own skin, the saliva of persons labouring under hydrophobia, without any bad result! We can attach no importance whatever to this fact; for although, according to M. Magendie's experiments, the disease may be communicated to the dog by inoculation with the saliva of a hydrophobic person,† it is entirely unknown whether it be communicable from one human being to another, under any circumstances, the susceptibility of the human subject being comparatively very small. It is equally unknown whether the virus acts through the medium of absorption, or by direct application to the extremities of the nerves; and even adopting the former supposition, a viscid liquid, like the saliva of a hydrophobic patient, applied in small quantity and drying immediately on the surface, is not likely to be readily absorbed through the cuticle, the imbibing power of which is acknowledged to be only of moderate intensity.

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* We do not mention the experiments of M. Collard de Martigny, because they are of a date posterior to that of the lectures under review. For an account of these and others, see the review of Dr. Madden, Br. and For. Med. Rev. vol. VI. p. 332.

† It were to be wished that these experiments had been repeated by some other observers.

We may now consider some of the principal applications which M. Magendie makes of his peculiar doctrine of absorption.

The Mode of Operation of Poisons. Some drops of an alcoholic solution of nux vomica are injected into the pleural cavity of a rabbit; the animal is instantly seized with tetanus, and dies. In another experiment, a small arrow, envenomed with the alcoholic extract of nux vomica, is driven into the thigh of a rabbit: five minutes elapse before any sensible effect is produced. The diversity of result in these two experiments arises, according to M. Magendie, from two causes, viz., the poison being solid in the one instance and dissolved in the other, and the greater vascularity of the pleura than of the muscular substance of the thigh. If, at the moment when the rabbit into whose thigh the poison has been inserted begins to experience its effects, the member be strongly compressed with the hand, the influence of the poison is suspended; but the moment the compression is removed, the animal falls dead. From these and similar experiments, M. Magendie deduces his opinion that the rapidity with which poisons produce their effects is in the exact ratio of the facility with which they are absorbed, and that the circulation is the only medium through which they operate. We cannot help thinking that if he had made a careful comparison of the physical properties of the different active poisons, he would have hesitated to adopt such a belief.

Hydrocyanic acid, the most rapidly fatal of all poisons, doubtless affords an instance favorable to M. Magendie's hypothesis, for it is a highly attenuated and volatile substance. But let us try strychnia. In an experiment of Orfila's, half a grain of this substance introduced into a small incision made in the back of a rabbit caused convulsions in one minute and death in three and a half.* It is to be observed that the question before us is not as to the energy of the poison in destroying life, but simply as to the celerity of its effect. Now strychnia is so insoluble in water, that it requires about 6667 parts of cold and 2500 of boiling water to dissolve it; and it is volatile only at temperatures so high as to decompose it: its properties are therefore entirely unfavorable to speedy imbibition; and we doubt much whether it be possible that, in the experiment alluded to, a quantity of the poison sufficient to produce its specific effects should have been conveyed into the circulation in a minute. According to the experiments of Müller, an appreciable quantity of a substance *in solution* brought into contact with a membrane not covered by epidermis, may be distributed through the system by means of the blood-vessels in from half a minute to two minutes;† but in this experiment with strychnia, the substance is *solid* and eminently insoluble.

Again, take the poison of the rattle-snake. Captain Hall saw a dog killed by the bite of this animal in fifteen seconds, and another in thirty seconds.‡ The effects of the bite of this serpent are not generally so rapid, but the observations seem to have been very carefully made, and the more than usual energy of the poison does not render the instance less available for our purpose, which is to show how quickly a poison may act on the system. Now, if M. Magendie's opinion were correct, this poison should be a very penetrating and volatile substance. But it appears from the observations of Dr. Russell that the poisons of venomous serpents, which are all remarkably similar in their physical properties, consist of a mucilaginous or glutinous liquid, of a yellowish colour, devoid of pungency, and insipid, readily soluble in water and alcohol, when fresh, but soon drying into a yellow, flaky, resinous-looking substance, which grows darker coloured when long kept, and is then not easily soluble.* An exact analysis of these poisons is still a desideratum, and, for anything we know, they may contain some highly subtile and volatile principle, but from all that is ascertained—which is all that anybody has a right to reason from—there is nothing in the physical properties of such poisons to account for any remarkable rapidity of effect on the score of ready imbibition. These few examples may suffice to show that M. Magendie's conclusion is at least premature, and that no attempt should be made to base any theory of the operations of poisons on their physical properties till these properties have been subjected to a careful and extensive examination. The confidence with which M. Magendie asserts the operation of poisons through the exclusive medium of the circulation perfectly astounds us, when we consider the extreme difficulty and complexity of the investigation, the great discrepancy of opinion among the best physiologists of the day, and the countenance afforded to an opinion very different to M. Magendie's by the careful and well-devised experiments of Morgan and Addison. The inference from the effects of compression on a limb is of little weight, because supposing the poison to operate on the nerves, these, like other organs, require a due supply of blood for the maintenance of their functions; and hence it is not surprising that no impression should be made on them when they are not in a condition to be impressed. Moreover, a similar arrest of action may be caused by compression where there is no poison in question: has not an *aura epileptica* often been arrested, and a fit averted, by a tight ligature round a limb? Yet no one supposes that in this case any poison enters the veins: the presumption is that a morbid action which has commenced in the extremity of a nerve, and would have been propagated along its course, is stopped by cutting off the supply of blood.

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* Traité des Poisons, tom. ii. p. 372. Paris, 1826.

† Elements of Physiology (Baly's translation), pp. 245-6.

‡ An account of some experiments on the effects of the poison of the rattle-snake. By Captain Hall. Communicated by Sir Hans Sloane. Philosophical Trans. for 1727.

On the whole, M. Magendie's experiments on the absorption of poisons do not seem at all to warrant the conclusions he derives from them. They merely tend to prove that, *cæteris paribus*, the more easily poisons are imbibed, the more rapid will be their effect.

Contagious Diseases. The real number of these M. Magendie regards as very small; and he flings equal contempt on governments and doctors who would extend the list. When speaking of the discrimination of truly contagious diseases from those which are only reputed to be so, he asks, "Is it not an afflicting thing to see that these questions, essentially belonging to the province of medicine, have been resolved by men strangers to this science; and that our medical legislation still rests on the most erroneous assertions? Thus the law recognizes five contagious diseases, and punishes with death every individual who infringes the rules laid down to prevent their introduction. Well; of these five diseases, four at least ought to be erased from the list." (Vol. i. p. 64.) The four diseases alluded to are typhus, cholera, yellow fever, and leprosy, "the contagion of which," says M. Magendie, "is positively denied by every enlightened physician." Now, we would ask, in our turn, is it not afflicting to see such a man as M. Magendie, who is no stranger to medicine, and who takes his ground as a philosopher, hazarding an assertion at once so rash and so illiberal, as that no enlightened physician believes typhus or cholera to be contagious? We humbly hope that the profession is not altogether benighted in this country, yet nineteen English physicians out of twenty, and probably a much larger proportion, firmly believe typhus to be contagious; and a considerable number are of the same opinion with respect to cholera. As to typhus, we must confess that we are ourselves shrouded in thick darkness; for we feel just as certain that typhus is contagious as that the gout is not. On the contagious nature of cholera we are in a state of twilight: being averse to hasty conclusions, we have not yet made up our mind; but it suffices for our argument that the disease is considered contagious by many men eminent both as physicians and philosophers.

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* Experiments on the Poisons of several Indian Serpents, &c., p. 86.

There is in general little to interest in our author's remarks on the diseases reputed to be contagious. The following fact, however, is worthy of notice: M. Magendie has found, by experiment, that if a few drops of water, impregnated with putrid meat or fish, be introduced into the sanguiferous system of a dog, the animal first presents an unusual degree of activity, then is affected with fever, lies down, refuses food, and vomits an immense quantity of the black matter characteristic of yellow fever. (*Ib.* p. 67.) In another place he states that in similar experiments petechiæ on the skin were also produced. (*Ib.* p. 117.) This observation, interesting in itself, is followed by one of those sweeping affirmations too common with our author, "We know also," says he, "that in all circumstances under which yellow fever is developed, the air has been vitiated and corrupted by the disengagement of putrid animal products. Thus it is not rare to see it break out when a vessel laden with cod runs aground in the neighbourhood of a town, and the merchandize (fish) being heaped up, exhales into the atmosphere infectious miasmata. Let these corrupted matters be thrown into the sea, and the disease will soon disappear. Thus the cause which produces and keeps up yellow fever being perfectly known, we have only to guard against it in order to have nothing to fear from this terrible scourge!" (*Ib.* pp. 67-8.) This certainty in the etiology of yellow fever is quite new to us. Since writers of the highest authority, and who have had the most ample opportunities of observation, have differed entirely on the subject, some referring the cause of this disease to imported contagion, some to marsh miasm, some to animal putrefaction, while others, who have had the advantage of preceding opinions in addition to their own experience, declare frankly that they do not know what the cause is, we had surmised that there must be great difficulty in settling the question; but M. Magendie soon gets rid of a difficulty!

Our author speaks at some length on the subject of plague. He doubts whether it be contagious, or communicable through the medium of fomites; but supposing it to be so, he is convinced that the notion of its being communicated by contact with the diseased body must be erroneous, and that the real mode of communication is by pulmonary absorption, which, he contends, is the medium through which all contagious diseases are propagated. He says that the itch forms no exception to this rule, since it is not propagated by a virus but by an insect. We will not stop to contest this point, though there is good ground; but may suggest, in passing, the instance of *porrigo scutulata*. Is this not contagious? There are few diseases more so. Is it also propagated by an insect? The assertion, we believe, has never been made. With respect to plague, M. Magendie asks, "by what singular exception to all the known laws of imbibition is it that the skin, protected by the epidermis, possesses such strong absorbent powers, while the pulmonary surface is completely destitute of them? This is opposed to every precept of experience and reason." (Vol. i. p. 72.) But he seems here to be contending against a doctrine which no one maintains. As far as we know, it is generally admitted by those pathologists in the present day who regard the plague as contagious, that it is very readily communicated by inhalation of the breath of the sick; and although it is maintained by many authors that contact, or a degree of proximity approaching to it is necessary to infection, this opinion, we apprehend, has reference not so much to the particular surface by which the poison is admitted into the system as to its limited diffusion, which renders a near approach to the body of the patient essential to its activity. Nor do we see any reason to deny that the poison may be admitted through the skin; for, notwithstanding M. Magendie's assertion to the contrary, there seems little reason to doubt that various attenuated substances are very easily absorbed through the skin; and if so, why may not the miasm of the plague? Lastly, we would suggest that (admitting the contagion), since the agent by which it is communicated entirely eludes our senses—since we know nothing whatever of its nature, and can only conjecture even that it is some form of ponderable matter—we have no ground of certainty that it acts by absorption at all, however probable the supposition may be that it does so. M. Magendie attacks the present system of quarantine with great vehemence, and surely not without reason; but this subject being foreign to the one immediately before us, we shall not enter into it.

Exhalation. We have hitherto been treating of imbibition only as it takes place from the exterior to the interior of vessels; but the same phenomenon occurs in an inverted manner, the liquids passing from the inner to the outer surface of the vessels. It is then termed *exhalation*, of which we have a remarkable and continual example in the transpiration from the lungs. There are some substances which, when introduced into the blood, cannot remain there long, and are speedily expelled, as ether, camphor, and phosphorus, which have scarcely passed into the circulation, ere their presence is detected in the vapour exhaled from the lungs.

Endosmosis and Exosmosis. Imbibition may take place simultaneously from within outwards, and from without inwards, thus causing a double current. M. Dutrochet, having placed a bladder filled with a liquid in another liquid of a different nature, found that, according to their different composition, the liquid contained in the bladder passed out through its parietes, or the external liquid passed in. These phenomena he named *exosmosis* and *endosmosis*. According to the physical properties of the liquids, a greater quantity of the one or the other will be transposed; generally speaking, the more viscid liquid will attract the other, but in every case there is in reality both endosmosis and exosmosis, or, in other words, a certain quantity of one fluid passes in, and a certain quantity of the other passes out. M. Magendie, therefore, proposes to call this process *imbibition by a double current* (imbibition à double courant).

The phenomena of endosmosis and exosmosis must be influenced in a variety of ways by the physical and chemical constitution of the fluids, as well as of the solids which they permeate. M. Magendie does not enter into the consideration of all the causes which determine the current in one direction rather than the other, and concludes, judiciously, we think, that in the present stage of our knowledge, they cannot be accurately explained—an opinion which coincides with that of M. Dutrochet himself, as most recently expressed.* The following is a neat experiment of M. Magendie illustrating imbibition by a double current. Take an egg, and remove a portion of the shell carefully, so as to denude the outer membrane; then place the egg in a vessel containing a little alcohol, having previously pierced a hole in the end which is not immersed. Here the results of double imbibition present themselves: the alcohol passes in by *endosmosis*, through the membrane, and coagulates the albumen, which passes out at the hole at the end of the egg, while by *exosmosis* the liquid albumen transudes through the membrane, and mixes with the alcohol in the vessel, rendering it turbid, and forming albuminous flakes. (Vol. i. p. 99.)

M. Magendie now proceeds to apply the doctrine of imbibition to the elucidation of disease. Having recapitulated the instance of a bruise, formerly cited, and instanced a leech-bite, the phenomena consequent on which admit of a similar explanation, he alludes to the subject of *encysted dropsy*. Tumours of this kind consist in a collection of liquid enveloped in a membranous bladder. It is important to attend to the nature of the contained liquid, for it is often so viscid that it will not flow through the canula of a trocar. The spontaneous cure of encysted dropsies is extremely rare; they slowly increase in size, and finally prove fatal through the impediment which they offer to the principal functions of life. We find here united the principal conditions of endosmosis: we have a bladder filled with a liquid immersed in other liquids of a different nature. It is to be observed, also, that these tumours are more difficult of cure in proportion as the liquid they contain is more viscid. Going on this principle, M. Magendie has attempted the radical cure of such diseases by modifying the nature of the fluid secreted by their internal surface. Several years ago he had under his care at the Hôtel Dieu a woman with a tumour of this kind in the ovary. By its bulk it impeded respiration and digestion. The patient was daily wasting away, and submitted to an operation as affording the only chance of recovery. M. Magendie made a puncture, with a view of exploration, and a viscid liquor escaped in a thread-like stream through the canula of the trocar. The tumour being emptied, some warm wine, diluted with half its quantity of water, was injected into the cavity, and, having been allowed to remain a few seconds, the greater part was drawn off. In consequence, no doubt, of the excitement produced by the injection, the tumour filled again with extreme rapidity, so that on the following day it had resumed its former volume. Another puncture was made, and the liquid, being much less viscid than formerly, flowed much more freely. The tumour again filled with serum, but by degrees diminished, and finally disappeared. M. Magendie has since repeated the same practice in two similar cases: in one he obtained the like success; in the other the tumour reappeared in spite of numerous punctures, and he was obliged to leave it to itself. In these cases M. Magendie's object was so to modify the action of the exhaling surface, as to render the liquid less viscid, and therefore more easy of imbibition through the parietes of the sac. He does not affirm positively that this was the means through which the cures were effected: his view, however, is ingenious, and accords well with his doctrine of imbibition. (Vol. i. pp. 103-5.)

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* Cyclopædia of Anatomy and Physiology, Art. *Endosmosis*.

A good instance of imbibition through the walls of blood-vessels is presented by an aneurismal sac, in which the concentric layers of coagulated blood are softer near the centre, and of firmer consistence towards the circumference, because the aqueous portion of the blood has been imbibed through the parietes of the vessel. (*Ib.* p. 107.)

We do not quite agree with our author when he declares that the treatment of dropsy is altogether empirical, and that hence each physician has his formula; one bleeds, another purges, and a third gives diuretics; while those who wish to conciliate different opinions, employ these several means in conjunction. (*Ib.* p. 107.) We freely admit that the treatment of dropsy is for the most part very ineffectual, but this, we think, is not so much for want of rational indications, or the means of fulfilling them, as from the too frequent presence of a perpetual cause of the disease in some organic affection of the viscera. Let us see what light M. Magendie throws on the subject.

> "Dropsies (says he) are in a great measure dependent on physical laws, and may be produced at will in a living animal by determining the conditions which give a preponderance to exhalation over absorption. Every obstacle to the venous circulation is attended with serous infiltration of the parts whose veins, destined to carry the blood towards the heart, are obliterated. But there are dropsies which cannot be attributed to these mechanical causes; thus ascites sometimes exists without any appreciable alteration in the system of the vena portæ. A careful enquiry into the composition of the blood in these cases of serous infiltration, independent of any mechanical obstacle to the circulation, would afford a study at once attractive and new. It is already known that the urine is charged with albumen: this observation is interesting, but we ought to go further. Do you think that it is indifferent, with reference to the equilibrium between absorption and exhalation, whether the blood which runs through our vessels be more or less viscid, or contain more or less of the watery element?" (Vol i. p. 108.)

All this is very true; but we can see nothing peculiar in M. Magendie's pathology of dropsy, except the omission of some causes of it which are generally recognized. Pathologists, in this country at least, fully admit the mechanical obstruction of the venous circulation as a cause of diminished absorption, and a watery crasis of the blood, accompanying a leucophlegmatic habit, as a cause of increased exhalation, and both these, consequently, as causes of dropsy; but they add inflammation of serous membranes as a certain and well-known cause, while some consider as probable, though not ascertained causes, a simple increased action of the exhalents, or a diminished action of the absorbents.

M. Magendie remarks that imbibition has much to do in the formation of the various morbid products which come under the cognizance of the pathological anatomist, in their increase, and in the changes which they undergo. The instances adduced, however, are not very striking. Among others he cites that of the tumours of bones called *lardaceous*, in which, he says, the circulation no longer exists, but which nevertheless increase in size and alter in their texture, the hardest points at length becoming soft and fluctuating. The materials of the blood are effused in the parenchyma of the bone, but how could they penetrate thither otherwise than by imbibition? He thinks that these tumours live, like vegetables, by imbibition, and gives a case of a tumour occurring on the side of a girl's head, which M. Dupuytren attempted to remove, but found it impossible to do so on account of the connexion of the tumour with important parts. M. Magendie sawed off a portion of it weighing several pounds, and only a few drops of blood exuded from this large wound. He found that the tumour consisted of scirrhous and cerebriform matter deposited in the areolæ of the osseous texture, but there was no trace of sanguiferous vessels. The wound healed very speedily. The tumour grew again, and a portion was removed, the wound healing with uncommon readiness. Notwithstanding these partial reductions, the symptoms grew worse every day, and death impended. M. Magendie, therefore, ventured to tie the carotid artery, the result of which was that the tumour ceased to grow, and the patient's life was preserved. (Vol. i. pp. 119-20.) This was a bold piece of surgery, and, as it happened, successful as far as the tumour was concerned; we find afterwards, however, that the operation was followed by hemiplegia and permanent lesion of the mental faculties. (Vol. iii. p. 153.) We cannot perceive that this case affords any corroboration of our author's particular views. Many parts have blood-vessels which are too minute to be easily discerned, and we conceive that the tumour in question ceased to grow simply because its supply of blood was cut off. If it were otherwise, how should the ligature of the artery have had so great an effect? The tumour still retained its attachment to parts from which it might have imbibed nutrition.

As an illustration of the effects of dilution of the blood, M. Magendie gives the dissection of a dog, into whose veins he had injected, three days before, three pounds and a half of water, and which died two hours after the operation. According to the laws of imbibition, the liquid exhales by the readiest outlets. The lungs are most favorable to its elimination; hence a thick vapour proceeded from the throat of the animal, and as there was not time for all the liquid to be converted into vapour, a part of it was found under the form of a light froth. In this dog M. Magendie met with a phenomenon which he had not before observed: half an hour before death, the whole surface of the body was covered abundantly with a liquid, proceeding, as was supposed, from cutaneous evaporation. On dissection, the pelvis of the kidney is found to contain a small quantity of a serous fluid of a slightly red colour, the quantity of which would probably have been greater, but that imbibition into the neighbouring textures has taken place since the death of the animal. The intestines and other viscera are pale, and seem as if they had been macerated in water for a long time. The pleura is very moist, and appears charged with serosity, but there is no effusion of liquid within its cavity. The diaphragm has lost its natural rosy colour, and is studded with blue and livid spots, arising from the effusion of blood in the interstices of its fibres. The pulmonary tissue is gorged with liquids, and offers that first degree of alteration called *engouement*. The internal surface of the stomach is covered with the same spots as the diaphragm. (Vol. i. pp. 113-16.)

Commenting on the dissection of this dog, M. Magendie observes that the extravasation of blood into the cellular tissue is a frequent phenomenon in the human subject, in consequence of changes in the chemical composition of the fluids. One of the physical conditions of a normal state of the blood is its incapacity of transuding through the parietes of its vessels without the separation of its constituent parts; but if any modification of these constituents occur, as, for example, a diminution of viscosity by the injection of water into the veins, ecchymoses will appear in various parts. M. Magendie cites the instance of sea-scurvy, in which the blood, impoverished by the use of salt and scanty provisions, and the privation of fresh vegetables, is effused in large spots on the surface of the skin. In reference to the state of the lungs, he expresses his conviction that the modifications which the parenchyma of the lungs undergoes in pneumonia should not be referred exclusively to vital properties; there are also physical phenomena well worthy of attention.

M. Magendie remarks that electricity is not without influence on the phenomena of capillary attraction. Thus, if a capillary tube be immersed in a liquid, the liquid may be made to ascend rapidly into the tube by transmitting an electric current through it. This fact was first ascertained by M. Porret, and it was from the analogy of the results of his experiments and those of M. Dutrochet, that the latter gentleman was at first led to refer the phenomena of endosmosis and exosmosis to the influence of electrical currents, and to suppose that he had discovered the immediate cause of the vital movements.

M. Magendie relates an experiment of Fodera, showing the influence of electricity on imbibition. On placing some solution of prussiate of potash on the intestinal mucous membrane of a dog, and a solution of sulphate of iron on the corresponding serous surface, imbibition was at first very slow; but when a galvanic current was passed through the parietes of the intestine, the imbibition was much more rapid, and the surfaces were soon tinged with blue. (*Ib.* p. 118.) It results from these facts, says M. Magendie, that electricity may be usefully applied to therapeutics. He has himself applied it with success in the resolution of some tumours and obstructions. He observes that if the poles of the galvanic pile be applied merely to the surface of the skin, no remarkable result will probably be obtained, because electricity has a greater tendency to expend itself on the surface of bodies than to penetrate into their interior; he is, therefore, in the habit of driving two small metallic needles into the part he wishes to affect, and these being brought into contact with the pile serve as conductors of the galvanic current.* It is also known, says our author, that when a moist surface is subjected to the influence of electricity, evaporation goes on much more rapidly. (pp. 118-19.)

Permeability of the Textures to Gases. It is a point of great importance in physiology, observes M. Magendie, that all our textures are permeable to gases. A remarkable instance occurs in the lining membrane of the lungs; and here, also, we have imbibition by a double current, for while oxygen passes in to vivify the blood, carbonic acid passes out and mixes with the air about to be expired. The permeation of the textures by gases does not however take place on the same principle as the imbibition of liquids. The particles of liquids are united by cohesion, while those of gases, in virtue of their characteristic elasticity, tend to diffuse themselves through space. M. Magendie, therefore, believes that the penetration of gases into the pores of solids depends on their elasticity. He makes some observations on the effects of deleterious gases, and gives a remarkable instance of the apparent generation of fever by exposure to carburetted hydrogen. Some years ago a chamber, in which several persons were asleep, was suddenly filled with carburetted hydrogen proceeding from a gas-pipe. They were all immediately seized with typhus in a very severe form, which M. Magendie has no hesitation in attributing to the effect of the gas upon the blood with which it became mixed in the act of respiration. He ascribes to the agency of the same gas the morbific power of marsh miasmata. (p. 130.) But how does this accord with the known effects of carburetted hydrogen? When an atmosphere excessively charged with it is breathed for any length of time, it acts purely as a narcotic, inducing fatal coma; but it is well known that air, prettly largely impregnated with it, may be breathed with perfect impunity, as is daily done by miners and gas-men.† This being the case, we cannot help thinking that there must have been some other cause for the typhus mentioned by M. Magendie. Again, we cannot attribute the morbific effects of marsh miasmata to an agent which, though it doubtless exists in the atmosphere of swampy places, must be in a state of dilution, in which daily experience shows that it is innocuous, or produces at least no sensible bad effects. Neither do we see any reason to believe that an agent which produced typhus at one time would produce an ague or a malignant remittent at another: we can trace an affinity between the two latter, and can conceive them to originate from a common cause, varying in degree and modified by circumstances; but we can see no resemblance between either of them and typhus, nor any ground for ascribing them to a common origin with the latter disease.

M. Magendie lays great stress on the study of the effects of gases, and seems to anticipate great success from the use of remedies of the same class, in cases where their deleterious action has been experienced. He gives the following illustrative experiment. An unstopped bottle of anhydrous prussic acid is passed rapidly under the nostrils of a small guinea-pig, and the animal falls motionless. The vapour of strong ammonia is then applied to the nose: the animal is agitated and utters cries; and M. Magendie has no doubt will survive the experiment. (Vol. i. p. 132.) He thinks that in cases of poisoning by prussic acid, immediate recourse should be had to the vapour of ammonia or chlorine; in like manner, if sulphuretted hydrogen were the deleterious agent, the first indication would be the introduction of chlorine gas into the lungs. M. Magendie is of opinion that the causes of many diseases reputed to be contagious might be found in changes in the atmosphere, produced by putrid effluvia—a point which we shall not discuss, as we have a dislike to reasoning without data.

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* This process was suggested by M. Berlioz in 1816, as a more efficacious modification of acupuncture, and put in practice by M. Sarlandiere, who published a work on *electro-puncture*, as he names it, in 1825. It does not appear generally to have met with much success.

† Christison on Poisons. 3d Ed. p. 743.

Viscosity of the Blood. There is an important circumstance connected with the porosity of the animal textures, namely, that the orifices which give passage to the materials of nutrition are so small, that these materials can only enter the parenchyma of organs in a state of the most minute subdivision. Thus, while water, introduced into the stomach, is immediately imbibed, albumen, mucilage, or oil require to be reduced to very fine particles, by the process of digestion, before they can enter the lacteal vessels. It follows, as a necessary consequence, that if liquids be introduced into the blood, which are too viscid, or the particles of which are too large in proportion to the minute canals which they ought to pass through, the blood will stagnate in the vessels, and death will inevitably ensue, even though the substance introduced be perfectly innocent in its nature. (*Ib.* pp. 136-7.) These considerations, observes M. Magendie, suggest important cautions in the use of venous injections in the treatment of diseases. If, for example, a practitioner were to inject a solution of gum into the veins, he would immediately destroy his patient. In the direct introduction of foreign substances into the blood, it is of consequence also to be well acquainted with their chemical action on that fluid. Thus if corrosive sublimate were injected into the veins, or even an acid devoid of poisonous properties, the albumen of the blood would be coagulated, and the pulmonary vessels immediately obstructed. If mercury be injected into the veins, death follows by arrest of the circulation, and on dissection, a globule of mercury will be found obstructing each capillary vessel of the lungs. Why then can we administer it safely by friction or through the stomach? Because it is reduced to particles sufficiently minute to traverse the pores of the epidermis, or of the intestinal mucous membrane. A very curious phenomenon is observed when mercury is injected into the veins of a living animal: the lungs appear stuffed with an immense number of tubercles, and when these are examined, each puriform globule is found to contain at its centre a globule of mercury. It is, then, by the deposition of coagulated blood around these small foreign bodies that the numerous concretions are formed. (pp. 137-8.) M. Magendie asks if it be not possible that tubercles are formed in the lungs by a similar mechanism? The question is not new. Mercury has been injected into the lungs by M. Cruveilhier, Dr. Kay, and others with a view to this very point; but we cannot perceive that these experiments throw any light on the origin of tubercles.

Our author remarks that, when an animal is kept on too azotized a diet, symptoms occur which partly admit of explanation on the supposition of too viscid a state of the blood; and he is inclined to believe that in the diseases styled *carbuncular*, the abscesses formed depend partly on obstruction of the vessels from this cause. (p. 139.) He does not state his reasons for this belief, nor can we form the remotest idea of what they may be.

Some experiments follow, illustrative of the foregoing principles.

About a demi-gros (a drachm) of olive oil is injected into the jugular vein of a dog. The respiration becomes embarrassed, the thorax dilates with difficulty, and the inspiratory movements are multiplied to facilitate the passage of the blood through the capillaries of the lungs; life, however, does not appear to be endangered. Another quantity of oil, about equal to the former, is injected: the animal is agitated, and struggles, turns on his side, and dies suffocated. On dissection the lungs present every appearance of recent pneumonia, the crepitation is lost, and the density increased. When cut into they discharge a frothy and reddish serum. The blood is evidently more viscid than natural. The air-cells are gorged with a thick liquid, in the midst of which the oil may still be detected. The mucous membranes are pale. The crural artery contains only dark-coloured blood, which appears to be imbibed by the parietes of the vessel. This change in the colour of the blood is easily accounted for by the obstruction of the pulmonary circulation; but it is sometimes found where no such obstruction exists. M. Magendie has repeatedly observed that, in cases of apoplexy, the arterial blood, instead of being frothy and bright-red, is dull and blackish. He does not pretend to explain the cause of this; but he regards such a change in the colour of the blood as a most alarming symptom, and all the patients in whom he has observed it have died. (pp. 139-41.)

It is not only by the introduction of foreign substances that the blood becomes too viscid to circulate in the capillary vessels. Other causes lead to the same result. Thus, if, by too abundant a transpiration, a large quantity of serum be abstracted, the effect will be the same. The blood of an animal, the globules of which are large, will not suit the vessels of another animal with smaller globules. If, for example, the blood of a reptile were injected into the veins of a man, the want of relation between the fluids and the capillaries would inevitably cause death. (pp. 141-2.)

M. Magendie proceeds to make some remarks on the treatment of diseases, based on the principle that an impoverished state of the blood has a tendency to give rise to inflammation, and particularly to pneumonia. He states that he has ascertained this to be true, with respect to animals which have been debilitated by repeated abstractions of blood at short intervals. A case of tertian ague is given, in which a fatal degree of debility was induced by the injudicious and repeated employment of the lancet: on dissection, all the viscera were found healthy except the lungs, which were in a state of extreme *engouement*, strikingly analogous to that caused in animals by impoverishing their blood. M. Magendie believes that the secondary inflammations of the lungs and pleura, which supervene in the course of acute diseases, are often the result of excessive bloodletting. To this cause he also attributes those affections of the thoracic viscera in acute rheumatism which are usually considered as metastatic. He never employs bloodletting, either general or local, in this disease, and declares that in his own practice he never meets with these secondary affections. (pp. 144-8.) We cannot say that we think our author's view of the subject is borne out by the state of the blood generally observed in acute rheumatism, for of all diseases this is the one in which the blood has the greatest tendency to dense coagulation, often presenting a buff coat and a firm coagulum, even after large and repeated bleedings. We do not, however, mean to express ourselves advocates for large bleeding in this disease, as we are in the habit of using the lancet with great moderation, and that less as a direct remedy than as preparing the way for the more efficacious use of the colchicum. The fact stated by M. Magendie of the non-occurrence of secondary visceral inflammation, where bleeding has not been employed, is valuable as coming from so experienced a practitioner, whether his explanation of it be correct or not.

Returning to the subject of the viscidity of the blood, M. Magendie relates an experiment of M. Dupuy, in which water, holding in suspension fresh cerebral matter, being injected into the veins of a horse, caused immediate death, and on dissection directly after, the lungs were found gorged with blood, and their parenchyma filled with petechiæ. M. Dupuy saw these infiltrations of blood take place before his eyes. These results are attributed, both by M. Dupuy and M. Magendie, to the large size of the particles of nervous matter which prevents their transit through the capillary vessels. In these cases of artificial pneumonia, an injection cannot be made to penetrate the vessels which are blocked up with globules of the foreign matter. The arteries are almost empty and the veins gorged with black blood. (pp. 151-2.) A similar experiment is performed by M. Magendie on a dog, with a corresponding effect on the lungs; but the crural artery is found to contain florid blood, because the death of the animal has been so sudden that the arterial system has not had time to empty itself. (pp. 153-4.) From the rapidly fatal effect of the cerebral matter in these experiments, M. Magendie was at first led to suspect that it had some actual poisonous property; but he afterwards shows, by two other experiments, that this is not the case. The portal system, he observes, affords the best means of putting it to the test, since any substance blended with the blood of the vena portæ, must pass through the parenchyma of the liver, and undergo a complete comminution of its particles before it can pass into the general circulation. Having exposed a portion of intestine, he injects some of the cerebral emulsion, with Anel's syringe, into one of the branches of the vena portæ, having previously applied a ligature on the orifice in the vessel to prevent the escape of the liquid. No deleterious effect is produced. (pp. 159-60.) The other experiment also is very ingenious. It is well known, says M. Magendie, how rapidly the effect of a poison is developed by contact with the nervous pulp; if, therefore, we find that the substance in question, when injected directly towards the brain, produces not the effects of general poisoning, but simply those of obstructed cerebral circulation, we may infer that it is innocent in its nature. Some of the cerebral emulsion is therefore injected into the carotid artery of a dog. The animal immediately struggles violently, shows extreme anxiety, rolls himself up, and inclines his head forcibly to the side on which the liquid was injected; the eye of the same side is convulsively turned upwards, and agitated with a peculiar tremulous motion. Now, as these phenomena appear referrible rather to obstruction of the cerebral circulation than to any poisonous effect on the system, this experiment corroborates the conclusion from the preceding. The general inference seems fair enough, but our author is not correct in stating that the effects of poisons are very rapidly developed by contact with the nervous pulp, for it is well ascertained that poisons, which appear to act on the extremities of the nerves and through these organs on the brain and spinal cord, produce no effect whatever when applied to the cut surface of the brain and nerves, or to the latter in any part of their course.* This series of experiments concludes with one in which a gros (two drachms) of a concentrated solution of corrosive sublimate is injected into the jugular vein of a dog. The animal dies in the course of the day. On dissection the lungs are found hepatized and gorged with black blood. The right cavities of the heart are distended with fibrinous clots, and their lining membrane is blackened; the left cavities are collapsed and nearly empty. The usual effects of the poison on the intestinal mucous membrane are not observed, which M. Magendie attributes to the circumstance that, as digestion was going on at the time of the experiment, the chyle protected the intestine from the action of the poison.

These researches on the viscidity of the blood are of considerable interest both in a physiological and pathological point of view; and we shall have occasion to refer to the subject again, when noticing the contents of the fourth volume. We see no objection to the general mechanical principle which M. Magendie seeks to establish; he appears, however, to generalize too far when he predicts with confidence what would happen in man from what happens in a dog. Thus, although it is very probable that the injection of a solution of gum into the veins of a man would be fatal, we should not be in the least surprised if it were to turn out otherwise, because there may be a variety of circumstances of which we know nothing, in which the properties both of the blood and the vessels which contain it differ in animals of a different species; for example, how different must be the state of the capillary circulation and the relation of the extreme blood-vessels to the rest of the system in an animal which perspires, as man, and one that does not, as the dog.

Elasticity. Three kinds of elasticity, says M. Magendie, are admitted by natural philosophers: 1, the elasticity of *compression*, by which bodies yield to pressure, and resume their former shape when it is removed; 2, the elasticity of *traction*, by which bodies when forcibly stretched resume their original dimensions as soon as the force is removed; 3, the elasticity of *torsion*, by which bodies, when twisted, resume their original position when the force is removed. From elasticity also result those vibrations in the particles of bodies which give rise to different sounds. The organic textures are all endowed with elasticity, some of them in a very high degree; and, according to M. Magendie, many actions which have been attributed to peculiar vital endowments, depend entirely on this property. Various instances are adduced of elasticity in the different textures which we need not particularize, unless it be that of the elasticity of the lungs. M. Magendie maintains that it is to the loss of this property that the embarrassment of the respiration in cases of pulmonary emphysema is owing. He and M. Dupuy are convinced, from their joint investigations, that broken wind in horses is caused by a loss of elasticity in the lungs arising from emphysema. (pp. 169-70.) Our readers are aware that this is a very old opinion of pathologists.

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* Christison on Poisons. 3d Ed. p. 25.

The opinions of M. Magendie on the effects of the elasticity of the blood-vessels coincide with those of some other physiologists, who deny the influence of any vital contractility of these vessels on the circulation. There is nothing very new in the illustrations or reasoning adduced, and we shall not argue the question at present, because it will come before us more in detail in a future part of these lectures. The fact that it has been so much argued, and that the opinions of the best physiologists remain so much divided, is sufficient to show that data are still wanting for positive conclusions as to all the powers concerned in the circulation of the blood. Yet M. Magendie tosses to the wind all opinions opposed to his own as "a whimsical assemblage of absurd suppositions," and declares his belief "that the human mind never produced anything more ridiculous than the pretended mechanism by which it has been attempted to account for the capillary circulation." (pp. 185-6.) Is it not strange to find M. Magendie afterwards freely admitting that the various local changes which take place in the circulation, independently of the action of the heart, depend "on something peculiar—something which has not, hitherto at least, come within the domain of physics?" (Vol. i. p. 203.) These changes, he supposes, are produced by the influence of innervation; and he adduces the well-known fact, that when a part is deprived of its supply of nervous energy, the circulation is immediately disturbed, or even suspended. May not M. Magendie's "unknown something" consist in the vital properties of the extreme blood-vessels which he regards with so much contempt? And may not a cause which operates so energetically on some occasions, be constantly in operation to a certain extent?

M. Magendie makes some observations on the modes of arresting arterial hemorrhage, as acting on the principle that the three tunics of arteries are possessed of elasticity in different degrees. The operations noticed are the application of the *ligature—torsion—abruption*, or the tearing of the vessel—and *perplication*, a method proposed by M. Stirling, a German author, and which consists in making a small incision in the side of the artery near its bleeding orifice, then introducing a small pair of pincers, seizing the open extremity and drawing it backward through the aperture made in the side of the vessel, so as to form a kind of knot. The success of the three first of these operations is connected with the greater elasticity of the external tunic of an artery than of the middle or internal tunic; but we do not see what elasticity has to do with the effect of perplication.

The last hundred pages of the first volume are devoted to the consideration of *sounds*, normal and anormal, as produced in the human body, and particularly in the heart and arteries. With some glaring defects and more inconsistencies, this section contains much valuable and interesting matter. For the present, however, we pass it by, intending to return to its contents in an early number, when we come to notice some other works on the same subject.

In the volume which we have just closed, we have been startled with strange novelties, puzzled with conflicting experiments, and overwhelmed with a rapid succession of subjects, any one of which might afford diligent occupation for half a life-time. In the second volume, we are allowed more breathing time; its matter indeed is spun out, and all that it contains might have been set forth as lucidly in a third part of the space. After some general remarks, the greater part of which are a repetition of those made at the opening of the first volume, and some observations on various subjects too inconsecutive to require or admit of notice here, M. Magendie enters on the subject of hydraulics.

Vital Hydraulics. Our attention is here naturally directed, in the first place, to the heart, the great hydraulic machine by which motion is communicated to the mass of fluids. The investigation of its structure and action seems highly congenial to the spirit of our author's enquiry; an organ whose source of motion resides in an intense vitality, acts in a manner purely physical, and through the medium of a mechanical structure on the exact adaptation of which the life of the whole organism immediately depends, yet so intricate withal that the utmost ingenuity has hitherto failed to explain the precise use of all the parts which compose it. In the absence of any original observations on the subject, we think that M. Magendie should at least have given a somewhat elaborate and critical view of the opinions that have been advanced as to the moving powers of the heart. His observations, however, are extremely meager and commonplace, and the only novelty is an attempt to establish an analogy between the heart and a *pump*, in which he thinks himself so successful, that the two sides of the heart are thenceforth distinguished as the *right* and *left pump*. We submit, however, that there are two conditions of a pump which the heart lacks, and which, unfortunately for the analogy, are precisely those which constitute the essence of a pump—namely, the play of a piston and the formation of a vacuum. To speak of a pump destitute of these is about as correct as it would be to speak of a steam-engine without a boiler and without steam; we can therefore only regard the comparison between the heart and a pump as a mere conceit. That it is nothing better, is sufficiently evinced by the absurdity of the language to which it gives rise. Thus M. Magendie speaks of "the dilatation of the pump," (vol. ii. p. 106,) and tells us that " the liquid which comes from the reservoir is projected with a certain force against the walls of the cavity which receives it; this cause, entirely mechanical, contributes powerfully to the dilatation of the body of the pump." (p. 108.) We were not aware either that a pump dilated, or that it was in any degree indebted for its action to the fluid which it set in motion; and we are convinced that M. Magendie would have been the first to laugh at such phraseology as the above, had it been used by any one else. There are, indeed, but too many passages in these lectures which would afford a facetious critic abundant opportunity of indulging his vein; and it must be confessed that M. Magendie would have little right to complain if the shafts of ridicule were launched against him, for no one departs more frequently than himself from the grave and urbane deportment of the true philosopher. As one out of many examples, we may cite the following polite notice of the *eclectics*, or those who are fond of conciliating extreme opinions.

> "If one could always discover and appropriate what is true in each system, this, without doubt, would be an excellent method. But do not trust these false illusions, these deceitful promises. Men should be judged of by their actions, not by their discourse. In what have these fervent followers of eclecticism deserved well of science? Do not the opinions they profess always betray their bastard origin? Poor eunuchs! Incapable of creating anything of themselves, they exhaust themselves in collecting here and there the fragments of errors which they pile up in the hope of arriving at the truth which they will never know." (Vol. ii. p. 67.)

We can assure M. Magendie that such rodomontade as this is neither conducive to the attainment of truth nor becoming those engaged in its pursuit. We must further maintain that, although the authors of original views have chiefly contributed to augment and impel the stream of human thought, the eclectics alone have directed its course in permanently useful channels, and led it to fertilize the fields of science. A teacher of any branch of knowledge, however endowed with creative genius, however distinguished for original discoveries, should always be an eclectic when in his chair; and placing his own opinions and those of others impartially before him, should separate facts from the inferences too frequently mistaken for them, and sound doctrines from specious theories. It is from the extreme difficulty of setting the mind free from an habitual bias, that many men, illustrious in science, make bad professors; their views require to be passed, with those of others, through the eclectic process, before they can become "profitable for doctrine."

To return from this digression—our author considers severally the functions of the two pumps, or, in ordinary language, the pulmonary and the general circulation.

Pulmonary Circulation. After some anatomical observations on the structure of the lungs, M. Magendie explains the pulmonary circulation in conformity with the opinion that the blood-vessels are simple elastic tubes—an opinion on which we shall have occasion to comment when we come to the general circulation. We meet with little otherwise which requires notice except some very peculiar pathological views, elicited by the presence of the influenza, which was very prevalent and fatal at Paris at the time these lectures were delivered. M. Magendie recurs to the experiments formerly rehearsed, showing that changes in the consistence of the blood cause effusion into the parenchyma of the lungs, disturbance of the circulation through them, and various changes of structure similar to those usually attributed to inflammation. There are some further experiments of a similar nature in the present volume, but they are scattered here and there with our author's characteristic want of arrangement. It were much to be wished that he had exhibited in a connected series all the experiments illustrative of the effects of foreign substances introduced into the blood, or the abstraction of any of its principles. We shall transfer hither some of those contained in this volume, and leave others until we come to notice the subject of the blood in the fourth volume.

A gros of sulphuric acid diluted with water is thrown into the jugular vein of a dog. The animal dies in a few seconds. The lungs are covered with brownish and livid spots; the right cavities of the heart are filled with clots, and the pulmonary artery and its branches are closed up by dense fibrinous masses; the left side of the heart is empty. Death seems here to be attributable to the sudden coagulation of the blood by the chemical action of the acid. (Vol. ii. pp. 179-80.)

A concentrated solution of subcarbonate of soda injected into the jugular vein of a strong dog causes instantaneous death. On dissection, the blood in the heart and large vessels and in the corpora cavernosa penis is found to be incoagulable; the lungs are in a state of *engouement*, and present the same brown spots and serous infiltration which M. Magendie has frequently observed in the lungs of animals which have died of hydrophobia: the cavity of the pleura contains a sanguinolent fluid; the liver, spleen, and kidneys, are softer than natural. (Vol. i. p. 319.) M. Magendie says he does not know whether any experiments have been made before his own on the introduction of subcarbonate of soda into the blood, and professes carelessness whether it be so or not, contending that the best plan is to make such experiments as suggest themselves, without reference to any which may have preceded them, and afterwards compare our results with those of others, so that what may have escaped one may be supplied by another. The only objection to this proceeding is that a man might spend his lifetime in ascertaining what was perfectly known before, or in drawing erroneous inferences from some source long since known to be fallacious. With regard to the effect of subcarbonate of soda on the blood, we may remark that this substance has been copiously introduced into the veins of the human subject in conjunction with the muriate, in the treatment of cholera by saline injections, without any bad result. M. Magendie afterwards finds that if water very slightly acidulated with sulphuric acid be added to blood, the coagulability of which has been destroyed by subcarbonate of soda, its coagulability is restored, and a clot formed exactly resembling the product of natural coagulation. The experiments were made on the blood of a guinea-pig. (Vol. iii. p. 75.)

We may add one more experiment illustrative of the effects of the *œnanthic ether*, a substance recently discovered by MM. Liebig and Pelouse, and which they suppose to be the principle on which the peculiar bouquet of wine depends. A gros of this liquor, mixed with an equal quantity of distilled water, is injected into the veins of a dog. The animal falls down intoxicated, and grows sleepy, the respiration becomes stertorous, and death takes place in three quarters of an hour. The principal appearances on dissection are engorgement of the lungs, a state of the intestines resembling inflammation, and other alterations, attributed by M. Magendie to the condition of the blood, which is found to be uncoagulated.

The following experiment is intended to illustrate the effects of abstracting a portion of the fibrine of the blood. Eight ounces of blood are taken from the jugular vein of a dog. The blood is agitated with a glass rod, and the fibrine is deposited in the form of yellowish filaments. The blood being then filtered through fine linen, is returned into the vein. The animal appears distressed, lies down, refuses food, and makes efforts to vomit. In the evening the same quantity of fibrine is again abstracted, and with similar but severer results; the animal becomes gradually weaker, the respiration is embarrassed, and death ensues. On the following day the body is remarkably rigid, somewhat swelled, and exhales a very fetid and putrid odour. On dissection the lungs are found hepatized; and there is a considerable quantity of reddish serum in the right cavity of the pleura. The heart contains a little serum with some small concretions suspended in it; but there is no true coagulation. (Vol. ii. pp. 196-200.) The experiment is afterwards repeated in a more gradual manner on several other dogs, a smaller quantity of fibrine being abstracted at once. In addition to the results already mentioned, inflammation and ulceration of the eyes and purulent discharges from the conjunctiva take place, and the skin is covered with vibices and petechiæ. (Vol. iii. pp. 324-9.) All these phenomena are attributed by our author to the changes in the blood induced by the abstraction of one of its elements. He draws no positive conclusion from the first experiment which he says came into his head only the day before it was made; but after repeated trials, he feels convinced that the effects produced were owing to the loss of the fibrine. We are amazed at M. Magendie's not being aware that experiments of the same nature were performed with very different results by Prevost and Dumas, some years ago; and that the results obtained by them have been fully confirmed by Dieffenbach* and Bischoff.† These enquirers have established beyond a doubt that blood deprived of its fibrine injected into the veins of an animal of the same species as that from which it was taken, or into the same animal, immediately restores it from the state of syncope occasioned by loss of blood, and that no bad consequences ensue. M. Magendie, indeed, afterwards alludes to the experiments of Dieffenbach and Bischoff, but merely states that those of the former *appeared* to succeed, while those of the latter failed entirely! (Vol. ii. p. 315.) The experiments of both these physiologists were perfectly successful and conclusive; and it is difficult to imagine the source whence M. Magendie can have derived such erroneous impressions with regard to them—surely not from the memoirs in which they are detailed by their authors. These experiments were instituted in relation to the subject of transfusion, and with a view to ascertain whether defibrinized blood could restore the actions of life; but if our object be simply to determine whether a change in the proportion of the fibrine to the other elements of the blood be productive of any immediate bad effects, there is no necessity for torturing unhappy dogs, because nature is continually making the experiment for us, and much more neatly, in the surgical wards of every hospital. When, for example, after the extirpation of a large tumour we find the cavity filled up in the course of a few hours with a dense mass of coagulated lymph, it is evident that a large quantity of fibrine has been suddenly abstracted from the circulating fluid, and that the blood must be minus this quantity. What ensues? Pneumonia, ophthalmia, and the putrid decomposition of the patient?—Not at all; the effusion of lymph is the sure forerunner of speedy reparation and recovery. We confess that we are quite puzzled to account for the result of M. Magendie's experiments; but that there was some source of fallacy we have not the least doubt.

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* Die Transfusion des Blutes. Berlin, 1828. † Müller's Archiv. 1835.

From the effects of alterations in the consistence of the blood, M. Magendie draws certain conclusions, which, if established, would render null and void at least one half of the science of pathology as it now stands. He objects to the term *inflammation*, as absurd and unmeaning, and seems to deny the existence of the state which it is intended to designate, attributing the phenomena usually so called to obstruction of the minute blood-vessels, owing to increased viscidity of the contained fluid, or extravasation into the parenchyma of organs from a preternatural tenuity of the blood. With respect to the term, it is singular that in almost all languages it involves the same idea, which looks as if there were something like common sense in its adoption. With respect to the state itself, we shall defer our observations till we notice the third volume, in which M. Magendie's views are more fully developed. The subject is only introduced at present in connexion with that of *influenza*. M. Magendie exhibits the lungs of numerous patients who have died of this affection. They present various degrees of engorgement and hepatization. These changes, according to M. Magendie, are the result of a dissolved state of the blood, which constitutes the essence of the disease. We cannot, however, see the slightest show of reason in favour of this opinion. That influenza is in many cases complicated with inflammation of the lungs is quite clear, both from the symptoms and from the benefit derived from the free use of the lancet. When such cases prove fatal, we believe that the inflammation, notwithstanding M. Magendie's contempt for that state, is abundantly sufficient to account for the appearances after death. We can conceive it possible, however, that in other cases similar changes may take place in the lungs, without any preceding inflammation; but here, instead of resorting with M. Magendie to a dissolved state of the blood, of the existence of which in the majority of cases we have not the smallest evidence, we should be inclined to regard an altered action of the vessels caused by disordered innervation as the source of the structural changes in question. The sufficiency of this cause for the production of the effect is well known, and is fully admitted and experimentally illustrated by M. Magendie himself. It appears to us, indeed, that the discovery of the influence of suspended innervation on vascular action, for which the world is indebted to Dr. Wilson Philip, has not been made sufficient use of in pathological reasoning; and we think it probable that many structural changes, which are attributed to inflammation, ought in reality to be referred to this cause. We are rather inclined to adopt this supposition, with respect to many of the local affections which occur in the course of adynamic fever, because disorder of the nervous functions is a prominent, and, perhaps, the most invariable and characteristic feature of this form of fever; because such disorder is known to be adequate to the production of the affections in question; and because these affections appear to differ from inflammation, both in their resistance to antiphlogistic treatment, and in the length of time they endure without inducing permanent change of structure. The same origin might, perhaps, be reasonably assigned to the organic changes in the viscera which are observed so frequently in the more protracted forms of insanity. But to return to the influenza: since this is evidently a disease affecting the whole nervous system, and the respiratory nerves in particular, would it not be more reasonable to ascribe the organic lesions which accompany it (supposing, with M. Magendie, that inflammation has no part in them,) to disordered function of these nerves, rather than to an alteration of the mass of the blood, especially as the changes are confined to the lungs?—whereas, if they arose from the state of the blood, they would be found wherever there was abundance of blood-vessels distributed in a lax cellular tissue. M. Magendie afterwards reads an extract from a letter he has received (p. 203), enquiring why, if the affection of the lungs in influenza depends on a change in the blood, all the vessels are not uniformly obstructed, and why one part of the lungs remains permeable, while others are hepatized? He acknowledges candidly that the objection is a grave one, and that he is unable to answer it. So much easier is it to frame short-sighted hypotheses than to answer the first and most obvious objections that may be raised against them!

It is singular that M. Magendie, in so often attributing local diseased actions to changes in the blood, forgets that an universal cause must have an universal effect, wherever circumstances admit of its operation; it is still more singular that he remembers this truth when it happens to suit his own purpose; for he adduces the fact that the *whole system* is affected in typhus in confirmation of his opinion that this disease consists essentially in a change in the *blood*.

> "These congestions of the parotids, these pneumonic obstructions, these petechiæ, that putrid decomposition which so forcibly struck the old physicians—these are so many symptoms connected with a *general morbid principle*. What is the material agent destined to place in relation the different textures of the living economy? It is the *blood*. Suppose this liquid altered, and you will have an excellent reason for the disturbances which fall on each organ and each apparatus." (Vol. iii. p. 9.)

This reasoning, as far as relates to the general influence of the morbid cause (we speak not here of its nature) is strictly just; and if M. Magendie had kept it always in view, he would have seen that no local diseased action can be attributed exclusively to the state of the blood, although the effects of a morbid state of the blood may be made to manifest themselves in one part rather than another, by particular diseased conditions of the solids.

M. Magendie makes some observations on the morbid anatomy of the lungs, in conformity with his peculiar views. They contain nothing very remarkable, except an opinion as to the cause of pulmonary apoplexy, from which we must entirely dissent. "One thing," says M. Magendie, "is very positively known with respect to cases of pulmonary apoplexy, carefully observed—it is, that the blood is very thoroughly altered, and has lost the power of coagulating so as to form a firm clot." (Vol. ii. pp. 249-250.) He adds, "I have seen individuals struck with pulmonary apoplexy discharge blood by stool, by urine, by cutaneous transpiration, and by the principal exhaling surfaces." (*Ib.* p. 250.) Pulmonary apoplexy is doubtless sometimes accompanied with a general hemorrhagic disposition, as described by Laennec, Andral, and others; and such cases appear to be much more frequent in France than in England; but in the majority of instances there is nothing of the kind. Pulmonary, like cerebral, apoplexy occurs in connexion with the most opposite states of the vascular system and of the blood; and we should say that if any one thing be "very positively known" with respect to either, it is the frequent coincidence of disease of the heart, which M. Magendie might have pointed out as a cause quite as *physical* and much better approved by observation than an incoagulable state of the blood; the latter, indeed, must be excluded even as a possible cause where the disease occurs, as it often does, in persons labouring under active plethora and too dense a crasis of the blood.

The latter part of the second volume and the whole of the third are occupied with the consideration of the circulation.

General Circulation. Our author's opinions on this subject are based on the supposition that the heart is the sole source of the power by which the blood is propelled throughout the system; the arteries, large and small, as well as the capillaries, he regards as simple elastic tubes, which, being distended by the blood, contract on it in virtue of their elasticity, and render continued and uniform the successive impulses it receives from the heart. That the larger arteries are not possessed of any power which can properly be called muscular, and that their ordinary action on the blood consists principally in an uniform contraction, which propagates an impulse originally received from the heart, is, we think, sufficiently established; that they possess elasticity is unquestionable; but that their contractile power resides solely in this property, we utterly deny, and we do so on *physical grounds.* 1. It was distinctly proved by the experiments of Hunter and Parry—and the fact may be verified by any one who will take the trouble—that the arteries contract, immediately after death, to a caliber smaller than that which they maintain some time after death, but before putrefaction has commenced: now the time to estimate the effect of mere elasticity in an artery, is evidently in the interval between the entire extinction of vitality and the commencement of decomposition, for the vessel is then possessed of its simple physical properties, and no other. 2. M. Poiseuille has demonstrated that if the artery of an animal just killed be distended with water, the force with which it contracts, in expelling the water, is greater than the force used to distend it, and greater than the same artery exerts a short time after death. 3. An artery of moderate size, if divided transversely during life, will contract to a point; that is, to a diameter smaller than that which it preserves after death.

From these facts, two conclusions appear inevitable; first, that the contractile power of the arteries depends on something more than mere elasticity, since it produces effects to which elasticity is unequal: secondly, that this power is a vital one, since it ceases with life. The hypothesis which regards the arteries as simple elastic tubes, though essentially false, does not lead to any very important practical error, so long as its application is confined to the large arteries, because, although the proper contractile force of these vessels differs from elasticity, both in kind and degree, it has nevertheless an analogous influence on the course of the blood; namely, that of continuing the impulse of the heart by an uniform contraction. When, however, we approach the minute extremities of the arterial system, the case becomes widely different, since we are here brought in contact with a new set of phenomena, and have to consider, not merely the force which propels the blood, but the means by which its movements are so regulated as to render it available for the purposes of life. It is only very lately, that the enquiry concerning the influence which the capillaries may exert on the circulation, by their vital actions, has been properly limited; indeed it still continues to be very vaguely pursued by many physiological writers. The question whether these vessels do, by their vital contractions, add directly to the force which *propels* the blood, is perfectly distinct from the question whether they do, by vital actions of some kind, regulate the *distribution* of the blood. The first question, we conceive, must be answered in the negative; because, admitting them to possess a vital power of contraction, the exercise of this power would antagonize the action of the heart, instead of aiding it. The second question is more difficult of solution; but as it is one of infinite importance to physiology, we will dwell on it for a moment. If we consider the capillaries as endowed with a vital power of diminishing their tube, and thereby lessening the quantity and augmenting the velocity of the blood which passes through them, and of dilating and thereby increasing the quantity and diminishing the velocity, we must admit that these vessels, by their vital actions, contribute most powerfully to the circulation of the blood, which, in the true physiological sense, does not mean the mere performance of a course from the heart, through certain tubes and back again, but the distribution of the vivifying fluid to every part of the frame, in a manner suited to the maintenance of nutrition, secretion, calorification, and other functions on which life intimately depends. Do the capillaries, then, possess such powers? We think there is evidence, little short of demonstration, that they do. As it is admitted on all hands, that local changes of diameter take place in the capillaries, independently of the action of the heart, what we have to ascertain is, whether such changes depend on mechanical or vital causes. Now many of these changes evidently take place under the influence of innervation. The glowing countenance of anger or shame, the alternately flushed or pallid visage of agitation or embarrassment, are universally referred to the influence of the nerves, the only channels through which the mind conveys its impulses to the corporeal organs. This is fully admitted by M. Magendie, who instances also the effect of dividing the pneumogastric nerve on the pulmonary circulation, than which no instance can be more to the purpose. If, then, sudden changes in the innervation of a part, have so marked an effect on its vascular functions, it cannot reasonably be denied that the ordinary and continued influence of the nerves is, in all probability, an important condition to the maintenance of those functions. But how do the changes of innervation affect the diameter of the vessels? It can only be by modifying their elasticity, or by influencing their vital properties. Now what ground have we for supposing that the nervous influence can effect an immediate change in the elasticity, or any other physical property of a texture? None whatever; there is no fact in physiology which affords the shadow of support to such an opinion. But we have ample proof that the nervous influence is capable of producing an immediate action, by affecting the vital properties of a texture, as when it causes the contraction of the muscular fibre. The influence of innervation, then, must be exerted, not on the elasticity but on the vital properties of the blood-vessels. It is true that some physiologists, and among them the distinguished Müller, have recently attempted to explain the phenomena in question, by the play of certain *vital affinities.* They suppose, that the influence of the nerves is to increase or diminish the vital affinity between the blood and the textures in which a change of distribution takes place; but, with deference to high authority, we must submit that the expression *vital affinity* appears to us exceedingly vague and indefinite, nor can we help suspecting that those who employ it would, if pushed hard, find considerable difficulty in explaining what they mean by it. Moreover, admitting the existence of such an affinity, we do not perceive how it could increase the quantity of blood in a part, unless new vessels were created, or those already existing dilated for its reception. The former supposition, we presume, will not be maintained, nor does the latter appear more tenable; for how can an *affinity*, in any conceivable sense of the term, dilate a vessel by forcing blood into it, in opposition to its mechanical tendency to contract? We must, therefore, conclude that the nerves do, in all probability, influence the vital properties of the capillaries, and thereby occasion changes in their diameter which regulate the distribution of the blood, and hence act a very important part in the circulation.

We have purposely abstained from enquiring whether the capillaries possess the texture of arteries and veins, or form a distinct order of vessels. We have made no attempt to determine whether the small arteries, which lead to the capillaries, possess vital endowments, similar to those we have attributed to the latter. These, and other disputed points, were not essential to our object, which has been to point out a few phenomena, the existence of which cannot be disputed, and to show that these phenomena necessarily involve the presence of a vital power in the extreme blood-vessels, which influences the distribution of the contained fluid. We are thus enabled to place in a broad light the fallacy of the doctrine, that the arteries are mere elastic tubes—a doctrine which, if admitted, would go far to divert all attention from enquiries which, in the present state of physiology, are perhaps of greater importance than any other; namely, those researches into the arcana of the capillary system, for the success of which the science is waiting, as for the probable means of indefinite extension. These things being premised, we are prepared to follow M. Magendie in his observations on the action of the blood-vessels, considered simply as parts of an hydraulic apparatus. He describes the instrument invented by M. Poiseuille, for estimating the force with which the blood moves, a very ingenious contrivance, which promises to be of great utility in researches of this nature. It is simple, of easy application, and seems to afford very uniform and accurate results.* M. Magendie repeats Poiseuille's experiment, showing that the force of the blood's motion in all arteries is the same, whether the vessel be situated near the heart or at a distance from it. Hence, if we ascertain the degree of pressure of the blood in any one artery, by the height of the column of mercury which it will support, the amount of pressure in any other artery of a given caliber may be ascertained, by multiplying the area formed by the circumference of the vessel by the height of the column of mercury which is supported by the pressure of the blood, as already ascertained, in the first artery. M. Poiseuille found, also, that the pressure of the blood was the same, whether the animal experimented on were large or small. On this M. Magendie remarks, that there is a circumstance which may influence, in some degree, the accuracy of the results obtained by M. Poiseuille's instrument: a certain quantity of blood mixes with some subcarbonate of soda, which is put into the tube in order to maintain the fluidity of the blood. In large animals the loss of this quantity of blood is insignificant, but in small ones it should be taken into account. (Vol. iii. p. 45.)

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* A figure of it, with explanations, will be found in the Cyclopædia of Anatomy and Physiology, and in Dr. Baly's translation of Müller's Elements. The worst thing about this instrument is its name—*hemodynamometer;* this, although more than sufficiently sesquipedalic, is yet too short for good etymology, which would make it *hematodynameometer.* See a good paper on this instrument, as indicating "the physiological effects of various agents introduced into the circulation," by Mr. James Blake, in the Edin. Med. and Surg. Journal, for January, 1839.

By the use of the same instrument it is demonstrated that the force of the blood's motion is remarkably increased by expiration and diminished by inspiration, a fact previously observed both by Haller and Magendie, and which depends on the increase or diminution of pressure on the large vessels by the contraction or dilatation of the thorax. M. Magendie affirms that the influence of the respiratory movements is much less in the crural than in the carotid artery, because, by the descent of the diaphragm during inspiration, the abdominal viscera are made to press upon the blood-vessels, and during expiration this pressure is removed, so that the effect of the movements of the thorax are in some degree antagonized by those of the abdomen. This differs somewhat from the observations of Poiseuille, who states that in ordinary respiration the increase or diminution of pressure is the same in all the arteries, but that during violent respiratory efforts the effects are much greater in the arteries near the heart than in those at a distance from it.*

M. Magendie makes an experiment to ascertain the effect of increasing the volume of the circulating fluid, which, he anticipates, will be to augment the pressure of the blood. He injects some tepid water into the jugular vein of a dog, but, contrary to expectation, the instrument indicates a diminution of pressure. (Vol. iii. pp. 50-1.) On reflection, he attributes this to the debilitating effect of the water on the muscular fibres of the heart. The experiment, however, being repeated, with transfused blood instead of water, no change of pressure is indicated. In a subsequent trial blood being withdrawn, by means of a syringe, from the crural artery of a dog, and the same blood being reinjected, the instrument marks a diminution of pressure as the blood is withdrawn, and an increase of it as it is restored; this, however, may be merely on the principle that the action of the heart is depressed by withdrawing its natural stimulus, and raised again by renewing it. (pp. 124-5.) An attempt to make a similar experiment on the jugular vein is frustrated by the sudden death of the animal from the introduction of air, and also, according to M. Magendie, from the sudden coagulation of the reinjected blood. (pp. 125-7.) He therefore, very properly, pronounces all these experiments inconclusive. We think, further, that it would be extremely difficult to devise any conclusive experiments on this subject, because the increased volume of fluid might operate not only by its mass, mechanically considered, but by influencing the irritability of the heart, and most probably influencing it differently at different times, according to the vital condition of the organ. An aqueous infusion of coffee, injected into the veins, causes a marked increase of pressure by augmenting the action of the heart. Diluted alcohol has no effect. (pp. 52-4.)

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* Magendie, Journal de Physiol. Tom. viii. p. 296.

While the pressure of the blood is equal throughout the arteries, it varies in every part, and almost in every tube, of the *venous system*. By the application of Poiseuille's instrument, M. Magendie demonstrates it to be much greater in the crural than in the jugular vein. The slower movement of the blood in the veins than in the arteries seems to depend on the greater capacity of the former vessels, rather than on the diminished influence of the propelling force of the heart; for M. Poiseuille has found that if all the blood carried to a part by an artery is made to return by a single vein, the pressure in the two vessels is nearly equal. M. Magendie repeats an experiment of Poiseuille's, illustrative of this fact. The crural artery and vein of a dog being insulated from the surrounding textures, the circulation through the other vessels of the limb is suspended by two tight ligatures: the pressure in the vein becomes equal to that in the artery. Even before the ligatures are tightened, the pressure in this vein is found to be very considerable, because the other veins are but small, and a great part of the blood returns by the crural vein. (Vol. iii. pp. 181-3.) On the preceding principle, the pressure in the deep veins is much greater than in the superficial, because the latter are much more numerous; and hence, observes M. Magendie, the much greater difficulty of suppressing hemorrhage from deep than from superficial veins.

Our author commences his investigation of the capillary system in a manner very characteristic of him. In alluding to the opinions generally maintained, he remarks ironically, that "there is nothing but extraordinary phenomena, from the sensibility which is insensible to the contractility which it has not been vouchsafed to any one to see. A globule of blood wishes to move in a particular direction—for it also has a will of its own;—the capillary will not let it. The vital laws are at strife with the physical; happily the former are victorious." This is smartly said, and if Verschuir, Thomson, Parry, Hastings, and many others, profess actually to have seen the contraction of the capillaries on the application of stimuli, who are they? *Nobody!* All of them put together would not make a physiologist of decent dimensions, according to the conceited estimate of M. Magendie. We shall not follow M. Magendie throughout his observations on the healthy functions of the capillaries; for there is nothing new in them, except the dogmatical decision of questions which the best-informed physiologists approach with diffidence, and regard as quite undetermined. We have already stated our acquiescence in the opinion that the capillaries do not contribute by their vital contraction to the propulsion of the blood, but we have also stated the grounds of our conviction, that the minute arteries, in the exercise of their vital powers of contraction and dilatation, afford indispensable aid to the circulation, by regulating the distribution of the blood and the various processes immediately dependent on it.

M. Magendie gives a summary of the observations of M. Poiseuille on the manner in which the blood traverses the vessels. All the observations of so ingenious and accurate an enquirer are worthy of attention, and we recapitulate these, as they have not yet found their way into the treatises on physiology. When a liquid passes through an inert tube, a portion of it adheres to the parietes of the tube, as by a species of attraction, and remains motionless. M. Poiseuille has found that this holds good, also, with respect to the passage of the blood through its vessels; an immovable layer of serum rests in contact with their parietes, while the blood traverses their tube. If the circulation be examined in a vessel large enough to admit of the passage of several globules abreast, their motion is perceived to be very rapid in the axis of the vessel, and less so near the parietes; while in the layer of serum adhering to the latter, the globules do not move at all. In the axis they have only a progressive movement, near the layer of serum they have a progressive and a rotatory motion; the latter being more conspicuous the nearer they approach it: the globules which are entangled in the substance of the serous layer become immovable, and remain so; those which merely touch it, roll on their axis as if they impinged on an undulating surface. The motion of the globules is slower in the distal portion of an artery than in that nearer the heart. The irregularities in the movements of the globules are referrible to their position in relation to the adherent layer. Thus, two globules may be moving abreast with equal velocity; one of them, jostled by its companion, is pushed towards the circumference of the vessel, and its progress is retarded; a shock from a new globule restores it to the centre, and it recovers its former velocity: at another time, a globule gets placed crosswise of the vessel, and its extremities become immersed in the immovable layer, its progress is therefore retarded; but others pressing upon it as it intercepts the passage, it soon yields, becomes longitudinal, and resumes its course along with the rest. These accumulations of globules happen very rarely, when the heart retains its full power; they are hence generally observed only towards the close of an experiment, when the animal is weakened. In the great vessels, the globules moving in the axis are not at all influenced by the immovable layer at the circumference, on account of their distance from it; in the capillaries, on the contrary, they are all obliged to traverse a mass of serum, and the central current only has a certain degree of velocity. (Vol. iii. pp. 259-61.)

Although we honestly confess that we are somewhat tired of wading through the deluge of ill-ascertained facts, hasty deductions, and gratuitous hypotheses, with which this work is overlaid, we must notice M. Magendie's views on the subject of *inflammation*, before we dismiss the third volume. These views have been several times partially introduced, and always with much confidence and infinite contempt of existing opinions: we might, therefore, naturally have expected a very distinct statement of them, and very striking facts, luminous illustrations, and conclusive arguments in their support. We acknowledge, however, that we have been more afflicted than surprised to find that our author ends in having no settled opinion on the subject. We have already seen that M. Magendie attributes what is called inflammation to mechanical obstruction of the capillaries from increased viscidity of the blood, or to extravasation of some of the elements of the blood into the texture of the part affected, owing to a morbid tenuity of that fluid. In entering more particularly into the subject, he attempts to explain the origin of inflammation as follows. If an alkali, or other chemical reagent be applied on any point of the mesentery of a living animal, the circulation is immediately arrested at that point. A dark immovable spot is perceived, all around which the capillaries are turgid, because the blood which should have flowed through the obliterated vessels, passes into the neighbouring ones, and distends them by its pressure. In this instance, the chemical agent causes a *racornissement* or crispation of the parietes of the vessel; but similar consequences ensue, whenever the normal relation between the diameter of the capillaries and the volume of the contained globules is subverted. (Vol. iii. pp. 423-4.) This hypothesis differs little from that of Boerhaave; its essence is obstruction—the globule being too large for the vessel, or the vessel too small for the globule. According to M. Magendie, the principal phenomena of inflammation are easily accounted for on these data.

> "The analysis of the course of the blood, in a part struck with inflammation, explains the signification of the four famous words *dolor*, *calor*, *tumor*, *rubor*. The pain results from the compression of the nervous filaments by the obliterated or distended vessels; the heat indicates, that the blood passes in more voluminous columns in the neighbouring capillaries which remain pervious; the swelling proceeds from the dilatation of the vascular net-work, and the extravasation of the materials of the blood; the redness depends on the presence of an increased number of globules in the vessels." (Vol. iii. pp. 128-9.)

M. Magendie regards the phraseology which expresses the dependence of inflammation on irritation, as a mere mystification of the old axiom, *ubi stimulus ibi fluxus*. If a puncture be made in the mesentery of a living animal, the blood flows from all quarters towards the injured point, and what is called inflammation is set up. This, say the physiologists, arises from *irritation;* but M. Magendie assures us that it is a mere hydraulic phenomenon: a capillary is punctured, the blood runs out, and, by a necessary consequence, that contained in the neighbouring vessels is determined towards the aperture, whatever were its previous course. He has observed, that if the tissue of the membrane merely be punctured, the course of the blood undergoes no change; he asks, therefore, how it can happen, if the phenomena depend on irritation, that the stimulus produces the effect when applied to the vessel, but not when applied to the membrane, the sensibility of the latter being infinitely the greater? We need scarcely suggest, that he is here confounding *animal* with *organic* sensibility; but setting this aside, we do not perceive how a visible change can take place in the course of the blood, where no blood-vessel is visible. The most familiar of all instances sufficiently shows the futility of M. Magendie's hypothesis. If the slenderest hair, or an impalpable particle of dust, get on the surface of the eyeball, in a person in whom the organ is at all irritable, the vessels of the conjunctiva immediately become injected in all directions; *ubi stimulus ibi fluxus*: but no capillary is punctured in this case; the *irritability* of the vessels —that despised and proscribed property—is alone concerned in the phenomenon. We shall not discuss the merits of M. Magendie's theory of inflammation, because, in so doing, we should merely be enquiring whether the pathology of that state had been advancing or retrograding for the last century; and were it otherwise, it would be scarcely worth while to contest opinions which the author himself fairly leaves in the lurch. He begins the last of these lectures with the following announcement,—a remarkable one, certainly, after the unhesitating confidence with which he has previously introduced his own views, and pronounced those of others to be nonsense:

> "It was my intention to have delivered, at the end of this course, the complete history of inflammation; but it has fared with me as with a traveller who views an object from afar. Distance, at first, prevents its true character from being appreciated: it appears small. As it is approached, its proportions increase; yet nearer, they expand still more, and finally, when we come close to it we are frightened at its gigantic size. Accordingly, I thought that a few meetings would suffice to consider all the aspects and explore all the depths of the important phenomenon of inflammation. I was mistaken. This question is much more complicated than it appeared to me at first sight, and for its study it is not sufficient to contemplate it at a solitary point of the capillary system. We must take into consideration each organ, and each texture, for they have each their individual mode of circulation; we must examine their vascular texture, and the causes which modify the course of the blood in their parenchyma, and thus rise gradually from exact physiological knowledge to the experimental analysis of inflammatory disorders." (Vol. iii. pp. 440-1.)

Entirely acquiescing in these sentiments, we may venture to suggest that candour in acknowledging hasty or erroneous opinions, though always laudable, might in many cases be advantageously superseded by the caution which would prevent their adoption.

In entering upon the fourth volume of these lectures, which is entirely devoted to the consideration of THE BLOOD, we must refer our readers to an article on the same subject in our twelfth Number, (*Br. and For. Med. Rev.*, vol. VI. p. 431,) to which we wish the following remarks to be considered as supplementary. In that article, we asserted that but little had as yet been done to aid our knowledge of the pathological changes which the blood undergoes in different states of disease. From the reputation of M. Magendie, we expected to find in these lectures, that this great deficiency would have become at least in part supplied by his experimental researches. But we regret to say that we have been as much disappointed in this expectation, as we have been in so many others, raised by the previous reputation of their author. No portion of the present work presents a more remarkable illustration of our Bacon's "Idola spêcus" than these lectures on the blood; and we think we may venture to affirm that saying this is equivalent to saying that no better illustraton is to be found in the whole course of modern medical literature.

The greater part of this volume is occupied with an examination of the properties of fibrine, the phenomena attending the coagulation of the blood, and the physical circumstances under which it may be retarded or accelerated. The properties of albumen and colouring matter are more briefly discussed, and the saline and other substances contained in the blood are scarcely noticed. The first lectures are occupied with a general sketch of the objects of the course: the author avows himself to be an ultra-*humoral* pathologist, conceiving that the sources of most diseases must be referred to changes in the blood. These changes he is for the most part disposed to regard as purely physical. Two conditions he considers to be indispensably necessary for the maintenance of life and health: 1, That the blood should consist of a certain proportion of liquid and solid parts; and, 2, That it should be in continual circulation throughout the system. The discursive style in which the lectures are given renders it rather difficult for us to condense the author's views.

"The blood," he observes, "in the living animal, easily circulates through the minutest capillaries, but if any one of its properties be altered, it has no longer the power of traversing those vessels; it becomes effused in the surrounding tissues, giving rise to extravasation, œdema, and inflammation. . . . The following experiment may be adduced in support of this opinion. This vessel contains liquid blood: it has the normal proportions of serum, globules, and saline matter. In short, it contains twenty-four out of the twenty-five principles which have been announced by M. Lecanu to exist in human blood. One principle is wanting, but the deficiency is not appreciable to the eye, and the blood appears in every respect analogous to that which circulates in the living animal. Nevertheless, if reintroduced into a vein, it will at first traverse the larger trunks, but when it reaches the capillaries, its course will be arrested; it will become extravasated, and the animal will soon die from this stoppage of the circulation in the capillary system. Yet nothing has been added to this blood. We have simply abstracted from it one of its principles which forms not more than one or two thousandths of the mass. I allude to fibrine, a substance which is liquid in the vessels, but which becomes solidified so soon as it has escaped from them. Hence it appears it is the fibrine which confers on the blood the property of traversing the most delicate capillary vessels." (Vol. iv. p. 43.)

We cannot agree to this conclusion, which seems to us to be little warranted by the experiment. Had the author removed the colouring matter or the albumen, or the saline matter only, and left the other principles, we are pretty well assured that the capillary circulation would have been equally disturbed, and would have led to serious consequences. At any rate, until experiments had been performed on the other principles individually and with negative results, it is not justifiable to ascribe to the presence of fibrine alone the property which the blood has, of traversing the capillary vessels. He seems to have felt in some degree the weakness of his position, for immediately afterwards he says:

"The blood may contain fibrine and its other constituent parts, but if a substance capable of chemically combining with it and forming a salt, as potash, soda, or ammonia, be injected into the vessels, the fibrine loses the power of coagulating: the blood not containing this principle in its *normal* state, will itself cease to be coagulable, and consequently will not have the power of circulating freely through the capillaries. Thus even fibrinous blood may be unfitted for circulation. Here then we have a valuable and fundamental fact in the history of the blood, namely, that in order to support life, this liquid must have the property of coagulating. When this is not the case, death is a speedy consequence." (*Ib.* p. 44.)

"The causes which deprive fibrine of its property of coagulating exist in the air, in miasmata, in food, in short in all that surrounds us, and becomes in any way a part of our organization. Connected with these researches, we may notice the employment of certain medicines calculated to give rise to serious consequences. Thus the use of carbonate of soda carried to a great extent, as it sometimes is in calculous disorders, may become injurious. The explanation is as follows: this substance has the property of rendering the blood liquid by combining with the fibrine. I am persuaded that from the too frequent use of this salt, the blood becomes changed,—liquefied,—hence there are infiltrations in the lungs, followed by successive attacks of pneumonia.

This at least occurred in the case of one of my friends, who was obliged to give up the use of the medicine." (Vol. iv. p. 48.)

Here we see the following points assumed: 1, That carbonate of soda, introduced into the stomach, operates chemically on the blood, in the same way as when it is injected into a vein; 2, That in both cases it acts only by liquefying the fibrine, and consequently obstructing the capillary circulation through the lungs; 3, That miasmata or febrile poisons and alkaline substances affect this liquid in a similar way, and cause death by giving rise to similar changes in its properties. The physical alteration in the blood is all that M. Magendie appears to look to; and whether it be due to carbonate of soda injected into a vein, or to the contagious miasmata of fever, is a matter apparently of little importance. The fibrine having lost its coagulating power, the blood, it is alleged, cannot circulate through the capillary vessels, and hence death ensues. On this principle, he says, artificial diseases may be formed "*de toutes pièces,*" (p. 7;) and, at the pleasure of the experimentalist, animals may be made to suffer from pneumonia, scurvy, yellow fever, typhus fever, &c. (p. 5.)

Admitting that carbonate of soda taken internally is actually absorbed as such into the blood, a point which M. Magendie assumes, we have yet to learn whether, when the salt is in that liquid, it operates only by annihilating a property of fibrine which does not belong to that principle until after its removal from the living vessels. Fibrine is already liquid in the blood,—how therefore can it be said that the salt acts by *liquefying* what is already liquid? But let us advance a step further and admit that the alkaline substance destroys a property not possessed by fibrine in the living body, it has yet to be proved that the liquid compound of fibrine and soda acts fatally in obstructing the capillary circulation. Is not the salt as likely to be mixed with the albumen as with the fibrine, and might it not act fatally on the system, when thus mixed up with the albumen or serum? M. Magendie supposes that it combines with the fibrine alone, when introduced into the blood; and that it thus destroys life: but we search in vain throughout the volume for a single experiment to countenance this supposition. In short, all we learn from his statements is that a salt which retards or prevents coagulation in blood removed from the body may destroy life when introduced into the circulation. The *modus operandi* is left wholly unaccounted for. Some salts have but little effect on the coagulation of blood; others accelerate it; but the author has not deemed it necessary to perform parallel experiments with these: and yet it is clear, if these salts, when injected, produce the same degree of mischief in the animal, his hypothesis must fall to the ground. Can an animal bear with impunity the injection of a saturated solution of alum into its blood? We think not. Alum, according to most chemists, has the property of favouring the coagulation of that liquid when removed from the body; and yet it remains to be proved whether it will not destroy life as readily as carbonate of soda, although the latter has the reverse action on blood. In truth, it appears to us wholly unjustifiable, in the present state of our knowledge, to assert that the action of salts introduced into the circulating fluid is at all indicated by their power of preventing or favouring coagulation. We say nothing of

Magendie's view of the supposed serious consequences liable to follow from the medicinal use of the carbonate of soda, since experience plainly shows that he is in error. Most practitioners must have seen this salt taken in considerable quantities by healthy individuals, and for a long period together, without any such effects ensuing as those described by him. (p. 41.) Again, because the blood does not coagulate when drawn in certain diseases, as cholera and yellow fever, miasmata are supposed to operate like carbonate of soda in keeping liquid, the fibrine. This speculation is ingenious, but we do not see, even if it be admitted, how it advances our knowledge of the causes of non-coagulation in these diseases, or of the cause of death.

After denouncing the modern doctrines of inflammation and irritation as romantic fables unworthy of science, the author proceeds to remark on the *viscidity* of the blood, as a condition indispensable for its circulation through the capillaries. He once more refers to the interesting observations of M. Poiseuille already noticed, which prove that water cannot be forced through tubes of small caliber, even with the greatest pressure; but that if a certain portion of mucilaginous matter be added to the water, such as gum, gelatine, or albumen, then the injection is easily accomplished. (p. 57.) M. Magendie assures us that if the viscidity of the blood be diminished, by the abstraction of successive portions and the substitution of water, the circulation is impeded, and the extravasation with serous exhalation follows; if it be rendered more viscid than natural by the introduction of any foreign substance, then its course is arrested through the close adhesion between it and the parietes of the vessels.

We find nothing of importance on the subject of the coagulation of the blood; but the author admits the liquid to be of a very different nature in and out of the body. In relation to the proportions of serum and crassamentum, he considers that where the serum is unusually abundant, venesection ought to be avoided, and this he believes to be an opinion which sooner or later will be recognized as a fundamental doctrine in the treatment of disease. An experiment performed on a dog, however, showed that, although this animal had been three times bled, and distilled water substituted each time for the blood abstracted, the result did not bear out this view. The blood, when drawn for observation, yielded but little serum. In relation to this unsatisfactory result, the author very properly observes that one fact cannot overthrow another. (p. 105.) This we freely admit; but at the same time, when such apparently conflicting results present themselves, we are not in a condition to draw a conclusion one way or the other.

The detection of albumen in the urine has been in general considered to indicate the existence of disease of the kidney: in our last Number we showed that this was a mistake: and M. Magendie informs us that he has found that a superabundance of serum in the blood is attended with albuminous urine. Thus human serum was injected into the veins of a dog: on examining the urine of the animal, and, at the same time, a specimen of urine from a patient labouring under diseased kidney, albumen was discovered in each, in apparently the same proportion. After death, the body of the dog presented no trace of diseased kidney. (p. 148.)

The author considers it a matter of extreme importance to trace out those substances which have the property of liquefying the blood, in other words of preventing its coagulation out of the body. One of these, the carbonate of soda, has already been alluded to. Putrid water is another agent of this description; when injected, it destroys life by rendering the blood liquid, but the chief seat of its action is on the intestinal canal. (p. 199.) The various kinds of fever may be considered as having their origin in miasmata introduced into the lungs, and through those organs into the blood. In the subjects of these diseases, he believes the blood to be altered in its properties, but has not been able to verify this by observation.

The effects of certain reagents on the blood are next adverted to. *Sulphuric acid* is generally set down as accelerating coagulation, and thus it has been used internally and externally as a styptic. The author from one experiment comes to a directly opposite conclusion. Sulphuric acid mixed with blood, diluted with an equal part of water, merely rendered the colouring matter black: there was no solidification of the fibrine. Hence it is inferred that this acid, administered internally, instead of favouring coagulation, may actually prevent its occurrence. (p. 204.) The result of this experiment might be accounted for by the very small quantity of sulphuric acid used; but whatever explanation we may adopt, we cannot agree in the conclusion of the author, and we think that general experience plainly contradicts it. The muriatic, acetic, oxalic, tartaric, and lactic acids were found by him to prevent coagulation. The blood, in these experiments, was quite liquid. We are surprised that no mention is made of the remarkable effect of the vegetable acids, more especially the oxalic, on the colouring matter. This is assuredly the most striking and obvious change produced by these substances on the blood. Potash, ammonia, lime-water, the hydrosulphuret of ammonia, and nitrate of potash were then tried. All of these, with the exception of the hydrosulphuret of ammonia, were found to prevent the formation of a coagulum. Other substances were then mixed with recent blood, and the results noted. The carbonate and bicarbonate of soda prevented coagulation; the sesquicarbonate of ammonia gave no satisfactory result; the carbonate of potash liquefied the blood, but differed from that of soda, in turning the liquid black; (?) the sulphate of potash gave a precipitate of albumen and globules; the chloride of calcium, no appreciable effect; solution of chlorine gave a dark tint without coagulation; sulphate of iron produced an evident chemical reaction, throwing down albumen; alum prevented coagulation; (?) the bichloride of mercury and acetate of lead precipitated albumen; the ferrocyanate of potash entirely liquefied the blood; the nitrate of silver precipitated albumen; phosphate of soda gave a coagulum, as did likewise tartar-emetic; alcohol had but a slight effect,—hence in small doses, this liquid is not injurious; cinchonine, in the proportion of a grain, allowed a thin coagulum to form; sulphate of quinine, a scarcely perceptible coagulum; and a decoction of dried digitalis prevented coagulation. (p. 211 et seq.)

The author candidly acknowledges that this series of experiments is not complete, that the results in some instances have left doubts upon his mind, and that some of the experiments will require repetition. We are glad to find this acknowledgment made, because we cannot ourselves put any confidence in the greater number of the results. Thus the neutral sulphate of potash is represented as giving a precipitate of albumen and globules, while the acidulous solution of alum, contrary to the observations of most chemists, is laid down as preventing coagulation. What clear inference can be drawn from the addition of complex metallic salts to the blood? How can the effects of the nitrate of silver be compared with those of the phosphate of soda and sulphate of potash, when we know that the first-mentioned salt is so easily decomposed by all kinds of organic matter, and the last two are not? So again, who can suppose that the grain of cinchonine had any effect in producing the coagulum, which it is described to have done? We have thought it right to quote these experiments, because upon them the author appears to rest the originality and importance of his views. Our readers will perceive that he has not taken any pains in the performance of them, to avoid fallacies which must attend all such investigations. The peculiar effect of metallic salts is not allowed for, nor does it appear that specimens of blood were preserved *unmixed*, to compare with those to which the substances have been respectively added. Without this latter precaution, it is impossible to connect the result with the experiment so strictly as sound induction requires. We cannot join him in applying these results to the living system, nor can we agree that the results obtained in test-tubes on blood drawn from the body, are necessarily the same as those which follow when the substances are injected into the living vessels. In one part of his work, already quoted, he admits that the blood is different in properties in the two cases; but apparently forgetful of this statement he is continually applying his experiments out of the body, without the least discrimination, to those effects which take place in parts and organs endowed with life, and therefore under very different conditions.

Other experiments are related with other acids and salts; but these we do not consider it necessary to extract, since they are open to the same objections as the preceding. While boracic and arsenious acids liquefy the blood, he finds that borax produces a clot. On inspecting an animal which had died from an injection of citric acid into its veins, he discovered, exactly as he had anticipated, extravasation of blood and serum in the lungs. "It is true there was no decided congestion, but that was explained by the injection not having been sufficiently concentrated to put an immediate stop to the circulation." (p. 242.) Among the practical conclusions, we find the following. In order to determine in what dose sulphuric acid acts as a poison, he took eight equal portions of blood,—to the first he added one drop of the acid, to the second two, and so on with the rest. In the first vessel the blood was entirely liquid and incoagulable; in the third it was still more changed, and he felt confident "that an animal in whose veins such a liquid existed, would suddenly die." (p. 237.) "Again, hydrocyanic acid is one of the most violent poisons: six drops of it added to a certain quantity of blood, diluted with five parts of water, caused the globules and fibrine to disappear, an alteration in properties so great, that it was no longer surprising this poison should so rapidly destroy life." (p. 254.) These are specimens of the vague generalities in which the author frequently indulges!

We shall pass over the experiments relating to the influence of gases on the coagulation of the blood, and proceed to notice our author's views of the nature of the *buffy coat.* In various lectures we find this subject adverted to, and, in general, the remarks on it are accompanied by a sweeping condemnation of the modern doctrines of pathology. Magendie considers, and in our opinion correctly, that the fibrine and colouring matter are distinct principles in the blood, and that the buffy coat is nothing more than a layer of fibrine, coagulated without the red particles, containing commonly a portion of serum. The separation is due to the difference of density between the fibrine and red particles; but we do not agree with him in thinking that the greater density of the latter is to be ascribed to their containing iron. (p. 285.) The peculiarity of our author's views on this subject is, that he denies the connexion which pathologists have stated to exist between the formation of a buffy coat and the presence of inflammation. He denounces this as a chimerical notion, which it will require but a few years to banish entirely from the doctrines of the schools. The reasons for his opinion are well set forth. He has often looked for it, in cases where it might have been expected, without success: an analogous condition of fibrine frequently occurs in the coagulation of the blood of horses, without its being connected with any inflammatory condition of the body; lastly, its occurrence in human blood depends on numerous extrinsic circumstances, totally unconnected with disease, even where inflammation undoubtedly exists. Thus, if the opening made in a vein be too small, or if it be not parallel with that in the integuments, or if a globule of fat intervene, so that the blood froths in escaping, the buffy coat will not form on the coagulum. If a large opening be made, and the blood be received in a deep narrow vessel, it may assume in coagulating a buffy character. Hence, those who trusted in this sign, as indicative of inflammation, would meet with two opposite states of the blood in the same individual. If this were a true pathognomonic sign of inflammation, it ought to show itself in all similar cases, which it does not: but it is an accidental and unimportant condition, not at all fitted to become a guide for treatment. Why certain kinds of blood present this phenomenon and others do not, it is impossible, in the present state of science, to say. (p. 290.) These arguments of the author appear to justify the view which he takes; but we think experience has long ago fully established a connexion between this condition of the blood and an inflammatory state of the body; and, as he justly remarks, on another occasion, one fact cannot overthrow another, we would beg to ask, whether these apparently conflicting circumstances may not be reconciled. Thus we do not see that it is a necessary consequence of the general view, that the buffy coat must, as a matter of course, be present in every case of inflammation, or that it may not occur in other states of the system. Again we see no objection to the doctrine of the formation of this coat being modified by the manner in which the blood is drawn from the system; for physical circumstances out of the body may easily be conceived to favour or prevent it, nor can we admit it as an axiom that a condition of the solids or liquids, to be regarded as a pathognomonic character of a particular disease, should be universally met with. Certainly, if the author's distinction between a pathological and an accidental change were to be regarded as true, pathology would be reduced to a chaos of useless facts. In practice the author's fears as to the effects of the generally received doctrine are, we think, groundless; because a well-judging practitioner does not continue to bleed his patient from the mere occurrence of a buffy state of the blood, but he is guided by numerous other circumstances.

The author now passes to the consideration of the other principles of the blood, namely, the albumen and globules; and here we find more original and interesting matter than in the first part of the volume. The most striking difference between fibrine and albumen is that the latter does not possess the same property of spontaneous coagulation. Albumen may be solidified by heat and various reagents; but when thus coagulated, it does not present that filamentous network observed in coagulated fibrine. (p. 283.) Alcohol is well known to cause the coagulation of albumen, a circumstance which proves that alcoholic liquids should be taken in moderation. If a pretty strong dose of alcohol be given to an animal, it speedily dies, and albumen is found coagulated in its vessels. (p. 311.) (?)

Albumen coagulated by alcohol differs from that coagulated by heat, principally in the fact that it is in small flocculi; while albumen which has been heated is met with in large masses. In these remarks we must not confound the albumen of the egg with that of serum. These two liquids are distinguished from each other by some well-marked characters. If a small quantity of solution of potash be added to the albumen of egg, the whole becomes a transparent gelatinous mass, very much resembling pure gelatine in appearance. It is an albuminate of potash. On adding the alkali to the serosity of ascites, a slight precipitate was produced, but the mixture was quite liquid. (p. 312.)

It is singular that Magendie should have used the serosity of ascites, when immediately before he had been speaking of the serum of blood. In repeating this experiment, however, we have found that serum is merely diluted by the alkaline solution, while the albumen is gelatinized. In admitting this difference, we must remember that albumen of egg is much more viscid and firm in its ordinary state than serum. Potash in excess, it is to be observed, equally prevents serum and albumen from coagulating by heat, a point of resemblance not adverted to by the author, but which materially affects the results of some of his experiments.

The following differences are new to us:

Acetic acid, added to serum, produces no particular effect; added to albumen it causes an apparent partial coagulation, but which is due to a separation of the membrane of the cells. On boiling the two liquids there is a striking dissimilarity in the results. The acetate of serum becomes opaline from partial coagulation, but it remains liquid. On cooling it becomes opaque and solid; on reheating it, it again becomes liquid. The acetate of albumen is differently affected by heat; it forms a solid coagulum, separating almost entirely from the acetic acid, which remains transparent and slightly gelatinous. A continued application of heat does not reliquefy the coagula of albumen.

Pure ammonia, added to serum and albumen, produces no particular effect; when boiled, the ammoniacal serum does not coagulate, while the ammoniacal albumen swells up and coagulates into a light spongy mass. (p. 312.)

The lecture is here so badly reported that we have thought it better to give the author's meaning, than to adhere to a strict translation.

M. Magendie remarks that there is a striking difference between albumen of serum and fibrine, for many substances solidify albumen, while few have the property of liquefying it. It is just the reverse with fibrine.

Many other reagents which affect albumen are next pointed out. Ether is commonly said to distinguish albumen of egg from serum, because, while it coagulates the former entirely, it scarcely affects the latter. This is generally true; but we have met with specimens of serum in which ether, besides dissolving out the oil, produced a coagulum. Oil of turpentine does not, in our view, exert a different action on these two liquids, as it is often stated to do. It coagulates both, when the mixtures are well agitated. M. Magendie asserts that the sulphate of lime in solution coagulates albumen; and he proceeds to draw an inference therefrom on the use of hard water as a diet drink. (p. 323.) In repeating the experiment with a perfectly saturated solution of that salt, we did not find that it had any other effect than the addition of distilled water. It separated the membrane of the cells in loose flocculi, but nothing further. It merely diluted serum. The very imperfect solubility of this salt will easily explain why it should exert no action on these liquids.

The author considers that he has made an important discovery in the fact, that when albumen of egg is injected into the veins of a living animal, it is speedily and rapidly deprived of those characters by which he has shown it to be distinguished from serum; in short, he imagines that it is transformed into serum. The experiment is not unattended with danger to the animal. "The albumen of four eggs, carefully strained, was introduced into the jugular vein of an animal. It was immediately seized with vomiting, but it lived two days without much pain, and then the albumen of two eggs was injected: on the following day a similar quantity was introduced, and the animal died immediately." (p. 324.) "It was once bled a few minutes after the injection, and its blood separated, on standing, into a liquid and solid part: but the serum, mixed with potash, did not form a gelatinous mass like albumen; it remained perfectly liquid. Hence it is to be inferred that this substance (albumen ovi), in traversing the economy, is entirely deprived of its distinctive characters." (p. 325.) That the animal should have died from the repeated injections of a thick and tenacious substance like albumen, so different from serum in its physical properties, does not in the least surprise us; but that the albumen thus employed was actually transformed into serum is a statement which we do not think can be admitted, since, to warrant the conclusion, the author should have examined the whole of the blood, and not a few ounces. Other objections might be urged to this speculative view. For instance, how far is the action of the alleged distinguishing tests likely to be affected by the intermixture of the two substances in different proportions? Can they detect a small portion of albumen ovi in a large quantity of serum? If not, what becomes of the conclusion? The author partly furnishes us with the means of answering this question; and we shall quote the following paragraph to show that we are justified in asserting that his experiments are not always trustworthy.

> "Here is another fact not less curious in support of what has been previously stated. Before the lecture, some human serum was mixed with $\frac{1}{10}$th of its volume of albumen ovi. My object was to determine whether we should have the same result as that observed in the living vessels (the transformation of albumen into serum); and, however extraordinary this may appear, it is undoubtedly the case. The albumen has disappeared, and the serum, treated by potash and heated, does not coagulate as the albumen ovi would do under the same circumstances. This appears to me to be a sufficiently convincing proof, from which we may conclude that the simple intermixture of serum with albumen ovi deprives this last-mentioned substance of the property of coagulating by heat." (p. 332.)

We have looked among the errata for some explanation respecting this paragraph; but finding none, we must charitably suppose that the experiment was hastily performed, and the conclusion inconsiderately drawn. We think it our duty to point out the error into which, we conceive, the Professor has inadvertently fallen. Potash, added to albumen or serum in sufficient quantity, prevents either liquid from coagulating by heat; and the presence of $\frac{1}{10}$th of albumen in serum can of course make no difference in the action of the alkali. Possibly the author may have intended ammonia for potash; but if we allow the substitution, it will assist him but little in his inference; since we cannot expect that the action of this alkali, aided by heat, should be very different on a liquid which was pure serum, or on one of which nine tenths were serum. The pretended transformation of albumen into serum, is as little established by these experiments out of the body, as by those which were performed on a living animal.*

We are glad to find that the opinion which we expressed in our Twelfth Number, relative to the experiments of M. Denis, on fibrine, is fully borne out by the result of their repetition before M. Magendie. Our author states, and we think justly, that there are not sufficient reasons for believing albumen and fibrine to be one and the same substance slightly modified. M. Denis's opinion of their being identical was chiefly founded on the statement "that fibrine, digested in nitrate of potash, becomes dissolved, and then acquires all the properties of albumen: among others, that of becoming coagulated by heat." The author shrewdly observes: "M. Denis, who is one of my colleagues, came to announce this fact to me; but, as I prefer seeing things with my own eyes, and not with the eyes of others, I begged him to repeat his experiments before me. They were accordingly performed in this very theatre before many of you; but unfortunately not one of the results announced was obtained." (p. 331.)

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* Since the preceding observations were written, we have had occasion to repeat some of M. Magendie's experiments, particularly those on *serum* and *albumen*, and are now in a condition to speak positively on the subject; we think it, however, better to let the remarks stand in the text as they were originally written.

M. Magendie states the differences of albumen and serum as follows: 1. Potash forms a jelly with *albumen*, not with *serum*. 2. On ammonia being added to *albumen*, and the mixture boiled, the whole forms a solid spongy coagulum: on ammonia being added to *serum* and boiled, no coagulum whatever is produced, the whole remains liquid. 3. Acetic acid boiled with *albumen* forms a thick coagulum: with *serum* it forms, on boiling, an opaline liquid, becoming nearly solid on cooling, again liquid on heating, &c.

Now, of these assumed differences, we find that the first two are dependent entirely, as we suspected, on the different degrees of viscidity and consistency in the pure albumen ovi and serum; so that if the albumen be diluted with water to the consistency of serum, potash and ammonia act precisely in the same way on the two liquids. The effect of acetic acid and heat on serum and diluted albumen is different. Acetic acid does not prevent the coagulation of diluted albumen by heat, but its effect with serum is as above stated (3); hence this is the only true chemical difference.—REV.

We have already observed that M. Magendie regards the *globules* of blood as distinct from fibrine. He considers that of all the constituents of this liquid, these undergo the least change in severe diseases. In the dead it has been remarked that the membrane which forms the envelope appears shrivelled and contracted, in which condition M. Donné considered that there existed a well-marked sign of death. But M. Magendie established that the same condition of the globules existed in the blood of a healthy living man, when they had been allowed to remain for some time in a vessel. (p. 347.)

The following remarks are to us new with regard to the globules of blood: but we strongly doubt whether the so-called animalcules really deserve the title.

> "When the globules have become shrivelled on the surface, a number of infusoria (vibriones) show themselves in the serum. They give rise to vibratory and other motions of these globules, by striking against them. After a time they actually destroy their substance, and reduce them to opaque, indeterminate masses. Fresh globules introduced into serum, containing infusoria, were speedily attacked and destroyed by these animals. They may be usefully employed in the interests of science, by placing globules among them, since they cause these bodies to revolve in all directions, and thus favorably present themselves for observation." (p. 357.)

We find nothing further in a chemical or philosophical view, in relation to the globules of blood, but what our readers must be well acquainted with. One curious fact is mentioned, with regard to cutaneous absorption, and the transmission of substances into the circulation; namely, that "if the hands be rubbed with ether, the pulmonary exhalation will have an ethereal smell for one or two hours." (p. 386.)

The last lecture is devoted to a general summary of the objects of the course, in which the author indulges in some sarcastic reflections on the tedious discourses delivered in the modern medical schools of Paris, London, and Copenhagen, on the nature of chronic and acute diseases. He inveighs against pathology and pathologists of all classes, and often in very bad taste, (pp. 71, 196, 354;) but we think that this proceeds simply from overstrained enthusiasm in his subject. He is very fond of quarrelling with a name—witness the word *inflammation;* but he should remember that so long as practitioners understand each other, the name attached to a morbid condition of the body is a matter of little moment. We see in M. Magendie an ardent physiologist, devoted to the illustration of his subject by all kinds of experiments, many of which lead to no practical end; and the results of others, if not conflicting, appear to us misinterpreted. He is by no means sufficiently cautious to avoid those fallacies which attend every experiment on the *living* body; and he is too ready to infer from a result on the dead what must be the effect on the living. This we are not surprised at, when he seems to banish the idea of vitality from his mind, and to look upon the living body as little else than a machine, ruled almost entirely by physical laws. If his opponents have assigned too much to the influence of a vital principle, it is certain that he has assigned too little.

We give M. Magendie every credit for ingenuity and a sincere devotion to his subject; but we have felt ourselves constrained to denounce what we considered to be erroneous in his experiments and reasoning. "Reasoning alone," he observes, at the conclusion of one of his lectures "can do nothing,—I am wrong—it may embarrass and obscure everything. Experiment, on the contrary, which is nothing more than the true method of reasoning correctly, can alone extricate us from a difficulty." (p. 339.) This observation is true to a certain extent; but we must remark that the falsest theories may be based upon experiments when the results are not rightly understood or are wrongly interpreted. Correct reasoning alone can guard us against this.

We here conclude our analysis of these singular volumes, which, whatever may be their influence on the progress of physiology, certainly deserve a place among the curiosities of medical literature. After the view we have taken of their contents, it is hardly necessary to say that we do not think they are likely to add to M. Magendie's reputation; nay, we must further express our conviction that if that reputation were not so firmly and justly established by his antecedent labours, such a production as the present would inevitably be fatal to it. Science, doubtless, stands indebted to M. Magendie for the bold and distinct announcement of the truth that many of the phenomena of life are simply physical, and should be investigated by the application of physical laws; but here his merit ends. Instead of carefully laying the foundations of a new branch of science, he has reared on the sand a frail and fantastic edifice, which crumbles away even under the hands of its architect.

On a sober view of the subject, it is evident that, in order to pursue with success the study of the physical phenomena of life, we should commence by a minute and laborious investigation of the physical properties of all the textures of which living bodies are composed; the relation of these textures to all the physical agents capable of effecting changes in them; their varieties at different ages and in different orders of animals, and the diversity of their properties in life and in death. How little is known on any of these heads! If so, the study of the physical phenomena of life is yet to be begun. Its philosophical cultivation could not fail to yield the most interesting results; many long-known physical truths would be placed in new points of view, and endowed with increased applicability; many sterile tracts of science would be fertilized, and many new relations established, from which physiology and physics would derive splendid and reciprocal light.

ART. II.

1. *Lehrbuch der Arzneimittellehre.* Von Dr. C. G. MITSCHERLICH. I. Theil. 1 Abth.—*Berlin*, 1837. 8vo, pp. 126.

A Manual of Materia Medica. By Dr. C. G. MITSCHERLICH. Part I. Division 1.—*Berlin*, 1837.

2. *The Elements of the Materia Medica; comprehending the Natural History, Preparation, Properties, Composition, Effects, and Uses of Medicines.* Part I., *containing the General Action and Classification of Medicines, and the Mineral Materia Medica.* By JONATHAN PEREIRA, F.R.S., &c.—*London*, 1839. 8vo, pp. 559.

3. *Die Neuesten Entdeckungen in der Materia Medica. Für praktische Aerzte geordnet.* Von Dr. J. H. DIERBACH. 2te Ausgabe. Ier Band.—*Heidelberg und Leipzig*, 1837. 8vo, pp. 656.

The Latest Discoveries in the Materia Medica, arranged for Medical Practitioners. By Dr. J. H. DIERBACH. *2d Edition. 1st volume. Heidelberg and Leipsic*, 1837.

4. *Von dem Wirkungen der gebräuchlichen Metalle auf den menschlichen Organismus überhaupt, und als Heilmittel; und dem Kupfersalmiak Liquor, und andern Kupferpräparaten als solchen insbesondere.* Von Dr. J. R. KÖCHLIN.—*Zürich*, 1837. 8vo, pp. 186.

Of the Effects of the Metals in common use upon the Human Organism generally, and as Remedial Agents; and of the Cupro-Ammoniacal Liquor, and other preparations of Copper, as such, in particular. By Dr. J. R. KÖCHLIN.—*Zurich*, 1837.

5. *Physiologisch-therapeutische Untersuchungen über das Veratrin.* Von Dr. F. A. FORCKE.—*Hannover*, 1837. 8vo, pp. 146.

Physiological and Therapeutical Researches concerning Veratrine. By Dr. F. A. FORCKE.—*Hanover*, 1837.

1. DR. MITSCHERLICH, favorably known as a scientific physician by his ingenious researches into chemical pathology, has presented us, in the present volume, with the first part of his work on materia medica and therapeutics. It embodies the most recent doctrines of modern physiology, and must be considered, as far as it goes, a meritorious performance.

After pointing out the influence of mind and various external agencies on the animal frame, the author proceeds to the principal subject of the work.

> "The material remedies derived from the three kingdoms of nature are partly elementary combinations according to definite proportions, and consequently always uniform in composition; developing, under similar circumstances, the same phenomena in the economy, the result, as will be afterwards proved, of their chemical properties. Such, however, is not the case with a vast number of substances, the products of animal and vegetable life. It is true, indeed, that among these we still find definite elementary combination, as the alkaloids and acids, to which the same principle applies: but in others, where the proportions are not fixed, as roots, bark, wood, flowers, milk, musk, and the like, the phenomena are variable, in consequence of their chemical composition being modified by the age, influence of nutrition, light, and heat." (p. 9.)

The material remedies are divided, as usual, into solid, liquid, and aëriform. Of course, the author observes, many articles of diet play the part of medicinal agents, when it happens that a disease can be mitigated or removed by a change in the nutrition of the body.

The second section is devoted to the forms and modes of exhibiting medicines. In mentioning the pharmaceutical operations of pulverization and solution, we are told (p. 12) that "the *lac sulphuris*, an impalpable powder, acts much more powerfully than the *flores sulphuris*, which consist of fine crystals." Now we have ascertained by accurate microscopical observation the very converse to be the fact: the *lac sulphuris* consists of delicate crystals, while the *flores sulphuris* are drusy, globular masses, more or less coherent one to another. Indeed, by this same test, it will be seen that every precipitated powder is crystalline in structure.

In treating of the changes which a remedy undergoes in the living organism, the author discusses those which it suffers at the immediate point of contact, during absorption, and lastly during elimination by the different emunctories, as the kidneys, skin, lungs, &c. On this important head he admits that our knowledge is hitherto confined to very few remedies; but we are enabled to infer from analogy the effects of the rest.

> "The decomposition of remedies at the first point of contact is always in accordance with the laws of chemistry and physics, with the laws of chemical affinity to the animal textures. As far as our researches have hitherto proceeded, the same law of chemical affinity holds good for the living as for the dead organism, without being in anywise influenced by the vital principle. In another point of view we meet with very important differences, according to whether the remedy has been introduced directly into the blood (by injection into the veins) or has been applied to the surfaces of the body, the skin, the mucous membrane of the bowels, &c. In the first instance the accidental decomposition is effected by the blood, which latter is in its turn altered. The thence-resulting new combinations, whether soluble or insoluble, mingle with the blood and circulate along with it through the body. If insoluble, they lodge in the capillaries, inducing local diseases, which do not belong to the proper effect of the medicine. If we consider in the other case this decomposition, the following phenomena present themselves: When applied to the stomach, rectum, or shin, we find the decomposition referrible to the reaction in the first place, and then to the secreting organ in the second. The changes which medicines undergo in this way are of the utmost importance, although little studied heretofore. Wherever the new combination is soluble, it is capable of absorption; but not so when insoluble." (p. 49.)

On comparing those different changes, we arrive at the following facts: 1. Certain constituents of the medicine remain unchanged and undissolved, and are neither decomposed nor enter into union with the animal textures. 2. Certain substances remain equally unchanged, but are fluid or soluble in the contents of the stomach, and therefore liable to absorption. 3. Many substances are not decomposed, but enter into combination with others, and are then absorbed. 4. Certain substances suffer, in obedience to the laws of chemistry, a partial decomposition. 5. Several medicinal agents become completely decomposed, and the elementary components unite differently, so as to form more complex combinations than before. The proofs of the absorption of medicines are based on the following circumstances:

> "1. The presence of the remedy in the chyle and in the blood. 2. Its combinations with the animal solids. 3. Its separation by the excernent organs with the urine, the perspiration, the pulmonary exhalation, the milk, the saliva, &c. 4. Its disappearance from the first point of contact without being carried out of the body. 5. The supervention of a mere local effect, when the transit of the remedy into the blood by the part to which it has been applied is impossible. 6. The similarity of the phenomena in a remote part with those produced at the first point of contact. 7. The analogous effect produced by poisoned parts, and the poison itself." (p. 56.)

Several pages are occupied in establishing the truth of the above propositions, the author founding his arguments upon the experimental researches of Tiedemann, Gmelin, Wöhler, Müller, and others.

In discussing the effect of remedies in general, a clear explanation is given of the terms *action* and *reaction*, the sum of which constitutes the *effect*. Formerly, when much less was known than at present of the action of a remedy after its absorption, all the resulting symptoms were looked upon as *sympathetic phenomena*. Sympathy was, in short, the loophole through which writers on materia medica were glad of escaping, when hard pressed to explain the remote effects of remedial agents upon the system. Ignorance of sound physiology led them to this subterfuge. We are disposed to believe that the number of those agents which produce their whole effect, independently of absorption, is very small indeed. Thus hydrocyanic acid—which, applied in a concentrated form, appears to produce death more rapidly than it could be conveyed, by means of the circulation, from the capillaries to the brain, namely, in from half a minute to three minutes—has been brought forward as an instance of this kind. But even here the fact of sympathetic action is questionable; since Wedemeier ascertained that hydrocyanic acid, in a highly concentrated form, placed upon an exposed nerve, produced no general symptoms; and since we know that in from fifteen to thirty seconds the blood passes from one side of the body to the other.

Dr. Mitscherlich next goes on to describe the various channels by which medicines are introduced into the system; and then the different circumstances which tend to modify their action, as constitution, age, sex, habit, &c.

The changes effected upon the blood by medicinal substances are, according to our author, now pretty well understood.

> "It has been already demonstrated that a small number of medicinal substances pass directly into the blood, altering its qualities; and the same thing can be indirectly proved to apply to most others. It now becomes a question, how do these particles which have been imbibed act upon this fluid? Do they form with it a homogeneous whole, or do they remain distinct from it, merely flowing in the same stream to the different parts and organs of the body? To elucidate this matter it is requisite to examine the state of the globules, and then that of the serum of the blood. . . . . . The globules of blood are partially dissolved by water, acetic acid, &c.; but these latter do not reach the blood in a simple form, but more or less changed; thus the water must previously have taken up several substances present in the stomach, and the acetic acid become associated with foreign matters, so that the action of these liquids upon the globules of blood by the medium of absorption is not the same as when brought into direct contact out of the body. The investigations relative to this subject upon the serum are still very defective; yet some facts of importance have been determined. After the continued employment of alkalies and their salts the coagulability of the fibrine and albumen of the blood is diminished, that vital fluid becoming clearer in colour, and more watery than before; and the inflammatory

> crust is essentially lessened, and at length disappears. As these salts have been detected in the blood, and as they alter its properties, both in the body and out of the body, it has been concluded that they operate through the medium of absorption, and that in consequence of the brightening of the colour, the globules at the same time undergo some change." (p. 103.)

The discovery of a medicament in an excretion is no proof of an increase of that excretion, as various colouring matters, as for example rhubarb, abundantly show. "In order to ascertain the real condition of an excretion," says Dr. Mitscherlich, "we must determine its specific gravity, the relative proportions of its solid and aqueous constituents, the nature and amount of saline and organic impregnation." His enquiries upon this head have led to some interesting results; namely, that in mercurial ptyalism the solid matters, which are greatly deficient, consist chiefly of salts; and that the urine, when copiously furnished after the ingestion of alkalies, has a lower specific gravity than natural, and is also wanting in solid contents.

It is well known that certain medicinal agents act with preference, so to speak, upon particular textures; and it has likewise been observed that the operation of some remedies is more specially directed to the function of one or more organs. Bitters improve the digestion, alkaline salts, cantharides, and the like, augment the discharge of urine, and narcotics affect the sensibility of the brain and spinal marrow.

It has been commonly supposed that there exists a class of medicines capable of exercising a direct influence upon the lymphatic system, in virtue of which solid deposits occurring in the structure of an organ or part are taken up and conveyed into the current of the circulation, without undergoing any essential change. There is, however, no conclusive evidence of the fact; and, as our author remarks, "it is much more probable that the deposits in question are first of all dissolved, the medicines acting on the blood in such a way as to fit it for this purpose, after which the absorption takes place in concurrence with an augmentation of certain secretions, as, for instance, that of the urine." (p. 108.)

In investigating the general effects of remedies it will be found that these correspond with their chemical and physical properties, and that this applies with equal force to the living as to the dead organism. Life interferes with neither the chemical affinities nor physical powers of bodies. It is true, indeed, that there are many phenomena in therapeutics which cannot be explained in this way, and have, accordingly, been viewed as the sequence of some unknown, or, to use the German term, *dynamical* action.

The systematic arrangement adopted by our author is based on the physiological and therapeutic effects taken conjointly; and for these reasons:

> "On examining the effects of medicinal agents, we perceive an intimate connexion subsisting betwixt the physiological and the therapeutic; the only difference with regard to the latter is that the organism during disease is more or less modified, and the symptoms of reaction vary accordingly. In all cases where the nature and not merely the prominent symptoms of a disorder is understood, we may with more assurance reason as to the effect of a particular remedy, and frequently infer from the therapeutical effect the physiological, and conversely. On this ground we may fairly employ the physiological and therapeutical effects as points of division. When the nature of the disease eludes our apprehension, we may often discover it by

reference to the physiological effect of such remedies as have proved effectual in combating or subduing it. Indeed, the two are so closely connected that, in contriving a rational arrangement of the materia medica, both the physiological and therapeutical effect ought to be taken into account." (p. 115.)

The first class comprehends Tonics; the second, Emollients; the third, Excitants; the fourth, Acrids; the fifth, Temperants; the sixth, Solvents; the seventh, Narcotics; the eighth, Alteratives; and the ninth, *Medicamina incertæ sedis.*

We sincerely hope that the subsequent parts of this work will be exempt from that taint of Galenism which pervades, to a greater or less extent most of the German treatises on the Materia Medica.

II. We have perused, with much satisfaction, the volume of Mr. Pereira. In addition to a good deal of original matter, the author has embodied a mass of valuable information, drawn from the most authentic sources, both foreign and British; and has displayed throughout a degree of labour and research, which deserves the highest commendation.

The first hundred pages, nearly, contain the general elements of pharmacology and therapeutics, comprehending the means of ascertaining the operation of medicines, their mode of action, their physiological effects, their absorption, their therapeutical effects, the parts of the body to which they are applied, and their classification. The author then proceeds to special pharmacology, adopting the natural-historical arrangement, " believing it to be the most convenient, and, on the whole, the least objectionable," (Preface, p. 1 ;) and at the same time pointing attention to the distinguishing characters of organized and inorganized beings, the peculiarities of chemical composition, of form and structure, of actions or functions.

The different remedies belonging to the inorganic kingdom occupy the remainder of the volume. Each of these is fully and copiously treated of under the following heads : General History; Natural History; Preparation; Properties; Characteristics and Composition; Physiological Effects on Vegetables, on Animals generally, on Man; Modus Operandi; Uses; Administration; Antidotes, when poisonous. Complex chemical decompositions are elucidated by appropriate diagrams; and, wherever the subject requires it, we find ample instructions relative to the applications of toxicology.

A work like the present, being itself a condensed summary of the subjects of which it treats, is, of course, unsusceptible of analysis; in noticing it we must, therefore, content ourselves with selecting, here and there, as we turn over its well-filled pages, a few passages that attract particular attention from their importance or peculiarity, or because they seem to claim from us some elucidation or qualification. The book is neither to be analyzed by the critic nor read by the buyer : it must lie for habitual reference on the table of the study.

"Every practitioner is familiar with a stomach complaint in which pain of a spasmodic character is the leading symptom, but which is not essentially accompanied by pyrexia, as in gastritis—by tendency to faint, as in cardialgia—by indigestion, as in dyspepsia, nor by loss of appetite; though one or more of these conditions may attend it. By some nosologists (as Sauvages and Sagar) it has been regarded as a distinct disease, and has been termed *gastrodynia.* It is not unfrequently accompanied by vomiting and precordial tenderness, which, however, cannot be regarded as indicative of inflammation, for various reasons; one of which is the alleviation of it often obtained by the use of stimulants and antispasmodics. What may be the precise pathological condition of this malady, I know not. Dr. Barlow (*Cyclopedia of Practical Medicine,* Art. *Gastrodynia,*) thinks the primary disease to be irritation or excitement of the mucous membrane of the stomach, whereby a redundant, dense, membranous, and opaque mucus is secreted, which accumulates and oppresses the stomach. The pain he supposes to arise from a contractile effort of the stomach to detach and expel the offending matter; but the immediate and permanent relief sometimes obtained by the use of hydrocyanic acid, is, I conceive, almost fatal to this hypothesis. Some time since, I prescribed the acid for a lady who had suffered for months with gastrodynia, and who was persuaded from her sensations, she had some organic disease. The remedy acted in the most surprising manner; in a few hours, to the astonishment of herself and friends, she was apparently quite well, and has since had no return of her complaint. It can hardly be imagined that irritation of stomach can be rapidly removed by a substance which is itself an irritant. For my own part, I conceive the affection to be, essentially, a disordered condition of the nerves supplying the stomach, or of the nervous centres from whence those nerves are derived; and that it is frequently, but not invariably, accompanied with the irritation of stomach alluded to by Dr. Barlow. But be the proximate cause of the disease what it may, the beneficial effects of the hydrocyanic acid, in some instances of gastrodynia, are most astonishing, while in others it totally fails. In all the cases in which I have tried it, I have obtained either perfect success or complete failure; I have met with no cases of partial relief. It not only allays pain, but relieves vomiting, and in the latter cases, frequently when all other remedies fail. . . .

"I have also found it useful in a painful affection of the bowels analogous to that of the stomach, and which, therefore, might with propriety be termed *enterodynia.* The most remarkable case of this kind which I have met with, was that of a gentleman, a relative of one of my pupils. He had suffered for several months excruciating pain in the bowels, commencing daily about two o'clock, and only ceasing at night. It was apparently a consequence of an ague. He had been under the care of several country practitioners, and had tried a number of remedies (including opium and sulphate of quinia) without the least benefit. I advised the employment of the hydrocyanic acid, and accordingly five minims were administered at the commencement of a paroxysm: the remedy acted like a charm; all the unpleasant symptoms immediately disappeared. Several doses of the acid were given before the time of the succeeding paroxysm, but the disease never returned; and after employing the acid for a few days longer, he went back to the country completely cured.

"I have seen hydrocyanic acid used with great success to allay vomiting and purging in severe forms of the ordinary English cholera, when opium has completely failed. In Asiatic or malignant cholera it has occasionally appeared to be serviceable.

"As a remedy for affections of the pulmonary organs, hydrocyanic acid was at one time in great repute. It was said to be capable of curing slight inflammation of the lungs, without the necessity of bloodletting, of suspending or curing incipient phthisis, while in confirmed cases it soothed the approach of death, of curing hooping-cough, and of removing all the symptoms of spasmodic asthma. (See *Dr. Granville's Treatise,* and also *Magendie's Recherches sur l'Emploi de l'Acide Prussique,* 1819.) Experience has shown the fallacy of most of these statements. I have employed hydrocyanic acid in a considerable number of cases of phthisis, and have occasionally fancied that it relieved the cough and night-sweats; but these effects were only temporary. Cases of genuine spasmodic asthma are rare; but in two instances in which I have seen the acid employed, no relief was obtained. In allaying cough (especially the kind called spasmodic) I have on several occasions, found it useful; but it has so frequently disappointed my expectations, that I now rarely employ it in any pulmonary diseases. I have never observed any ill effects from its use in these cases, though others assert they have.

"It has been employed in affections of the nervous system. Cases of hysteria,

> epilepsy, chorea, and tetanus, have been published, in which this remedy has been found beneficial. I have seen it employed in the first three of these affections, but without any evident relief. It has been proposed and tried in hydrophobia; it apparently mitigated the symptoms. Dr. Hall (*Lect. on the Nerv. Syst.* p. 155,) proposes that, in addition to the use of this acid, tracheotomy, as suggested by Mr. Mayo, should be tried.
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> "Hydrocyanic acid has been administered as an anodyne in several painful affections, namely, cancer, tic-douloureux, rheumatism, &c., but, with a few exceptions, it has not been found serviceable." (p. 248.)

Page 261 is embellished with a small woodcut, representing the cascade of Vinagre, in Colombia, so called from its waters being impregnated with sulphuric and hydrochloric acids. The author might at the same time have mentioned the remarkable occurrence of *pure* sulphuric acid in large quantities, both in a diluted and concentrated state, in the town of Byron, Genessee county, ten miles south of the Erie canal. The place has been known in the vicinity for seventeen years, by the name of the *Sour Springs.* It has been described by Professor Eaton. (*Quart. Journ. of Science*, p. 200, 1829.)

The compounds of potassium are well described. In speaking of potash, the fact pointed out by Gay Lussac, of a great number of animal and vegetable substances when acted upon by it in the caustic form, being transformed into oxalic acid with a simultaneous production of acetic acid and water, has escaped notice. In the enumeration of the remedial uses of the *liquor potassæ*, we find the following judicious observations:

> "The alkalies have been lately celebrated for producing beneficial effects in those *inflammations* which have a disposition to terminate in exudation and adhesion; that is to say, those that frequently give rise to the formation of false membranes or of adhesion; such for example, as croup, pleurisy, and peritonitis. If experience should subsequently confirm the assertions already made respecting their efficacy, we shall have another analogy between the operation of alkalies and of mercury. Theoretically, it has been argued, the alkalies are likely to be beneficial in these diseases on two accounts; first, they have a tendency to diminish the supposed plasticity of the blood, which some have assumed (though without proof) to be connected with the exudation; and, secondly, we find these albuminous deposits readily dissolve, out of the body, in alkaline liquids: but arguments of this kind are to be received with great caution. In conclusion, I may add that Eggert recommends the alkalies as specifics against croup, though Sundelin (*Heilmittel*, 1er Bd. § 182,) found them inoperative. Hellwag employed them to cause the removal of the deposited lymph: Memminger gave them with benefit in hooping-cough; Mascagni, in pleurisy and peripneumony. (*Vogt, Lehrbuch d. Pharmakodyn*, 2er Bd. p. 529.) It is asserted that in the latter complaints, the alkalies render the expectorated matter less viscid, and at the same time, act powerfully, as diaphoretics and duretics." (p. 279.)

The following remarks upon the exhibition of lime are interesting:

> "When administered internally, it neutralizes the free acid of the gastric juice, diminishes the secretions of the gastro-intestinal membrane, and thereby occasions thirst and constipation. It frequently gives rise to uneasiness of stomach, disordered digestion, and not unfrequently to vomiting. After its absorption, it increases the secretion of urine, and diminishes the excessive formation or deposition of uric acid and the urates. With this exception, it does not, as the alkalies, promote the action of the different secreting organs; but on the other hand, diminishes it, and has been in consequence termed an astringent. But it does not possess the corrugating action of the astringent vegetables, or of many of the metallic salts: it is rather a drying remedy and might be more correctly termed a *desiccant* than astringent. In this

> respect, lime differs from the alkalies, but is analogous to the oxide of zinc. Vogt (*Pharmak.*) considers it to be intermediate between the two. Weichard and others have ascribed to lime an antispasmodic property; and if this be true, its relation to zinc is still further proved.
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> "A power of exciting and changing the mode of action of the absorbent vessels and glands has been ascribed to lime-water, and probably with foundation. At any rate, under the use of it, glandular enlargements have become softer and smaller. Sundelin (*Heilmittel*) says that the excessive use of lime does not, as in the case of the alkalies, bring about a scorbutic diathesis, but a general drying and constriction, analogous to that caused by zinc." (p. 344.)

Pages 374 to 397 contain a very excellent account of the arsenical preparations. The nascent hydrogen test of Marsh, for detecting arsenious acid, is fully explained and illustrated by appropriate engravings on wood. In pointing out the fallacy of this test, caused by antimonial impregnation, the author has omitted to indicate a ready means of obviating any fallacy whatever, derived from the property of the arseniuretted hydrogen gas of depositing its arsenic by heat. Hence, in order to render the apparatus at once sure and efficient, it is only requisite to conduct the gas, in proportion as it is disengaged, along a tube of glass kept at a red heat by the flame of a spirit-lamp. Where more precision is desired, a determinate quantity of copper reduced by hydrogen is to be placed in the ignited part of the tube. From the resulting arseniuret of a copper, characterized by its silvery whiteness, we can easily appreciate with extreme exactitude the amount of arsenic combined with hydrogen. A simple means of distinguishing arseniuretted hydrogen from antimoniuretted hydrogen has also escaped our author's notice; it consists in introducing a small portion of chlorine gas, which precipitates from the one metallic arsenic, while it leaves the other unchanged.

The precautions to be observed in administering this potent but dangerous remedy are given in detail at pages 393 and 395. A fact stated at p. 386 ought never to be forgotten by the medical practitioner, namely, "if the use of small doses of arsenious acid be continued for a long period, it acts as a slow poison; and if persevered in, will ultimately occasion death. The same effects take place, in a shorter period, from the administration of large medicinal doses. Sometimes the digestive apparatus, at other times the nervous system, first shows symptoms of the poisonous operation of this agent."

Among the antidotes in cases of poisoning by arsenic, the hydrated sesquioxide of iron has been lately brought into repute; and a very complete account of its effects will be found in a former Number of this Review.* As however its efficacy as an antidote has been denied, (*London Med. Gazette*, xv. p. 220,) we think it right to put our readers in possession of certain particulars in regard to its preparation, which MM. Bunsen and Berthold hold to be essential to its successful operation.† In order to obtain the hydrated sesquioxide in a state of purity, we are to peroxidize by nitric acid, with the aid of heat, a solution of pure sulphate of iron, then add caustic ammonia in excess, and wash the precipitate by decantation. It is important that the sulphate be quite dissolved before the nitric acid be added in small portions, otherwise there separates a considerable quantity of the neutral sulphate of sesquioxide, which falls to the bottom in the form of a yellowish powder, of very sparing solubility. The muriate of iron is ill adapted for the preparation of this oxide, because in precipitating with ammonia we run the risk of obtaining in combination a large amount of the basic chloride.

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* Vol. IV. p. 237. † Archiv. der Pharm., Bd. xi. Cab. iii. p. 323.

In order not to remove the water from the precipitated hydrate of sesquioxide, so as to diminish in the slightest its feeble state of aggregation, it should not be filtered; but after settling for some hours the supernatant liquor is to be decanted, and the residuum preserved under water in well-closed vessels.

In alluding to the diseases in which nitrate of silver has been given internally with advantage, such as epilepsy, chorea, angina pectoris, chronic affections of the stomach, &c., Mr. Pereira says (p. 430) "its *methodus medendi* is inexplicable." We are disposed to concur with the ingenious explanation suggested by Dr. Osborne, that in all the above cases nitrate of silver allays irritation of the gastric membrane, and does good primarily in that way. (*Dublin Journal of Med. Science*, Jan. 1839.)

Mercury and its compounds are discussed at great length and in a very masterly manner. The author's summary of their morbid effects is admirable. (pp. 441-3.) In reference to Dr. Wilson Philip's notion, that "mercury has a specific operation on the liver, a power not merely of exciting its functions, but of correcting the various derangements of that function in a way which it does not possess with respect to any other organ, and which no other medicine possesses with respect to the liver;"* he very justly confesses his ignorance of any facts warranting the assertion. (p. 445.)

At page 461 binoxide of mercury is declared to be "insoluble in water," whether procured by calcination or precipitation. It appears, however, from some recent experiments of M. Bondet, recorded in a late Number of the Journal de Pharmacie, that perfectly pure binoxide of mercury is slightly soluble in distilled water.

In indicating the different substances which decompose corrosive sublimate, the author might have annexed the mucilage of quince-seed and that of salop, which act instantaneously; a fact first noted by M. Fabian, (see *Quart. Journ. of Science for* 1830, p. 198.)

In the history of zinc (p. 521) it is stated that, "although the ancients were acquainted with the method of converting copper into brass by means of an ore of zinc, yet they were unacquainted with metallic zinc, one of the constituents of this alloy. Albertus Magnus, who died in 1280, is the first who expressly mentions this metal." But a passage in Strabo authorizes the belief that they also knew this metal in its separate state. The geographer says† that near Andeira, a town of Troas, is found a stone, which being burnt becomes iron; and distils false silver, (ἀποσταζει ψευδάργυρον,) when heated together with a certain earth, which, receiving the addition of copper, forms the alloy that some call brass (ὀρείχαλκον). He adds, respecting this *false silver*, which was probably our zinc, that it occurs also near the Tmolus. Stephanus states the same thing in somewhat clearer words, and refers to both Theopompus and Strabo as authorities.‡

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* On the Influence of Minute Doses of Mercury, p. 14.

† Strabo, p. 610.

‡ Steph. de Urbibus, word Andeira.

In treating of the salts of zinc, it is observed of the acetate that "its effects are analogous to, though milder than the sulphate of zinc, but more energetic than the oxide." (p. 529.) Its dose, when exhibited internally as a tonic or antispasmodic, is one or two grains gradually increased. As an emetic, the dose is five or ten grains. Whereas the dose of the sulphate, as an emetic, is stated to be from ten to twenty grains; as a *tonic*, *antispasmodic*, or *expectorant*, from one to five grains. Does not this involve a posological contradiction?

Mr. Pereira constantly uses the term hydrosulphuric acid as synonymous with sulphuretted hydrogen. This we conceive objectionable, inasmuch as hydrosulphuric acid signified in Sir H. Davy's nomenclature hydrous sulphuric acid.

In concluding our notice of this the first part of Mr. Pereira's work, we have to express our thorough conviction of its intrinsic worth and usefulness. It contains a most clear and comprehensive exposition of the principles and practice of therapeutics; and, when complete, will constitute a very valuable addition to the literature of the materia medica. Perhaps, the only objectionable part are the woodcuts, many being commonplace, and others ill adapted to convey a distinct notion of the process they are intended to illustrate. The whole style of the engraving is, indeed, below the level of the present condition of the art, and calls for improvement in the next edition of the book.

III. As the more important parts of Dr. Dierbach's compilation have been embodied in the work of Mr. Pereira, we shall content ourselves with furnishing our readers with a brief analysis. Pages 1-49 contain the modern literature of the materia medica under the following heads: the knowledge of drugs, medical mineralogy, medical botany, medical zoology, dietetics, the doctrine of the effects of remedies, toxicology, the art of prescribing, pharmacopœias, taxation of drugs. The next eleven sections (pp. 50-648) are occupied in considering remedies individually. I. Plants or parts of plants which have been recommended within the last few years, (pp. 50-225, secs. 1-102:) those growing in Europe, wild or cultivated, occupy secs. 1-54: whilst the exotic vegetable productions occupy secs. 55-102. II. New preparations of vegetable substances, (pp. 225-312, secs. 103-146:) and 1, Mild nutritious, or tonic excitant articles, for the most part bitter, secs. 103-121; 2, Articles more or less acrid, determining occasionally violent vomiting or purging, secs. 122-131; 3, Potent, chiefly narcotic drugs, secs. 132-142; 4, New remedies allied to the acids, secs. 143-146. III. New preparations procured by the combustion or the dry distillation of organic substances, (pp. 313-34 . secs. 147-156.) IV. Various animal products, and the resulting preparations, (pp. 343-358, secs. 157-166.) V. Hydrocyanic acid and other compounds of cyanogen (pp. 358-388, secs. 197-176.) VI. Chlorine, iodine, bromine, and their respective preparations, (pp. 388-463, secs. 177-205.) VII. Sulphur and sulphuretted remedies, (pp. 463-472, secs. 206-210.) VIII. Salts, soap, metallic preparations, (pp. 473-542, secs. 211-236.) IX. New methods of employing certain gases, (pp. 543-555, secs. 237-243.) X. Oil of grain and hydrogenated oil of grain, (Germ. *Fermentoyl*,) (pp. 555-55 sec. 244.) XI. Pharmacological miscellanies, (pp. 558-648, secs. 245-253.) At the beginning of the work there is an index of the remedies according to their therapeutic effects. Dr. Dierbach, actuated by the true spirit of German industry, may be said to have supplied a register of all the most recent discoveries in the materia medica, which cannot fail to be of use to the medical practitioner and student of pharmacy; containing, as it does, a full and copious account of the nature and properties of the several articles of the diseases in which they have been found useful, of their doses and forms of employment.

IV. Dr. Forcke's lucubrations will not detain us long. They are evidently those of a tyro, dazzled by accounts of the virtues of a remedy, which subsequent experience has proved to be in many instances exaggerated, in others false. The following is his account of the effects of veratria upon the human body in the healthy and morbid conditions.

> "After a person has taken, for two or three times, from one sixth to one fourth of a grain of veratria, frequently, indeed, in from half an hour to an hour after the first dose, there arises a feeling of tingling, sparkling, prickling, (gefühl von prickeln, funkeln, oder pinkeln,) in parts remote from the stomach, most commonly in the points of the fingers and toes, sometimes in the elbows, bends of the knees, and shoulders, occasionally on the forehead or over the eyebrows, more rarely and at a longer interval in the thighs, abdomen, or back. Simultaneous with this, or soon after, some patients experience a sense of warmth, others of cold referred to different regions of the extremities and trunk, mostly in the hands and soles of the feet, the knees, and mouth. While one has the feeling as if a stream of warm air or drops of hot water were issuing from these parts, another perceives, as it were, a frozen atmosphere around the feet and particularly the knees, or as if cold water was poured upon them. Some compare the sensation in the mouth to that produced by sucking peppermint. In general, the warmth occurs where there is integrity of the vital powers but abdominal torpor, while old hypochondriacs and hysterical women, with predominant asthenia, experience the cold. . . . . Besides the above sensations, there is occasionally superinduced this peculiar phenomenon, namely, that a pain, which may have been of long standing in some part of the body, either suddenly disappears altogether, or else is replaced by another, equally sudden in its invasion, in some distant part. Examples are not wanting of a limb or of the muscles of the face, especially if previously the subject of painful or spasmodic paroxysms, becoming seized with starting and tremor soon after the taking of the medicine. Thus, in from half an hour to an hour after the dose had been swallowed, parts began to start, which were in a half paralyzed state from apoplexy, or which used to present violent spasms or trembling during severe paroxysms of *tic douloureux*. In some cases the development of heat was very marked. In the case of a torpid lymphatic subject, in whom, from "abdominal epilepsy," the left arm had got into a half-palsied state, attended with frequent twitchings, a fleeting but sensible warmth succeeded the application of the veratrine, which had never been noticed at any other time. A few doses sufficed to restore permanent warmth to the feet of a feeble, emaciated, melancholy female, which for years before had remained of an icy coldness. This seems to show that the remedy acts powerfully upon the organic nerves." (pp. 20-2.)

The author is of opinion that all the cases of facial neuralgia which have yielded to the veratria, have been merely dependent on sensory and functional disorders of the nerves. (p. 46.) On this point he has our full concurrence. After adducing a few instances where benefit seems to have been derived from the employment of this alkaloid in neuralgia, partial paralysis, and diseased heart; others are brought forward of hooping-cough,* catalepsy, epilepsy, eclyse (fainting), hypochondriasis, hysteria, dropsy, &c., in which the results are by no means satisfactory.

V. Dr. Köchlin, a follower of Paracelsus, and a humoral pathologist, has favoured the profession with a treatise on the healing virtues of metals. His doses are minute, for without being a homœopath, he restricts them to the 100th of a grain. After some preliminary observations on sensibility, irritability, and the like, the author proceeds to discuss the action of metallic substances upon the organism. This, in some instances, is that of expansion, in others that of contraction. He informs us that some remedies are *mediate*, others *immediate* in their effects. Thus tannin and heat illustrate the latter, while whatever changes the *crasis* and *plasticity* of the blood pertain to the former. Iron and mercury serve as instances; the first augmenting, and the second diminishing the *plasticity* of that fluid; but they are not the only metals endued with such powers. He next passes on to the liquefacient (*verflüssigenden*) resolvent, and then to the antiphlogistic action of metallic substances; and repudiates the notion entertained by some authors of there being a specific qualitative and a specific local action.

The different external influences and conditions which concur towards the preservation of life, and have to be taken into account in the employment of remedies, are fully noticed. As peculiarities, we may remark, that the author lays great stress on the necessity and importance of selecting the preparation best suited for accomplishing the end in view, considering as most efficient the union of the metallic hydrochlorates with sal-ammoniac, and urges the propriety of the medicine being swallowed directly after taking food. In the subsequent part of the book he treats, 1, of the action of metals upon the organism generally; 2, of their therapeutic agency; and 3, of the virtues of the cupro-ammoniacal solution, dignified by the title of the *Liquor of Köchlin*, and other cupreous preparations in particular. A series of extracts culled from German journals, and letters from German doctors, forming an appendix of twenty-eight pages, attest the wonderful powers of the pet liquor. Vogt, in his Pharmakodynamik (Bd. i. p. 320), informs us, that Köchlin having learned that a Dr. Beisser in the East Indies possessed a nostrum of a cupreous nature, which proved remedial in certain instances of syphilis, where mecurials failed, proposed his liquor as a substitute. The same eminent writer states that it has answered in the hands of other practitioners, and is highly serviceable in inveterate atonic syphilis, in old eroding scrofula and rachitic ulcers, in the so-called *dyscrasia ulcerosa*, in cancerous sores, and *esthiomenous* affections of the skin. Kopp recommends its use in certain abdominal disorders, in *infarctus* and *physconia*, in incipient mesenteric disease, and especially in pyrosis with cramp of the stomach. It does not, like other cupreous preparations of equivalent strength, produce gastric derangement. The solution is prepared as follows: take of copper-filings one drachm, of aqueous ammonia an ounce and a half, digest in the cold, until the liquid assume a blue colour; to an ounce of this add two drachms and forty-two drops of muriatic acid, and dilute the resulting combination with six pounds and a half and two drachms of distilled water. The dose for a child is a teaspoonful.

We deem it right to state that we consider the above formula to be faulty, inasmuch as the proportion of copper held in solution will vary according to circumstances. The blue tint depends wholly upon the absorption of oxygen; caustic ammonia may be kept in contact with copper-filings for an indefinite period without changing colour, provided the atmospheric air be excluded. It is only by a process of alternate coloration and decoloration, that is to say, by an alternate reproduction of the protoxide and dioxide that the ammonia becomes fully saturated.

Art. III.

Die geburtshülfliche Auscultation. Von Dr. Herm. Franz Naegele. —*Mainz,* 1838. 8vo, pp. 140.

Obstetric Auscultation. By Herman Francis Naegele, m.d. — *Mayence,* 1838.

Although we have, on more than one occasion, discussed the merits of obstretric auscultation, particularly in reviewing the works of M. Hohl, (vol. I. p. 85,) still we have not had until now an opportunity of entering exclusively upon the consideration of this important subject; having, however, recently obtained the little work above mentioned, we lose no time in earnestly requesting the attention of our readers to it. The author is the son of a distinguished professor of that name at Heidelberg, who has long been honorably known as one of the first obstetric teachers upon the continent. Although a very young man, Dr. N. has already distinguished himself as the author of some interesting papers in the German journals, one of which we have ourselves recently noticed; and he has been for some years closely occupied in collecting materials for the present undertaking. The work itself is purely practical; and in his preface the author refers his readers to the elaborate work of Professor Hohl, for the historical part of obstetric auscultation.

We need scarcely observe that Dr. Naegele prefers the stethoscope to using the ear alone; the instrument which he uses (as far as we can judge from his description of it) does not appear to differ materially from those commonly used in this country, except that it is somewhat thicker than usual; in giving his directions how to apply and use it, he strongly deprecates (with Laennec) stooping with the head downwards over the patient, as such a position must necessarily produce much congestion of the head, and so much buzzing in the ears as materially to interfere in the diagnosis of those sounds which are faint and but indistinctly heard. For the same reason he advises the operator not to auscult for many minutes at a time, without removing his ear from the disc of the instrument; for otherwise the pressure will more or less obstruct the free circulation of blood in the external ear, and thus produce souffle-like sounds, which may tend to mislead him.

In describing the situation of the uterine souffle, Dr. N. observes that it may always be heard in one or both inguinal regions.

> "From this part it usually extends, although mostly on one side, towards the hypochondrium, or more forwards towards the umbilicus; and the limits within which it can be heard cannot be marked very exactly. In the majority of cases they at any rate correspond to the circumference of the placenta; in some it extends from both groins over the whole uterus; in others it is more confined to the lower portions—there is no part of the uterus which can be reached by the stethoscope in which we have not heard it." (p. 17.)

Dr. Naegele considers that the uterine souffle may be detected for the first time as early as the beginning of the fourth month. He distinguished it in 20 cases out of 35 at the fifteenth week; and in three cases only at the fourteenth week. This agrees with professor Hohl's observation; but we have already stated that Dr. Evory Kennedy, the present master of the Dublin Lying-in Hospital, has detected it at the twelfth, eleventh, and perhaps even tenth week of pregnancy—a fact of which Dr. N. does not seem aware. "In the early periods of pregnancy the uterine sound is more feeble; it extends over the whole uterus, and is heard most distinctly immediately over the symphisis pubis." (p. 18.) He warns the beginner not to press the stethoscope deeply into the abdominal integuments, as this will only hurt the patient and not assist him in his diagnosis.

The author differs from Professor Hohl, respecting the changes of tone which the uterine sound undergoes, upon the accession of a labour pain; we must confess that the description which Professor H. has given of them (see *Br. and For. Med. Rev.* vol. I. p. 93,) accords with our own experience; it is not very easy to make an accurate auscultation during a pain, for reasons which are self-evident; still, however, the peculiar piping character of the uterine sound at these moments cannot be mistaken, and may be heard best in the inguinal region; the difficulty of ausculting the fundus during a pain is perhaps the reason why we were not aware of the interesting fact which Dr. N. has mentioned, viz. that at the moment of greatest contraction the uterine sound generally ceases entirely over the fundus and body of the organ, although, as he also observes, it continues audible in the groins.

That the uterine sound generally ceases at the moment of the placenta being detached from the uterus is a fact which has been long since pointed out by Dr. Kennedy, in his excellent little work on this subject; we regret that the auscultation of the uterus during the first forty-eight hours after labour has not been made the subject of enquiry by our author, as we feel sure he would have noticed the souffle which has been described by Dr. Kennedy and confirmed by ourselves. (*Ib.*, vol. I. p. 91.)

The uterus does not long maintain that state of firm contraction in which it is usually found immediately after labour, and which must be looked upon as the reason why the souffle ceases to be heard at this moment, as also in the upper portions of the organ during the height of a pain; in the course of twenty minutes or half an hour the contraction gradually abates in power, the uterus increases in size, its vessels are again filled, and this state usually continues until about forty-eight hours have elapsed, when the uterus again begins to contract and to diminish in size, until it is not much larger than it was in the unimpregnated state. It is during this condition of slight relaxation that the lochia make their appearance; and, as far as we have the opportunity of observing, the uterine souffle after labour is chiefly confined to this period.

It is well known that the bruit which is heard in certain conditions of the heart and large vessels occasionally bears a strong resemblance to the uterine sound or souffle; this is most remarkably the case, as Dr. N. observes, in aneurismal varix, where a direct communication for the blood exists from an artery into a vein. "Dubois compares the uterine parietes, as respects their structure, with a tissue which consists of a natural varicose aneurism. Hohl fully agrees with him on this point, and carries it even further, since he considers the existence of the placenta uterina fully proved, and that the passage of the arteries into veins at this part is not effected by means of anastomosis, but rather by large cells." (p. 25.) We cannot imagine it possible for any one with an impartial mind to read and repeat the admirable observations of Hunter on this subject without coming to a similar conclusion. We consider that the facts connected with the placental circulation are as distinctly and satisfactorily established as any point in anatomy or physiology can be; we cannot, therefore, agree with our author, when he says that the vascular connexion between the uterus and placenta is far from being determined. "The souffle which is so uniformly heard in the inferior portions of the uterus arises, in our opinion, from the uterine arteries before they enter the uterus; and we observe that these vessels, as soon as they reach the broad ligament, assume a different character, become thicker than where they branched off from the main trunk, and, after having made numerous windings, sink into the substance of the uterus." (p. 28.) Dr. N. considers that the tortuosity of these vessels, their dilated state, and the thinness of their parietes, sufficiently account for the uterine souffle; we know, however, that these vessels are not so remarkable for their tortuous course in a first pregnancy as afterwards; and therefore must take this opinion with some degree of caution.

In remarking the variable character of the fœtal pulse and the sudden alterations to which it is subject, the author has very properly examined the rate, &c. of the heart shortly after birth, and finds that it presents the same sudden changes as in the heart of the fœtus before birth. "How variable is the pulse of a child, even in the first years of its life: to-day it is 80—to-morrow, without any assignable reason, we find it up to 160 or 180. The pulse varies with extraordinary rapidity, not only whilst it is awake, but in apparently placid sleep—how rapidly it rises when the child moves, takes nourishment, or is subject to any mental excitement! we see it suddenly to rise 30, 40, or even 60 beats in a minute, changes which must tend greatly to confuse the practitioner who is not familiar with them, and show why the most eminent physicians place so little value on the pulse in diseases of children." (p. 37.)

We have already shown, in a quotation from Professor Kilian's work, (*Rev.*, vol. I. p. 90,) that nothing which accelerates the pulse of the mother affects that of the child; the author of the treatise before us has repeatedly examined females immediately after violent exercise, when the action of the mother's heart was greatly increased, and beating at the rate of 90, 100, or even 120 strokes in a minute, and yet the action of the fœtal heart was neither increased in rapidity or power. In cases, also, where this effect had been produced by drinking ardent spirits, the fœtal heart remained unaffected. In cases of slight catarrhal fever, and even of severe pleuritis and pneumonia, the fœtal heart was found beating at the normal rate: these and other equally well-marked cases, especially where the dyspnœa was very severe, show that such circumstances do not affect the fœtal heart, as supposed by Professor Hohl. One of the author's cases is of considerable interest, where, even in a state of really dangerous syncope, the fœtal heart was heard beating with its usual strength and rapidity; he has observed the same fact in cases of hemorrhage.

The earliest period at which Dr. Naegele has detected the sound of the fœtal heart has been the eighteenth week of pregnancy: the cases in which it was heard thus early were primiparæ. "Before the first half of pregnancy had elapsed we have been enabled to recognize the sound of the heart in 30 cases out of 50. Although our attempts were frequently unsuccessful before the twentieth week, yet when the second half of pregnancy had commenced, we usually succeeded; now and then the sound of the fœtal heart did not become audible until the sixth month." (p. 48.) This agrees pretty much with the observations of Dr. Kennedy, who says, "although we have in a few cases detected this sound, even before the expiration of the fourth month, it will not in the majority be possible until a later period; and in those cases where it can be detected about this time, it is sometimes so delicate and feeble as to render it necessary for the individual exploring to have an ear well trained to stethoscopic sounds." (p. 101.) In fact, the discrepancy as to the earliest moment of its becoming audible is scarcely appreciable, if we bear in mind that Dr. K. has reckoned by calendar months, whereas the German authors do it either by weeks or lunar months, and hence consider that the duration of pregnancy is ten months, or forty weeks.

In examining the sound of the fœtal heart during a labour pain, Dr. Naegele has found it to become indistinct or quite inaudible, in proportion to the strength and energy of the contraction. He considers that this results from the sound which is produced by the uterine fibres and abdominal muscles, when contracting. It is well known that a peculiar grinding sound, if we may thus describe it, is heard when the stethoscope is placed upon a muscle in action; and Dr. N. compares the sound which now overpowers that of the fœtal heart to that which is produced by the masseter muscle during mastication; this may possibly be the case, but we confess that the difficulty of keeping the patient either still or quiet at these moments is such as to be quite sufficient to render such slight sounds inaudible.

The author has very properly noticed the fact of the sound of the fœtal heart becoming more distinct when the membranes have ruptured, and a portion of the liquor amnii come away; but we find no mention of a fact which attends this change, and which ought not to be omitted, viz. that now the sound, although more distinct, is heard over a smaller extent; whereas, when there is a stratum of liquor amnii between the thorax of the fœtus and the uterus, the sound, although less clear, is diffused over a greater extent. "If in cases where the head presents, the fœtal heart has been heard in one of the superior abdominal regions, previous to the discharge of the liquor amnii, it will continue to descend during the latter part of labour, so that in proportion as it becomes inaudible in the superior abdominal region, it becomes distinct in the inferior one. At this latter part the heart may be heard distinctly until the head has completely entered the vagina: the moment this is born all traces of the fœtal heart above the pubes cease."

In describing the sound produced by the circulation in the umbilical cord, we regret that the author has made no mention of Dr. Evory Kennedy's highly interesting observations on this subject. These were published as early as 1833, and contain much original matter. Dr. K. has also remarked the peculiar souffle of the umbilical cord, and rightly ascribes it to pressure, having succeeded in producing it at pleasure. This souffle of the cord is chiefly heard when a portion of it is pressed between a limb or the back of the child; it does not require to be coiled or twisted round any part to produce this effect, as Dr. N. has rightly observed; but this was shown long ago by Dr. Kennedy, who succeeded in *feeling* the cord, as it passed across the child beneath the thin uterine and abdominal parietes of the mother; he states it as being easily distinguished, prominent, rolling under the finger, and pulsating." (p. 124.) In the same case to which we have now alluded, Dr. K. made an experiment, which is too interesting to pass unnoticed. "Having fixed the funis against the limb of the child, between the finger and thumb of the left hand, I made a gentle pressure with the fore-finger of the right hand on the cord, keeping my ear applied to the stethoscope, the other end of which was over the funis, at a point nearer its insertion into the placenta. The pulsation which, up to the moment of my making this pressure, was remarkably strong and distinct, became converted into a souffle; and on increasing the pressure it immediately ceased, recommencing the moment I discontinued it. I then removed the stethoscope to the spot where I had discovered the heart's pulsation, and repeated the experiment as above. The action of the heart at first became laboured, but fuller; afterwards it became fluttering and indistinct; and not judging it safe to continue the pressure any longer, lest the child should suffer, I removed it, when the action became regular as before, but somewhat quicker." (p. 125.) We cannot help thinking that the facts related in the above quotation prove the existence and nature of the funic souffle far more distinctly than the various reasons which Dr. Naegele has given; at the same time we must in justice say that the care with which he has made these observations reflects much credit upon him; and, although we have no doubt whatever of their being original as far as he is concerned, still, nevertheless, the merit of priority must rest with Dr. Kennedy.

In speaking of the stethoscope, as connected with the diagnosis of pregnancy, Dr. Naegele mentions a sound which, as far as we know, has never been made available for this purpose: we allude to that produced by the movements of the fœtus. "Among these signs (of pregnancy) we must enumerate the sound produced by the plunging movements of the child's limbs, which we are frequently able to hear much earlier than they can be felt by the hand of the practitioner, when applied upon the patient's abdomen, or indeed by the patient herself. Although we are inclined to look upon the audible movement of the child's limbs as one of the earliest certain signs of pregnancy, this symptom does not present itself in every case or at the same period." (p. 62.) The actual percussions which are occasionally transmitted to the ear when auscultating the gravid uterus during the last weeks of pregnancy, must be familiar to every one; but we are not aware of their having been noticed at an early period, as described by Dr. Naegele, who has, to the best of our knowledge, the merit of having first called the attention of the profession to this fact.

Much as our author respects the stethoscope, as a valuable addition to our means of diagnosis, still, however, he is very far from looking upon it as a substitute for manual exploration.

> "We are thoroughly convinced that this latter mode of examination will never be rendered unnecessary, and that auscultation will never succeed in taking the place of examination per tactum. Auscultation is an important addition to the common method of examining, and the combination of these two means of diagnosis enables us to draw much more distinct conclusions from their mutual results; in illustration of this, let us suppose the following cases: for instance, we have reason to suspect pregnancy, but we can hear no sound of the fœtal heart, the fœtus may be dead; if we hear the uterine sound in this case, and other symptoms speak in favour of pregnancy (such as feeling a portion of the child presenting), we are led to the conclusion that the patient is pregnant but that the child is dead. If, on the other hand, in a case of doubtful pregnancy, we hear no sound whatever, after the most careful auscultation, and cannot detect any of the usual symptoms to be found upon manual exploration, we shall be enabled to decide, with so much more certainty, that our patient is not pregnant. In short each of these modes of examination offers its peculiar advantages, the greatest, however, are when they are combined, where they mutually act as checks upon each other." (p. 64.)

With regard to the diagnosis of pregnancy where there are twins, Dr. Naegele considers that the assertion of the uterine sound being much louder and more extended, especially in two different portions of the uterus, from the presence of two placenta, is not borne out by experience. He is inclined to think that the presence of twins in the uterus can only be ascertained by hearing the sound of the two fœtal hearts. "Whatever may be the position of the children, the pulsations will never be heard at the same horizontal line of the mother's abdomen. This difference, with regard to the spot at which the pulsations are heard most distinctly, is well worthy of notice, because the pulsations of the two fœtal hearts are frequently perfectly synchronous." (p. 67.) In a case of twins which the author auscultated, he observes, "we perceived distinctly the pulsation of a fœtal heart in the left inferior abdominal region, showing that the back of a fœtus was in this direction; in the right inferior abdominal region we could hear nothing, whereas in the right upper abdominal region (near the hypochondrium), the pulsation of another heart was audible, the distinctness of which plainly showed that the organ itself was at no great distance: in the left upper abdominal region, on the other hand, we could detect nothing of the sort, both hearts corresponded exactly with each other in point of rhythm." (p. 68.) This isochronism of the two fœtal hearts is by no means always the case; generally speaking, there is a considerable difference between them, both in rhythm and strength of pulsation. These points cannot be ascertained by one person, the two fœtal pulses must be ausculted at the same moment by two observers, and thus, however small the difference may be between them, it will be correctly determined.

With respect to the sounds which are heard in cases of extra-uterine pregnancy, Dr. N. has not been fortunate enough to meet with a case of this rare description; in a case of ventral pregnancy which we have had the opportunity of ausculting, and where the child was distinctly placed across the lower part of the abdomen, the bruit over the whole lower and anterior half was so extravagantly loud (if we might so express ourselves), as instantly to strike even the most inexperienced in the art of auscultation; we ourselves had not the opportunity of hearing the fœtal pulse, the child having evidently died about twenty-four hours before we saw the patient for the first time; the sound, however, had been recognized by a gentleman who had ausculted the abdomen, and, as far as we could ascertain, in nowise differed from that of the fœtal heart under ordinary circumstances. From this period the loud souffle became day after day more circumscribed, until when we last ausculted her (an interval of four or five weeks), it was confined to a small space and was very feeble. In speaking of the various species of deception to which auscultation of the abdomen may give rise, Dr. Naegele has observed that in some cases the sound of the mother's heart may be heard over a remarkable extent, even as far as the ossa ilia. He attributes this "to its being transmitted along the intestinal tube when filled with gas; the same effect may be produced where the abdomen has been distended by some morbid growth, and where other deceptive symptoms, as cessation of the catamenia, swelling of the breasts, &c. may lead to the supposition of pregnancy; if we apply the stethoscope in such a case, we hear a pulsation, and if the circulation of the patient be quicker than usual, it is considered to proceed from a fœtus in utero, and thus the case is pronounced to be pregnancy." (p. 71.) He has quoted a case, related by Dubois, so remarkable, that we cannot help giving a brief account of it.

> "A young woman, in whom the menses had ceased for five months and a half, applied for admission at the Maternité under the supposition that she was pregnant; the size of the abdomen corresponded with the date of the cessation of her menses, and she assured him that she felt the child move. In about a month after, Dubois ausculted her, and found a double pulsation, varying from 128 to 130 strokes, at the lower part of the abdomen on the left side. Happening shortly afterwards to feel her pulse, he was astonished to find it beating at precisely the same rate. On repeating his auscultation he discovered that these double pulsations became more and more distinct as he approached the epigastrium, so that it was impossible not to recognize their real source; the sound of the patient's heart extended over the whole abdomen, and at its lower portion was so feeble as to be easily mistaken for the sound of a fœtal heart; upon making a careful examination, per vaginam, it proved that she was not pregnant." (pp. 72-3.)

In determining the situation of the placenta, Dr. Naegele considers that the souffle, which is commonly heard in both groins, is more distinct and over a larger space on that side at which it is attached; in ten cases of adherent placenta, the introduction of the hand has confirmed this observation. The industry and zeal which the author has displayed in determining these interesting points, are not less creditable to him than the talent by which they have been directed. The result of his observations shows a curious discrepancy from what had hitherto been considered as an established fact, respecting which side of the uterus the placenta is most frequently attached to, viz., that it is the left and not the right side, as commonly supposed. In 600 cases which he carefully ausculted, "the placenta was found in 238 cases on the left side and in 141 cases on the right side; of the remaining ones, no uterine sound was heard at all in twenty cases, and in 160 others it was too weak or too much limited to the inguinal regions to give any result; in seven only was the placenta distinctly in the fundus uteri." (p. 85.)

The author has noticed a peculiar piping, hissing character in the uterine sound in cases where the placenta was situated in the vicinity of the os uteri; he also confirms the observation of M. Hohl, viz., that this sound is met with where there is much of those calcareous depositions which are occasionally found in the placenta.

In determining whether the child be alive or not, Dr. N. considers that the stethoscope affords us the only *certain* means of forming a correct diagnosis.

> "On detecting the sound of the fœtal heart, we can unhesitatingly decide that the child is alive, and we coincide most strongly with the assertion of Professor Hohl, in declaring ourselves opposed to the opinion of those authors who will not allow our not hearing the fœtal heart to be a proof of the child's death. If the practised auscultator be unable to detect any trace of the fœtal heart at a period of pregnancy, when it is wont to be heard with distinctness, not even by repeated careful examinations of the patient in different positions, provided the presence of pregnancy be satisfactorily established, there can be no doubt but that the fœtus is dead." (p. 91.)

We cannot altogether agree with Dr. Naegele in denying the correctness of Professor Hohl's remark, that the uterine souffle ceases when the fœtus dies; this sound continues for some time after the child's death in utero, and betrays little or no perceptible change in its strength or clearness; as, however, expulsion is seldom retarded beyond a few weeks after this occurrence, we have not often had an opportunity of determining the fact. The author is certainly right if Professor Hohl's observation refers to the auscultation of the uterus immediately after the child's death; but we have strong reason to doubt whether the uterine souffle will retain its full degree when three or four weeks have elapsed; indeed, his own observations appear to confirm this opinion, for in the case to which he refers in illustration (p. 95), he says, "the uterine sound was heard loudly in both inguinal regions, and extended from thence, *although with less power*, over the whole abdomen."

Dr. N.'s observations on the effects which prolonged uterine action will produce on the circulation of the child, although doubtless well known to experienced practitioners, are not the less worthy of notice.

> "The life of the fœtus is greatly endangered when the progress of the labour, after the escape of the liquor amnii, is unnaturally protracted, and the uterus eventually remains in a hard, contracted state, whether this arises from faulty position of the child, or want of proportion between it and the passages; the closer the uterus contracts upon the child's body, the more is its circulation obstructed, and at length stopped altogether." (p. 99.) . . . In a considerable number of cases where the pains have been remarkably long and powerful, and where, after the escape of liquor amnii, one contraction of the uterus followed another so quickly, that the patient had scarcely time to get her breath, we have observed the pulse of the fœtal heart become slower and weaker; its rapidity has diminished from thirty to forty and even more beats, and yet the child has been born alive and perfectly strong and healthy; in such cases it was only able to resist the violence of the uterus during the short period that the labour lasted." (p. 102.)

In illustration of this interesting point we will merely quote a passage from the case which follows:

> "The head descended in the first position into the pelvic cavity; during this storm of uterine contractions, the stethoscope was constantly applied, and at every brief interval of quiet, the fœtal heart was counted and gave the following result: the

pulsation became every minute slower and in the following ratio, 140, 134, 136, 128, 130, 122, 120, 116, 100, 108, 100, 94, 86, 80, and, as the head was passing over the perineum, 75. It was now expected that an extremely feeble child would be born which could not live; but the result showed otherwise: when the contractions had continued for nearly three quarters of an hour, the child was expelled with a single pain; the funis pulsated very slow and feebly; but as soon as the child had made some efforts to breathe, the heart began to beat with strength, and shortly afterwards the child cried stoutly: the pulse was now 120 in the minute; in an hour after, during sleep, it was 110; the next day, during sleep, it was 126; and when the child was awake, 136." (p. 103.)

The author has given some interesting observations on the effects which partial detachment of the placenta, before labour, have on the uterine souffle; this gradually diminishes, and at length ceases entirely; at the same time the fœtal heart beats more slowly, and even sinks below the mother's pulse in point of frequency, it then makes several violent struggles and the child dies. If the head be pushed somewhat to one side, or slightly raised by the finger, a small quantity of dark blood will escape. For further particulars on this interesting subject, as also for two very instructive cases of prolapsus of the cord, we must refer our readers to the work itself. Dr. N. has called our attention to a remark made by Levret, respecting the situation of the placenta, viz., that the nearer this mass is attached to the os uteri, the nearer is the insertion of the cord to the lower edge of it. Dr. N., from having observed these circumstances in the above-mentioned cases of prolapsed funis, rather prematurely in our mind, deduces the following conclusion, viz., "the attachment of the placenta, in the vicinity of the os uteri, is undoubtedly one of the predisposing causes of prolapsus of the funis during labour; and this circumstance is the more to be dreaded, the nearer the insertion of the cord is to the lower edge of the placenta." (p. 114.) We must differ here most strongly from him. In all the cases of partial situation of the placenta over the os uteri, which we have had the opportunity of seeing (and they have been pretty numerous), especially where *merely* the edge of the placenta covered that of the os uteri, we have never met with prolapsus of the cord, nor do we recollect to have noticed the marginal insertion of it into the placenta, as above mentioned, in such cases; nor can we call to mind a single case of prolapsed cord which we could attribute to this cause, but rather to unusual quantity of liquor amnii, to want of regular contraction in the lower segment of the uterus, to death of the fœtus, &c. Dr. N. has tried Michaelis's method of replacing the funis without success, and finds that using the hand alone is preferable. We have already noticed Dr. Michaelis's observations in our Second Number, page 588, also in Number VI. page 523, to which we must refer our readers.

We thus close rather a long review of a short book; and this very circumstance is, we trust, of itself a sufficient proof how favorably we think of it. It contains a great deal of valuable and original matter in a small space; the style is simple, clear, and unaffected; and we earnestly recommend it to the perusal of our readers, the more so, because it contains many highly interesting observations which want of space precludes us from noticing. The cases which Dr. Naegele has given in illustration are peculiarly interesting, and will be found very instructive. The work is well worthy of translation.

Art. IV.

Das Hirn des Negers, mit dem des Europäers und Orang-Outangs verglichen. Von Dr. F. Tiedemann. *Mit sechs Tafeln.—Heidelberg,* 1837. 4to, pp. 84.

The Brain of the Negro compared with that of the European and the Orang-Outang. By Dr. F. Tiedemann. *With six plates.—Heidelberg,* 1837.

The relative capabilities of the Negro and the European races of mankind for intellectual and moral improvement—a question in itself of the highest interest, from its intrinsic value, as elucidating the natural history of the human species—has of late years acquired additional importance, from the fact that it is now upon the eve of being withdrawn from the regions of mere speculative enquiry, and subjected to the severely-searching test of actual experience. That opinions upon this point should yet remain so much divided, notwithstanding the numerous and varied investigations which it has undergone, is a sufficient proof of the great practical difficulties in which the subject is naturally involved. At the same time it must be confessed that these difficulties have been materially increased by the prejudiced views so industriously spread abroad by those whose interest it is or seemed to be to degrade the unfortunate negro as much as possible. For although, in the present state of general information, any attempt to represent the negro as merely a connecting link between the human species and the ape, as maintained by Monboddo, Rousseau, and White, would scarcely be received with any other feeling than that of ridicule, yet it cannot be denied that the exaggerated statements of his intellectual and moral inferiority, his unconquerable indolence, and the inherent treachery of his disposition, which have been thus widely circulated, have excited in the minds of many considerable doubts as to the wisdom of the recent measures of emancipation, and no slight degree of fear for the safety of the white settlers. It is therefore with great satisfaction that we hail any attempt to place the question in a clearer light, and more especially when, as in the work before us, the principles of philosophical induction are applied to its elucidation.

The monograph whose title stands at the head of these remarks is an extension and amplification of a paper by the same learned author, which was read before the Royal Society of London, June 9th, 1836. Of this paper we formerly gave a very brief notice, (*Brit. and For. Med. Rev.*, vol. IV. p. 529;) and in our number for April, 1838, we inserted a few observations by Dr. Combe, extracted from the Phrenological Journal, and calculated to throw strong doubts upon the accuracy of the inferences drawn by the author. But the great interest and importance of the several topics embraced in this enquiry have induced us to believe that a more extended investigation into its merits would not be without value; and we propose, therefore, in the following pages, to enter upon a detailed examination of the anatomical facts brought forward, and the inferences which have been deduced from them, making, as we proceed, such remarks as may appear to us necessary, either to confirm or refute the statements in the text.

Our author endeavours to determine the much litigated point of the Negro's intellectual and moral capacity, by following out a hitherto untried method, viz. by a comparison of the relative size and weight of the brain in the European, the Negro, and the orang-outang; and, by enquiring into the existence or non-existence of differences (if any) in their external appearance and internal structure, he attempts in this way to answer the two following questions: 1st. Is the brain of the Negro in any material respect different from that of the European? 2d. Does the brain of the Negro bear more resemblance to that of the orang-outang than the European?

Abundant materials for this enquiry were afforded him by numerous observations already carried on during a series of years upon the brains and skulls of individuals of all races of mankind, and by frequent admeasurement of the same parts in the orang; the most celebrated anatomical museums both on the continent and in Great Britain having been consulted for this purpose.

I. *Weight of the Brain of Europeans.* But little attention has, until lately, been paid to the determination of the size and weight of the human brain, the older anatomists almost uniformly contenting themselves with coinciding in the dogma laid down by the Greek physicians. that the brain of man is larger than that of any other animal. Piccolnomini appears to have been the first who attempted to compute its weight, which he states as varying from four to five pounds. Succeeding observers have given very different estimates; but they are all of little value, because the scale of measurement is not specified in most instances (comparison being thus rendered impossible), and because no distinction has been made regarding the age, sex, stature, and condition of each person examined. The observations of Sir W. Hamilton and Dr. Sims were conducted with greater care, and are more worthy of confidence, but they too differ considerably in their results; and it may be remarked of all these estimates, and of those also which were obtained by Tiedemann, that one source of fallacy has been uniformly overlooked, viz. the influence which various diseases exert over the weight of the encephalon, as has been most clearly shown by the very valuable results of Dr. Clendinning's experiments, recorded in his Croonian Lectures for 1838 (vide *Lond. Med. Gaz.*) "An increase of weight in the encephalon," he remarks, "is an usual effect or concomitant of morbus cordis;" and when we consider the frequency of affections of this nature, either idiopathic or as complications of other diseases, we shall see at once that any conclusions drawn from cases in which these distinctions are not noticed, cannot be entirely relied upon, however accurate they may be in other respects. Professor Tiedemann himself, in a subsequent part of his essay, acknowledges that the weight of the brain is altered by diseases; and yet in his tables he has altogether neglected this important fact. We proceed, however, with our author's investigations.

His object being to ascertain both the absolute weight of the brain and its relation to that of the whole body, numerous enquiries were instituted upon persons of both sexes, from birth up to the 82d year, great attention being in most instances paid to the stature, weight, and state of nutrition of the individual. The brain was always separated from the spinal cord, at the part where the pyramidal bodies, after their decussation, terminate in the medulla oblongata. The nerves were divided at their entrance into the foramina at the base of the cranium, and the arachnoid and pia mater were then carefully removed.

The first two tables exhibit the results obtained from the examination of the brains of 47 males and 18 females, from which the following inferences are deduced: Of 39 males, between the ages of twenty-two and eighty-two, the weight of the brain varied from 3lb. 2oz. 20gr. to 4lb. 11oz. 4dr. Among these 39, in 14 the weight of the brain exceeded 4lbs. These estimates, it may be observed, are considerably above those of Dr. Sims, who proved that in 70 cases the majority varied between 3lb. 8oz. 1dr. 20gr., and 3lb. 11oz. 6dr.; and that 13 only had a brain weighing more than 4lbs. Of 11 females, from twenty to eighty years of age, the weight varied between 2lb. 8oz. 5dr. 50gr. and 3lb. 10oz. 2dr., thus showing that the female brain has in general a much less weight than the male, a fact which is in accordance with the observations of Sir W. Hamilton, who remarks that in 100 cases scarcely one will be found weighing 4lb. troy. This difference exists even at the time of birth, the brain of new-born males weighing on an average from 13 to 14oz., that of new-born females from 9 to 12oz.

Various opinions have been adopted by anatomists regarding the period at which the brain attains its greatest size. Sœmmerring placed it in the third year; Gall and Spurzheim about the fortieth; while Sims believed that he had ascertained what, if true, would be a most singular fact—that its growth continued until the twentieth year; that from this to the thirtieth the brain diminished in bulk, then increased again until it attained its maximum size, between the fortieth and fiftieth year, after which period it once more gradually declined. According to the observations of Professor Tiedemann, the full size is acquired about the seventh or eighth year; from this period there is little or no alteration, until a very advanced age, when in many cases a real diminution of bulk does actually appear to take place, accompanied by a corresponding contraction of the cavity of the cranium.

That the computations of anatomists concerning the proportion which the brain bears to the whole body should vary even more than their estimates of its absolute weight will not excite any wonder, if we reflect upon the very great differences which exist in the magnitude of the bodies of different individuals, altogether independent of any causes which could be supposed to exert an appreciable influence over the brain. We are, therefore, inclined to attribute but little importance to this branch of the enquiry; nor do we think it necessary to enter upon any consideration of the facts adduced by Professor Tiedemann, in refutation of the old opinion, first promulgated by Aristotle, that the human brain is both absolutely and relatively larger than that of any other animal, because we believe that the fallacy of this assertion is universally acknowledged by anatomists, and because it is of little importance in determining the point in question. It may be well, however, to allude to the fact, which was first ascertained by Sœmmerring, and has been since confirmed by the researches of Blumenbach, Ebel, Cuvier.

Treviranus, Tiedemann, &c., viz. that the proportion which the brain bears to the cranial nerves, is greater in man than in any other animal, the orang not excepted; and also that the human cerebrum is the largest, when compared with the cerebellum, medulla oblongata, and medulla spinalis. This circumstance, when taken in connexion with the great superiority of man in regard to his mental capacity, the seat of which is undoubtedly in the brain, and his inferiority in many other functions of the nervous system, appears to us of the highest importance, and far more valuable in the present enquiry than any which has been hitherto mentioned.

Professor Tiedemann endeavours to show that the sensibility of different individuals, and of the same at different periods of life, is proportioned to the relation which the brain bears to the whole body. This may possibly be the case, though we cannot as yet believe it to be absolutely determined; but, even if true, it throws no light upon the question before us, for the very obvious reason that augmented nervous sensibility is by no means necessarily, nor indeed commonly, connected with increased mental capacity. Infants are possessed of a nervous system whose susceptibility is of the very highest degree, yet no one would contend that they exhibit any proofs of superior intelligence. The same is true of hysterical patients, who often, so far from being remarkable for more than ordinary mental powers, are even lamentably below the usual standard. "Emaciated persons," says our author, "are more sensitive than the corpulent;" but are they also more intellectual? It is needless to pursue the enquiry further.

II. *Weight of the Negro Brain.* With regard to the weight of the Negro brain, the observations of Tiedemann are much less satisfactory, on account of their very limited number. He commences with the assertion that the calculations of the weight of Negro brains by no means bear out the declaration of Camper, that the Negro, besides his acute facial angle, has also a smaller brain than the European; and the following are the grounds upon which he bases this opinion: Sœmmerring weighed the brain of a Negro boy, aged 14, and found it 3lb. 6oz. 3dr. apoth. In another, aged 20, but not full grown, it weighed 3lb. 9oz. 4dr., a greater weight, he remarks, than in many Europeans, better grown, and larger men. Mascagni found the brain of a Negress of twenty years to weigh 3½lb., which is more than he had ever met with among whites; but in a Negro the same observer found the brain only 1lb. 10oz. Sir A. Cooper gives the weight of the brain of a very fine Negro at 49oz. or 4lb. 1oz. A Negro examined by Tiedemann was twenty-five years of age, of short stature, and slender make. His brain, separated from the spinal cord at the medulla oblongata, weighed 2lb. 3oz. 2dr. He also measured the brains of two Negroes and a Bruges woman, in the Museum of Comparative Anatomy in the Royal Gardens at Paris, and declares them to be no smaller than the European, but he does not give the measurements.

Although from such limited data it is scarcely possible to draw anything like a fair conclusion, yet, as far as they go, they appear to us certainly not to support or confirm the inferences drawn from them by our author. The average weight of European brains, as stated by himself, varies from 3lb. 10oz. to 3lb. 11oz. in adult males, and from

3 lb. 5 oz. to 3 lb. 8 oz. in adult females, (pp. 17, 18); while of the four Negroes, whose weight can be readily compared with the above, because the scale of measurement is known, the average amounted to only 3 lb. 5 oz. 15 gr., thus scarcely exceeding the lowest average for females. The *highest* weight of the European brains which he examined was 4 lb. 11 oz. 4 dr.; the *highest* of the Negro 4 lb. 1 oz. The *lowest* male European was 3 lb. 2 oz. 20 gr. apoth.; the *lowest* male Negro 1 lb. 10 oz. (Mascagni, scale unmentioned). It may be well to remark here that Professor Tiedemann's averages for European brains in general are considerably above those of most other observers: thus, for males, he exceeds Sir W. Hamilton by 5 oz., Dr. Sims by 7 oz., and Dr. Clendinning, by 7¾ oz.; for females, also, there is an analogous difference. (Vide *Lond. Med. Gazette*, June 23, 1838.) But even this will not alter the state of the case, for the average of the above-mentioned Negroes falls much below even the smallest of these.

III. In the absence of sufficient opportunities for examining Negro brains, Professor Tiedemann next directs his attention to measuring the *cavum cranii* in the different races of mankind, and thus computing the size of their contents. For this purpose the following means were employed. The dry skulls were first weighed; they were then filled with millet-seeds, introduced through the foramen magnum, the escape of these by the other foramina and sutures being carefully guarded against. During the process frequent agitation was employed, in order that the seeds might lie closely packed. The full skull was then weighed, and its previously-ascertained weight deducted; the remainder gave, of course, the measurement of the cavity. In this manner several hundred skulls, from all nations of the earth were measured, either by himself or under his immediate inspection, few being submitted to this examination whose authenticity was in any degree doubtful. The results for each great division of mankind are given in separate tables; but these are far too long for insertion, and we shall therefore content ourselves with a simple analysis of them.

Æthiopian race. In 53 true Negroes the cavum cranii contained by weight from 54 oz. 2 dr. 33 gr. to 31 oz. 5 dr. 16 gr. In 4 Caffres it was between 43 and 37 oz. In 7 Hottentots and Bosjemans, between 42 and 32 oz. In 5 Mulattoes, between 48 and 34 oz. In 12 Negresses it was between 38 oz. 6 dr. 30 gr. and 24 oz. 7 dr. 39 gr. In a female Caffre, 39 oz. 1 dr. In four Hottentot women, between 35 and 31 oz. In a female Mulatto, 34 oz. 6 dr. 16 gr.

Caucasian race. Of 186 males the cavum cranii contained from 57 oz. 3 dr. 56 gr. (a Cossack from the Don,) to 27 oz. 6 dr. 30 gr. (a Hindoo.) In 22 females it varied from 40 oz. 6 dr. 20 gr. (Dutch,) to 28 oz. 4 dr. 24 gr. (Hindoo.)

Mongolian race. In 46 males the cavum cranii varied from 49 oz. 1 dr. 22 gr. (Esquimaux) to 13 oz. 5 dr. 24 gr. (Baschkir.) It should be remarked, however, that in two other Baschkirs it was 44 and 40 oz., so that this very small capacity cannot be looked upon as characteristic of that tribe. In 3 females it was between 36 and 31 oz.

American race. In 31 males the cavum cranii weighed from 59 oz. to 26 oz. 1 dr. 44 gr. In 4 females, from 40 oz. 5 dr. 22 gr. to 31 oz. 43 gr. The same tables also prove that the artificial flattening of the skull, which is so commonly practised among certain tribes of this race, has no effect in diminishing its capacity.

Malay race. In 98 males the cavum cranii varied from 49oz. 1 dr. 45gr. (inhabitant of the island Huaheine,) to 22oz. 2dr. (inhabitant of Amboyna.) In 10 females, it was from 41oz. (Javanese,) to 19oz. 2dr. 29gr. (Lascar.)

The whole of the facts ascertained on this point by M. Tiedemann are exhibited in a very clear manner in Tables, which are, however, too voluminous to extract. The following are the more important results deducible from them:

Males. The cavum cranii in the different races of mankind exhibits every variety, from 59 to 13oz.; but by far the greater number are found between 42 and 32oz. Thus,

Of	69	males of the Æthiopian race	64	were so circumstanced.
	186	Caucasian	143	
	46	Mongolian	29	
	31	American	21	
	98	Malay	63	

A capacity of *more than* 42oz. occurred in 5 Æthiopians, 42 Caucasians, 10 Mongolians, 7 Americans, and 21 Malays; and a capacity *under* 32oz. occurred in only 1 Negro and 1 Caucasian, but in 3 Americans, 7 Mongols, and 14 Malays.

Females. The cavum cranii varies among females from 41 oz. to 19oz., and is upon the whole smaller than in males. In the majority it was between 38 and 30oz. viz.

Of	18	Æthiopian women	16	were so circumstanced.
	22	Caucasian	19	
	3	Mongolian	3	
	4	American	3	
	11	Malay	7	

More than 38*oz.* were only found in 1 Negress, 2 Caucasians 1 American, and 1 Malay; *less than* 30*oz.* only in 1 Negress, 1 Caucasian, and 3 Malays.

"The results of our enquiries," says Professor Tiedemann, in commenting upon these tables, "undeniably show that those anatomists and naturalists have fallen into error who have described the cranium of the Negro of a less capacity, and his brain smaller than that of Europeans and the other races." To a certain extent this inference is correct, the tables evidently showing that there is no such great inferiority in the Negro, as has been frequently asserted. At the same time, it is impossible for an unbiassed observer to fail being convinced of the fact, that the average capacity of the Æthiopian skull is somewhat lower than that of the European, and that a large size is considerably less common among them than among *any other races of mankind.* We forbear, for the present, entering more fully upon this subject, reserving our remarks until we have occasion to notice more particularly the state of intellectual development of this people.

IV. *Comparative measurements of the Brain and Spinal Cord of Negroes and Europeans.* There was no apparent difference in the external aspect, nor in the internal structure of the medulla spinalis of a Negro (Honoré), a man of small stature, whose body Tiedemann had an opportunity of examining. The following table exhibits a comparative view of its dimensions and those of the same parts in a male European of 5 feet 8 in. and in a woman of 5 feet.

	Negro.		Europ. Man.		Eur. woman.	
	in.	lines.	in.	lines.	in.	lines.
Length of whole cord from the pons variolii...	14	11	17	3	14	10
Breadth of medulla oblongata below the pons		10		11		$10\frac{1}{3}$
at decuss. of pyramids		$5\frac{2}{3}$		$6\frac{1}{4}$		$5\frac{1}{3}$
of med. spin. in upper part of neck		$5\frac{1}{3}$		$5\frac{1}{2}$		$5\frac{1}{3}$
in lower part of neck		$6\frac{2}{3}$		$6\frac{3}{4}$		$6\frac{1}{3}$
in middle dorsal region		$4\frac{1}{2}$		5		$4\frac{1}{3}$
in lower dorsal region		$5\frac{1}{3}$		$5\frac{2}{3}$		$5\frac{1}{4}$

The slight differences observed here may be evidently traced to variations in the whole size of the body; and it is of importance to notice that the Negro differs from the others on the side of *deficiency*, not of *excess*, because, as we shall see afterwards, in the orang, to which he has been so often compared, the case is precisely reversed.

The two next tables in the text exhibit the measurements of the cerebellum and nodus encephali (pons variolii) in 4 Negroes and 9 Europeans; we have reduced them into one, giving merely the averages, which are quite sufficient for our purpose.

	Negro.		European.	
	in.	lines.	in.	lines.
Greatest breadth of the cerebellum........................	3	$5\frac{3}{4}$	3	$7\frac{1}{3}$
	average of 4		average of 9	
Long diameter of cerebellum in the middle...............	2	$4\frac{7}{8}$	2	$5\frac{1}{12}$
	average of 4		average of 6	
Breadth of the nodus encephali between the fifth pair of nerves..	1	$1\frac{1}{2}$	1	$1\frac{5}{8}$
	in 1 case		average of 8	
Long diameter of the same	0	$10\frac{1}{3}$	0	$14\frac{1}{12}$
	in 1 case		average of 6	

Here, again, it should be particularly remarked, that in the Negro these parts are rather of an *inferior* than *superior* magnitude.

The following table represents the comparative measurements of the cerebrum in 4 Negroes, 7 European males, and 6 European females; it is reduced from three tables, which are given in the original, by taking the averages as before.

		in.	lines.
Average length of brain in	4 Negroes	5	11
	7 European males.......	6	$2\frac{1}{4}$
	6 European females	5	$10\frac{1}{3}$
Average greatest breadth in	4 Negroes	4	$8\frac{1}{6}$
	7 European males	5	$1\frac{1}{4}$
	3 European females ...	5	$4\frac{1}{3}$
Average height of brain in	3 Negroes.................	2	$11\frac{1}{3}$
	7 European males	3	4
	4 European females	2	$9\frac{1}{3}$

There is clearly, then, notwithstanding the assertions of Tiedemann, a very considerable inferiority of size in the cerebrum of the Negro; and this perfectly coincides with the inferences we have already drawn from the measurements of the cavum cranii. In the arrangement of its parts and in their more intimate structure, the Negro brain is precisely similar to that of the European: the only perceptible difference being that the convolutions were somewhat broader in the anterior lobes, and also more symmetrically arranged in the two hemispheres, than is commonly observed. The cerebellum was entirely concealed, and partially overlapped posteriorly by the cerebrum. In apes the opposite obtains.'

V. *Has the Negro thicker or larger Nerves than the European?*—Sœmmerring, who, of all anatomists, has paid most attention to this subject, has laid it down as a rule that in the Negro the cranial nerves are somewhat larger than in the European, and that this difference is most remarkable in the olfactory, the optic, and the fifth pair. But Professor Tiedemann not having observed this to be the case in the four brains which he examined, considers himself entitled to deny the existence of any such difference whatever. We must, however, confess that there appear to us very insufficient grounds for such a conclusion; and in the absence of more complete and satisfactory evidence, we feel much inclined to adhere to the old opinion, which is, moreover, supported by the larger size of the foramina in the base of the skull, through which these nerves pass, and the much greater development of the organs of sense in the Negro. "Nature," says Dr. Prichard, "seems to have made, if we may use the expression, a more careful provision for the perfect and full development of the sensitive power. The orbits are large and capacious; the cavity of the nose has a remarkable amplitude in the Negro, and all the parts which are subservient to the sense of smelling have a very perfect conformation. The upper turbinated bones are large and finely convoluted, presenting an extensive surface for the expansion of the nasal membranes. The passages of the posterior nostrils are wider in the Negro than in the white man. The African has accordingly, as it has been often remarked, a very acute perception of odours. It has been asserted that the Negroes of the Antilles can distinguish in pursuit the track of white and black people by the sense of smell."

VI. *Does the Brain of the Negro bear a greater resemblance to that of the Orang-outang than the European?* The result of M. Tiedemann's dissections of two individuals of this species, one from Java, the other from Africa, and preserved in the Hunterian Collection, Royal College of Surgeons, London, have led him to the conclusion that the brain of apes and orangs differs from the human in the following respects: 1. The cerebrum is absolutely and relatively to the mass of the body smaller, shorter, narrower, and more flattened. 2. It is smaller in relation to the nerves. 3. The cerebral hemispheres bear a smaller proportion to the spinal cord, medulla oblongata, cerebellum, and corpora quadrigemina. 4. There are fewer convolutions and shallower sulci. In all these it differs from the Negro as well as from the European, the only point of greater resemblance between them being the more symmetrical arrangement of the convolutions. How much less the relative size of the brain to the body is in orangs than in man is well illustrated by the fact that in a large pongo from Borneo, the cavum cranii measured only 11 oz. 7 dr., which is even less than in congenital idiots.

VII. As a final result of these investigations, Professor Tiedemann believes himself entitled to draw the following conclusions: 1. That the brain of the Negro is, in general, or on an average, as large as that of the European and other races of mankind. (In Europeans and Malays a brain *below* the average size is more common than in Negroes.) 2. That the cranial nerves of the Negro are not, as Sœmmerring imagined, larger than those of the European. 3. That the medulla spinalis, the medulla oblongata, the cerebellum, and cerebrum of the Negro exhibit no difference from the European, either in their external appearance or internal structure, excepting that the hemispheres of the cerebrum are somewhat smaller. 4. That the brain of the Negro bears no more resemblance to that of the orang than the European does, excepting the more symmetrical arrangement of the convolutions in the two hemispheres. But this cannot be considered as established.

In reflecting upon these inferences and the data upon which they are founded, we have been unavoidably led to the opinion that the learned author of the paper before us has allowed his philanthropic feelings to betray him into the very common but most dangerous error of hasty generalization. We have seen that the few Negro brains submitted to examination exhibited an average size considerably inferior to that of Europeans, and that the measurements of the cavum cranii bore witness to the same fact; the conclusion from which is inevitable, and directly opposed to the inference drawn by our author. And it should be particularly observed here that the *average* size is all with which we are concerned, not the fact that in a few instances the brain reaches a more than ordinary magnitude; because it is evident that if the question is to be determined at all in this way, it must be from the *general rule*, not from the *exceptions*. But we may very properly enquire in this place whether the method adopted in this investigation is calculated to lead to the results expected, whether the magnitude of the whole encephalon is a measure of the intellectual capacity of the possessor? On this point we would again refer our readers to the very valuable observations of Dr. Combe, inserted in our Fifth Vol., p. 385, believing that they exhibit in a most satisfactory manner the errors into which Professor Tiedemann has fallen. For whether we adopt wholly or only partially the views of the phrenologists regarding the subdivision of the brain into organs, we must acknowledge that it seems to us sufficiently established that the higher powers of the intellect are more decidedly proportioned to the magnitude of the anterior lobes of the cerebral hemispheres than to that of any other portion of the cranial contents; and in this respect, by Tiedemann's own confession, the Negro is very deficient. At the same time, it is but justice to observe that the same measurements which have led to this conclusion also prove that the organs which, if phrenologists be correct, are chiefly connected with mere animal propensities, do not exhibit any excess of development, and that if the Negro brain is, in some respects,*inferior* to that of the European, it is in the same particulars much more decidedly *superior* to that of apes.

On the whole, then, we feel considerable doubts as to the possibility of determining the point in question by any such researches as those which we have been engaged in examining, partly on account of the difficulty of obtaining sufficient data for the purpose, and partly because the evidence thus procured is of a very uncertain character. The same objections, however, do not by any means apply to the other statements advanced by our author in favour of the Negro's capability for moral and intellectual improvement. To these we are inclined to attribute far more weight, because they are based upon the results of actual experience, and are unconnected with any theory or hypothesis whatever.

That even the external characteristics of the Æthiopian variety of mankind, as laid down by many writers, are mere caricatures, drawn from partial observation of exaggerated peculiarities, is now sufficiently established; and precisely the same may be said with regard to their intellectual and moral capabilities. The picture drawn by those who have only been acquainted with this unfortunate people, in the degraded position of slaves, and which portrays them as utterly vicious, malignant, perverse and faithless, irretrievably subdued by indolence, incapable of improvement, and scarcely even deserving a place among rational creatures, is so palpably overcoloured, so evidently the result of prejudice and imperfect observation, as to deceive none who will examine the subject for themselves. The accounts of those enterprising travellers who have penetrated into the interior of Africa are well known to be altogether different. These represent the African to possess, in full vigour, all the intellectual and moral qualities of his white brethren. It seems, however, to have been too generally overlooked by all parties, that there are distinct tribes among Negroes as among Europeans; and that, while some (those formerly most known) are very inferior, others, more recently discovered, especially in the interior of Africa, are proportionably superior. The former clans have low heads and smaller brains, while the latter approach, both in brain and feature, the European standard.

Another point of great importance, as influencing mental manifestations, has been entirely overlooked by Tiedemann and most others, viz. the *quality* of constitution or temperament. Generally speaking, the European is finer in fibre than the Negro, and hence, with otherwise equal powers, the former would be more susceptible of polish and refinement. Why should all Negroes be alike, any more than Europeans? The French brain is smaller than the English, as the French hatters experienced when they had to make blocks expressly for the English soldiers. This remark applies to Dr. Sims, and nearly all others who have weighed brains. How could the weight be uniform, when it is palpable to the eye that, taken generally, both Irish and French brains are smaller than English and Scotch; and, in some districts, all these are inferior to German and Swiss? But then the Swiss, with their large heads, have slow, heavy temperaments, and are, consequently, frequently inferior in actual performance to those with feebler but livelier powers. In this, as in every other enquiry, we must look about us, on all sides, if we would find truth; we must not follow any one path only, merely because we have chanced to hit upon it. In investigating the physical and intellectual characters of races as of individuals, we must take Nature as she is; and, with the more philosophic phrenologist, be careful not to mix the front and back, the top and base of the head, and one temperament with another, into one mass, and call it "fact."

In bringing these remarks to a conclusion, we feel it necessary to make a few observations in regard to the manner in which Professor Tiedemann has performed his self-imposed task. In perusing the work before us, we could not avoid being forcibly struck with and much grieved by the evident spirit of partisanship in which the whole enquiry has been conducted. Every circumstance, however trivial, which in any degree favoured his manifestly preconceived views, is largely dilated upon, and most fully credited; while facts of an opposite tendency are carelessly slurred over, or declared to be fallacious. To give an example: In comparing the brains of the Negro and the Orang, he remarks, that there appears to be one point of resemblance between them, which does not exist in the European brain, viz. the more symmetrical arrangement of the convolutions in the two cerebral hemispheres; but he immediately adds, "whether this mark is constant I cannot determine, because I have only examined four Negro brains." Yet, though the number of observations are precisely the same, he finds no difficulty whatever in founding upon them the general rule, that the cranial nerves of the Negro are neither thicker nor larger than those of the European. This is not the spirit in which a philosophical enquiry should be conducted; and it is truly melancholy to behold so old and experienced an observer falling into an error which should at least be confined to novices alone. On the whole, we do not think that this publication is by any means calculated to extend the learned author's reputation. It can boast of little originality, excepting in the method applied to elucidate the proposed questions; and this, for reasons already stated, we do not look upon as satisfactory. And although few could read his memoir without feelings of admiration and pleasure, at the heartfelt warmth and enthusiastic zeal with which he advocates the cause of an oppressed people, yet we believe that the majority would rise from the perusal with the impression that the facts and arguments brought forward, while they certainly exhibit the measureless superiority of the Negro over the ape, yet in a great measure, if not entirely, fail to establish the so eagerly contested point of his *perfect equality* with his more fortunate European brethren.

Art. V.

1. *Beiträge zur operativen Orthopädik, oder Erfahrungen über die subcutane Durchschneidung verkürzter Muskeln und deren Sehnen.* Von Dr. Louis Stromeyer, Königl. Hofchirurgus, &c. zu Hannover. *Mit* 8 *Lithographirten Tafeln.—Hannover,* 1838. 8vo, pp. 154.

Contributions to Operative Orthopædia; or, Practical Observations on the Subcutaneous Division of Contracted Muscles and their Tendons. By Dr. Louis Stromeyer, of Hanover. *Illustrated by* 8 *Lithographic Plates.—Hanover,* 1838.

2. *Handbuch der Plastischen Chirurgie.* Von Dr. Eduard Zeis, Doctor der Medicin und Chirurgie, &c. (19 *Abtheilung.*) — *Berlin,* 1838. 8vo.

Manual of Plastic Surgery. By Dr. Edward Zeis, of Dresden. (*Chapter XIX. on the Division of Tendons.*)—*Berlin,* 1838.

3. *Mémoire sur la Section du Tendon d'Achille dans le Traitement des Pieds-bots.* Par M. Bouvier. *Agrégé libre à la Faculté de Médecine à Paris, &c.—Paris,* 1838. 4to, pp. 73.

Memoir on the Section of the Tendo-Achillis in the treatment of Club-foot. By M. BOUVIER, of Paris. *Forming part of Vol. VII. of the Memoirs of the Academy of Medicine.—Paris*, 1838.

4. *A Treatise on the Nature of Club-foot, and analogous Distortions; including their Treatment, both with and without Surgical Operation. Illustrated by a series of Cases and numerous Practical Instructions.* By W. J. LITTLE, M.D., Lecturer on Comparative Anatomy at the Medical School of the London Hospital, &c.—*London*, 1839. 8vo, pp. 276.

AT the close of an article on "Plastic Surgery," in our Fourteenth Number, we promised to pursue our notice of Dr. Zeis's work, by passing in review that division of it which treats of the operations for the cure of deformities resulting from contracted muscle or tendon; and having now added, as text-books, the other works which head the present paper, we proceed to redeem our pledge. Although the operations alluded to by no means form an essential part of plastic surgery, yet we cannot but coincide with Dr. Zeis in regarding the two subjects as legitimately allied. The object of both is rather the reparation of some existing deformity than the cure of a disease: and this consideration, taken in connexion with the fact of their having sprung almost simultaneously into general notice, points to the discussion of the one as an appropriate sequel to that of the other.

There are few more interesting sources of meditation than that which is afforded by a comparison between the state of our past and present knowledge, as we review the progressive advance of the human mind through ages; and we have slight sympathy with the chilling and misnamed philosophy which can regard with indifference the mighty achievements of the intellect of man, unless moved by the selfish promptings of the economist and calculator. Yet how difficult is it for us to place a due value upon discoveries with which we have been always familiar: how little can we appreciate the obstacles presented by previous ignorance, limited means of observation or experiment, and, worse than all, by the confirmed prejudice of long-established error! Indeed, a certain proportion of wonder at the obtuseness of the discoverer's predecessors, appears to be almost inseparable from a contemplation of the simplicity which a newly-developed law presents. Thus, for example, we are far more prone to wonder how it was possible for any one conversant with the simple anatomy of the circulating system to mistake the real use of its various component parts, than to admire the sagacity of Harvey, which led him, undisturbed by the many difficulties which encompassed him, to an exposition of the real and appropriate functions of the heart, the arteries, and the veins; and thus, now that the noseless, the wry-necked, and the lame are restored to happiness and society, we cannot forbear reflecting on the singularity of the fact that we should have so long been content, whilst cultivating the maiming department of our art to the utmost, to allow this beautifying and comfort-restoring division to have remained comparatively neglected.

The operations which we are about to discuss are available for the cure of certain deformities of the neck and extremities alone; and of these, such only as have for their cause the contraction or shortening of some portion of the active locomotive organs: it becomes the operator, therefore, to make the state of the articulation, which forms the angle of contraction, his peculiar study, before he attempts to remedy that which original malformation or disease of the hard parts may have rendered irremediable. Thus it is the abnormal condition of the spinal column which, in nearly every instance, is the source of the various distortions of the trunk, and which necessarily precludes the possibility of relief from section of muscle or tendon. In most of these cases the alteration in the relation of the bones to one another is the primary condition; and the resulting wasting or shrinking of the muscles is altogether secondary or consequent; whereas precisely the converse is the case in curable instances of contracted neck or extremities. There are, doubtless, exceptions to the former of these positions, in which much may be done by attention to the proper employment and development of the muscles; and to such of our readers as may take an interest in the subject, we recommend the perusal of Dr. Stromeyer's little work, "Ueber Paralyse der Inspirations-Muskeln," as well as a paper by Dr. Gunther, of Hamburg, which will be found in our Fourth Volume, p. 509. Further, the contraction of articulations consequent on destruction of the superficial soft parts is a condition to which both the neck and extremities are obnoxious; but these, for the most part, incurable deformities require an essentially different treatment, and do not form a part of our present subject; we shall, therefore, with trifling exceptions, confine ourselves, on the present occasion, to the discussion of such cases only as find their remedy in section of muscle or tendon.

Dr. Stromeyer's preface is succeeded by a long, we might say lengthy, introduction, wherein are discussed various abnormal conditions of the muscular system, with their bearings upon coincident deviations from natural conformation in the bony fabric, and the causes and treatment of the same. From among these observations we propose to select a few of such as possess most interest or bear more particularly on our subject. The author next lays before us a history of what has been done in times past for the advancement of this branch of our art; and subsequently proceeds to the successive consideration of deformities resulting from muscular contraction, of the feet, knee, and hip-joints, of the fingers and elbow, and lastly of the neck. Dr. Little's work is also preceded by an extended introduction, which includes the morbid anatomy and history of talipes, in the former part of which department he presents us with a great deal of matter possessing both novelty and interest. In the order we shall pursue in our subsequent analysis of the various materials we have collected, we shall endeavour, by a careful selection, synthetically to arrange that which, we trust, will form a useful though a brief compendium of the various opinions and modes of practice adopted by different operators.

Our subject offers a striking illustration of the essential connexion between the extension of anatomical research and the advancement of surgery; for, in tracing the causes which have retarded the progress of the curative means employed for club-foot in particular, probably none have operated so powerfully as a neglect to examine anatomically the sources of distortion, and an undue appreciation of the abnormal relation of the bones in the adult foot. Besides this, however, as Zeis remarks, the groundless fear of tetanus, exfoliation, or non-union of tendon after section, aided in no trivial degree in throwing obstacles in the way of attempting a cure by operation. To a certain extent, the remark of Scarpa is justified by dissection of deformed feet, viz., that the appearances presented are not necessarily identical even in the same form of distortion; but, as Dr. Little justly observes, it is in the infantile and unused foot that we must seek for the true anatomy of these altered relations, and not in feet, the abnormal form of which has been further modified by external agents, such as "improperly-directed mechanical treatment and the act of walking." The following is Dr. Little's arrangement of opinions relating to the causes of these distortions.

> "1. That the primitive formation of the bones is unnatural and incomplete. 2. That the bones, being originally perfectly formed, become injured and distorted by causes independent of the formative process; viz. by pressure occasioned by the fœtus drawing the limbs into unnatural positions; by an improper situation of the fœtus in the uterus; or by certain ligaments becoming elongated, and the articulations distorted from contraction of some of the muscles and relaxation of others. 3. That, whatever may have been the condition of the bones on the occurrence of distortion, the act of walking displaces and injures them." (p. 22.)

Glisson, Camper, and Blumenbach, were amongst those who advocated the first of these opinions; but Scarpa, in a memoir, in which he described the most ordinary appearance on dissection, and the mechanical treatment of these distortions, remarked that the deformity of the individual bones was slight, and also noticed the important point, that the relation of the astragalus to the articular surfaces of the tibia and fibula was comparatively but little disturbed. The dissections of Dr. Little have induced him to confirm this statement of Scarpa.

> "In every instance I have examined, either of an infant or adult, however great may have been the state of extension and adduction of the foot, some portion of each of the three articular surfaces of the trochlea of the astragalus was in contact with an equal proportion of the three articular surfaces presented by the tibia and fibula in the ankle-joint. The astragalus usually inclined inwardly, but only to such an extent as to cause the external surface of its trochlea to project from the ankle-joint somewhat further than the internal surface." (*Ib.* p. 29.)

The dissections of Jörg, Colles, Cruveilhier, disposed them to attribute the deformity in most instances to malformation of bone, which they considered as a serious, if not an insuperable, obstacle to the replacemen or reduction of the foot to its normal condition. Such was likewise the notion of Delpech when he first published upon the subject;* but he was subsequently induced to subscribe to the opinion that muscular action is the cause of the distortion. After expressing his own opinion, which we have quoted, Dr. Little concludes his summary of the investigations into the anatomical changes of varus, in the words of Scarpa: "none of the tarsal bones are actually dislocated; but in addition to the state of extension of the ankle-joint, they undergo rotation on their axes, and the astragalus suffers less alteration of position than either of the tarsal bones."

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* Chirurgie Clinique, p. 166. 1823.

In investigating the causes of these changes, we find the enquiry naturally resolves itself into two clauses, the source of mischief in the congenital distortion, and that which occurs in after-life. Club-foot and the analogous distortion of the hand are of far more frequent occurrence, as congenital malformations, than the production of any exciting cause which is brought into operation subsequently, and therefore quite independently of the fœtal period of life. This fact has induced early writers upon the subject, at a period when, as we have already observed, the changes in anatomical relation had not been accurately investigated, to refer these congenital distortions to an improper position of the fœtus in the uterus, or an undue and unequal pressure of this organ upon its contents. We are indebted to Rudolphi* for directing attention to the nervous centres as the probable seat of primary disturbance in most instances; an opinion which he founds upon the violent convulsive motions to which children are sometimes subject during uterine life, and also upon the early period at which these malformations are occasionally found, before the uterus can be supposed to have acted mechanically upon the embryo. Dr. Little refers to several embryos of from three to five months old, affected with talipes, which were collected by Rudolphi and his distinguished successor Müller, and deposited in the Berlin museum: some, he remarks, "exhibiting malformations or deficiency of the cerebrum and medulla spinalis; some anencephalous and others hemicephalous, both the hands and feet being affected in this manner, showing the extensive participation of the system in the disturbance and destruction of the nervous centres." (p. 33.) The frequent occurrence of coexisting talipes in both limbs has been cited by Cruveilhier as an argument in favour of his theory that malformed astragalus is the most frequent cause of the distortion. This opinion Dr. Little combats; and we fully coincide with him in regarding the above fact as rather favouring the inference that one common disturbing cause, such as is met with in a disordered nervous centre, must be sought for; a supposition which is by no means impaired by the occasional accompaniment of monstrosities in other organs. The hypothesis of Von Walther† is more curious than probable, viz., that "talipes is a natural grade of the development of the foot, and embryos of three or four months very frequently retain one or both feet in this state." The observation with which Dr. Little dismisses this theory is sufficiently conclusive. "From this (he says) it would follow, that every fœtus, at an early period of development, has two club-feet. But if talipes varus be a state of natural embryo-development, the enquiry may be instituted as to which of its natural stages talipes equinus and talipes valgus are respectively referrible. By whatever further stretch of imagination this theory may be applied to the explanation of talipes equinus, its application to talipes valgus is decidedly inadmissible."

A memoir, which had for its object the elucidation of the above question, (viz. the cause of congenital club-foot,) was read before the

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* Physiologie, p. 319. † System der Chirurgie. Band i. p. 349. Berlin, 1833.

Academy of Medicine in Paris by M. Guerin, on December 11th, 1838; as it presents, probably, the most rational account of the matter, we will quote the leading points given in the Gazette Médicale for the same month.

> "M. Jules Guerin draws attention to his idea that convulsive retraction of the muscles is the cause of congenital club-foot. He asserts that a great number of observations made on monsters and on the normal fœtus have proved that a relation exists between congenital muscular retraction and congenital alterations of the cerebro-spinal system, from the complete destruction of the brain and spinal marrow to the lesion of a circumscribed point of one of these centres. When club-foot exists together with other deformities of joints, they are all the result of convulsive retraction of the muscles, characterized by an extreme shortening of the greater part of the muscles of the trunk and extremities. When the retraction is limited to the muscles of the leg alone, forming club-foot, it may be referred either to a general convulsive affection which leaves unequivocal marks of its existence in the features of the countenance, the conformation of the skull, the direction of the eyes, the inequality of strength on the two sides of the body; or to a mere local lesion confined to certain nervous branches, and consequently to certain muscles which are alone affected. The convulsive retraction of the muscles is a complex action made up of three elements; the immediate shortening of the muscle, a certain degree of paralysis, and a consecutive arrest of development, which prevents its following, *pari passu*, the growth of the skeleton, and thus increases the primitive shortening of the retracted muscles. M. Guerin does not admit the existence of any other causes of club-foot, but allows that the compression of the uterus may produce a degree of deformity, which has not, however, the anatomical characters peculiar to club-foot."

M. Duval coincides with M. Guerin in these conclusions, rejecting, as a cause of club-foot, the pressure exercised on the fœtus by the walls of the uterus, in cases where the liquor amnii is deficient in quantity; and in this opinion he is joined by Breschet: but Cruveilhier maintains that in such cases the pressure of the uterus is an efficient cause of club-foot; and in proof of this he alleges that the foot, which is placed anteriorly, and is consequently more exposed to pressure, is generally affected, and, when both are deformed, the anterior one is more so. We have already noticed that Delpech abandoned his original notion that bony deformity was the cause of club-foot; it may be interesting to add that his reason for so doing was founded upon witnessing two cases of this distortion, of which one arose from the division of the popliteal nerve, and the other was consecutive to an abscess in the thigh with necrosis of the femur. In these, as in most if not all cases, the immediate cause is an inequality in the power of antagonist muscles, producing a deviation from the natural position in the direction of the contraction. We will conclude our remarks on this division of our subject, by quoting some excellent observations of M. Duval, which bear upon the point at issue. Having stated that a "bad position or an exaggeration of the natural position of the feet in the uterus" (not simply uterine pressure), "during some period of pregnancy, aided by a partial delay in development," may be an occasional though a comparatively rare source of the mischief, he proceeds as follows:

> "A more frequent cause of club-foot is *a lesion of the cerebro-spinal system*. Infants born with paralysis of one or both sides are frequently affected with club-foot on the paralyzed side. The paralysis may disappear after a few months and the deformity remain; or it may persist, and in that case the deformed limb will generally become atrophied; indeed, at birth it is generally smaller and possesses less warmth

than the opposite one, and this symptom will often enable us to decide if the deviation has arisen from disease of the nervous system, which may occur to the fœtus in the womb as well as to the infant possessing an independent existence. Arrest of development is by some persons, and particularly by M. Breschet, placed among the causes of *strephopdoy*,[*] and in corroboration of this idea it is stated that in one case the affected limb was three inches shorter than the sound one; in another there was a deficiency of five inches; in another case the last phalanges of the toes were wanting. Eight out of ten cases of consecutive club-foot arise from inflammation of the cerebro-spinal system, and are preceded by convulsions, paralysis, or muscular contractions. Why may not the same be the cause of the congenital disease? When a limb is first affected with paralysis either partial or complete, there is first retraction rather than actual shortening of the muscles; for a long time they may be brought back to and maintained in their normal length by means of a machine; but when the muscles are actually shortened, division of the tendons is necessary to restore the natural length and position of the limb." (*Revue Médicale.*)

Both Zeis and Bouvier present us with notices of the successive changes which tendons undergo in the process of reunion, (*Zeis*, p. 548; *Bouvier*, p. 437.) The former author quotes the experiments performed by Von Ammon upon horses and dogs; and although the results obtained from their several observations disagree in some points, (owing, probably, to the different class of animals selected for experiment,) they all coincide in leading to the important conclusion that union is equally solid in the end, even after the separation of the divided extremities has been very considerable; that, in other words, a wide separation is no obstacle to perfect reunion, although the process is, in such case, necessarily more protracted. We had an opportunity, a short time since, of examining a tendon (the tendo achillis), in which reunion was perfected subsequent to division by the knife; after short maceration, a longitudinal section presented a cartilaginous texture with distinct, glistening, tendinous fibres interspersed, and taking a parallel course from the normal portion above to that below, indicating a renewed continuity of the original structure.

We have already indicated the propriety of making the condition of an articulation the first object of careful examination, before having recourse to the knife for the relief of a contracted muscle; supposing, then, that such investigation prove satisfactory, it becomes a matter of much importance to discover the original source of mischief, the cause of the contraction, (which, as we have shown, may or may not be local,) and to employ such palliative or curative remedies as may appear adapted to the peculiarities of the case; for, be it remembered, although the section of a muscle or tendon be but a simple operation, yet it is by no means calculated to supersede, in every instance, all other remedies. Even Dr. Stromeyer appears willing to admit thus much, for he devotes some little space to the consideration of various palliative means; and as they are not likely to receive more commendation than they deserve at his hands, we cannot, perhaps, do better than select a few of his observations upon the subject.

"Every species of muscular contraction," he remarks, "whether the consequence of habitual spasm, or of irritation of the peripheral extremities of the nerves, or the nervous centres themselves, agree in regard to the result, that the muscular fibres are so deprived of their healthy tone, that neither antagonist muscles nor external force have the power to restore them to their natural length. . . . . Nature seems to hold out but little prospect of unaided reparation in such conditions, save, perhaps, in the spontaneous cure by anchylosis, as in white swelling, &c. But art has done a vast deal for these cases; and the most eligible and universally applicable remedy for contractions is to be met with in warm bathing, by the benefit derived from which many thousands have annually restored to them the use of their limbs." (p. 12.)

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[*] From στρέφω, *to turn*, and πους, *a foot*, club-foot, so called by M. Duval.

Dr. Stromeyer then goes on to observe that "friction may be a most valuable agent when appropriate cases are selected for its employment." In expressing his opinion upon the nature of the embrocation, or whatever form the application may be made to assume, he deems the sedative class as alone applicable to contracted limbs, whilst the opposite are indicated where the muscles have lost their tone or become paralyzed. In the former class, irritating liniments, electricity, moxas, &c. may be all employed in vain to relieve a condition which the simple section of the affected tendon or muscle has at once cured. Again, with respect to manipulation (shampooing), Dr. Stromeyer thinks that gentle friction with the hand, and the warmth thereby produced, may aid collaterally in the cure by restoring tone to relaxed muscle: but his experience has induced him to place but little reliance on the employment of antispasmodics or narcotics, which, whether exhibited internally or applied as embrocations to the surface, act, as he justly observes, more readily on the nervous centres than locally. Where deformity is solely the result of *local* irritation, leaving behind it a retracted condition of a certain set of muscles; the same author remarks that bandages and other mechanical apparatus may exercise a beneficial effect in some instances, but adds that the section of such contracted tendons is ordinarily the more ready and satisfactory means of completely removing the defect.

Supposing, then, that an operation is decided upon, it by no means follows that we are, therefore, to dispense altogether with the abovementioned remedies; on the contrary, it behoves us to call in aid all such means as may collaterally assist in procuring a successful result from the use of the knife; and, as every prudent surgeon accustoms his pauper-patient to the air, diet, and confinement of the hospital before he proceeds to the performance of any important operation, so may bathing, rest, and the use of the apparatus to be afterwards applied, be advantageously employed for some little time before the section of a tendon: the counteracting tendency offered by opposing elastic or contractile tissues is thus decreased, and the limb is habituated to the irksomeness of confinement and compression. Previously to describing the operation, Dr. Stromeyer makes some practical remarks of a general kind respecting the incision, the knife to be employed, &c. That the external opening should, if practicable, be single and small, is an axiom to which all operators willingly subscribe; although, doubtless, it may be requisite occasionally to deviate from this rule where a large muscle is the object of division. A narrow knife is, however, under any circumstances, the most desirable, and in form it should be slightly curved, as a bistoury, but without any button-point, an addition which renders the section more troublesome, and by contusion of the neighbouring cellular membrane, is likely to give rise to consequences which may retard the healing of the external wound. Dr. Stromeyer has from this cause seen a tedious suppuration ensue. Mr. Whipple, of Plymouth, lays some stress upon the advantages derivable from an oblique division of the tendon, the reasons for which he thus states: "first, by so doing, you have a larger surface for nature to carry on her operations; secondly, you have the obliquely divided tendon in nearer approximation, and thereby secure a firmer ligamentous band than in the transverse division; and thirdly, the application of the instrument does not separate the lips of the wound, a desirable point, as the sooner it heals, so as to prevent the escape of lymph, the better."* Respecting the period of commencing the after-extension, Bouvier and Stromeyer are at issue: the former advocating the *immediate* employment of mechanical means for this purpose, whilst the latter deprecates what he styles the "so-called improvement" of our French author, by reason of the evils which he enumerates as resulting from this practice, of which the principal is the production of an unnecessary quantity of new intervening material, having the very prejudicial effect of impeding, if not of permanently preventing a complete restoration of the muscles to a state of healthy tone. In this point Dr. Little coincides with Stromeyer, whilst Mr. Whipple informs us that he has treated his cases, like Bouvier, by immediate extension. Dr. Zeis states that both he and Von Ammon have practised extension immediately after section of a tendon with good results, and therefore thinks it unnecessary to defer the application of the requisite apparatus. Thus there are a variety of opinions upon this simple point; for ourselves, although we think that in the long run the ultimate success may be nearly the same in each plan of proceeding, we are disposed to regard Stromeyer's as the more eligible, because less abruptly coercive, mode of treatment. The apparatus which is needed for the purpose of making and keeping up extension after division of the tendo achillis may be very simple; and as every surgeon would probably have his own contrivance for producing the required end, it is needless to describe any particular form. One positive and one negative property it is most essential to combine in the construction of the said machine, viz., the capability of graduating and preserving a steady extension, without making such local pressure as may induce injury to the superficial soft parts. For the purpose of avoiding this evil, the surgeon cannot be too careful in the frequent inspection of the apparatus, and in making enquiries of his patient whether pressure is felt more at one part than at another: we had occasion once to witness a very severe excoriation and partial destruction of the cutis on the instep from this cause; the apparatus was necessarily laid aside, but the case had fortunately proceeded sufficiently far for the cure to be completed without its readjustment. We need scarcely add, that it is by no means less important to the favorable issue of an operation, that the extending medium should not be removed or even relaxed, except under urgent circumstances, such as that above named, which leave no alternative. After perfect union has taken place, Dr. Stromeyer recommends that friction with stimulating lotions should be combined with gentle use of the limb. We are, however, anticipating our subject and outstripping our authors, and must therefore commence, *de novo*, with what we can collect of the *history* of tenotomy or tendon-cutting.

Of all the operations of this class, the cases in which the tendon common to the two gastrocnemii muscles has been divided have far exceeded all others put together, both numerically and in the interest which that particular operation has excited in the profession; the deformities, for the cure of which it has been practised, being of frequent occurrence as congenital conditions, and likewise as the result of disease or accident. From the notice which Hippocrates takes of the subject of distorted feet,* we have little reason to doubt that he understood and successfully treated these deviations from a normal condition by simple mechanical means, such as bandaging and support of various kinds afforded; and, as Dr. Little remarks in the introduction to his work (p.xlvii.), we may even question whether he had not contemplated the practicability of some operation if he did not try one; for, after speaking favorably of the bandaging system, he adds, "atque quidem est curatio, et neque sectione, neque ustione, neque aliâ varietate quicquam opus habet." Be that, however, as it may, we read of no advance being made in the treatment of these deformities, until quite the latter end of the last century; it was then that the first recorded case of section of the tendo achillis was published by Thilenius,† who practised his profession near Frankfort. The subject was a girl, aged 17, who had been afflicted from a very early age with inverted club-foot on the left side, which caused her to walk on the outer border of the member; other means of remedying the deformity having failed, the operation was performed by cutting across the tendon, and thus dividing both it and the superjacent integument. The operator (by name Lorenz) having reduced the foot to its normal form, retained it in that position by means of a suitable bandage; and in six weeks reunion was complete, and the perfect restoration of the foot was ultimately accomplished. In 1811, Michaëlis published a memoir in which he described a modification of this operation, consisting of a partial division of the same tendon, by which a more ready extension of the remaining portion was admitted of, and, from the cases he relates,‡ was very successful. Michaëlis also operated on the flexor tendons of the leg and fingers; but as he considered immediate extension requisite for the cure of the case,—and the healing of his wounds was generally protracted,—Dr. Little's suggestion is not improbable, viz., that by the force he employed to place the limb in a proper position, the remaining undivided fibres were ruptured. The case related about the same time by Sartorius§ (of the Duchy of Nassau) was anything but encouraging, and his mode of proceeding certainly most barbarous. He exposed the tendo achillis for a considerable distance, and after freely dividing it, flexed the foot on the leg with such violence as to produce a loud cracking sound, doubtless ligamentous rupture at the least: the cure was tedious and eventually accompanied by an anchylosed articulation. Up to this date no attempt seems to have been made to avoid the extensive lesion of superficial soft parts, which different operators had deemed it requisite to make as a preliminary to dividing the tendon. Delpech made the first step towards attaining this desirable object, but his operation was far from complete;

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* For Mr. Whipple's paper and cases, we refer our readers to vol. xx. of the Med Gazette, p. 826, and to an abstract of the same in our Fifth Vol. p. 285.

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* Liber de Articulis.

† Med. und Chir. Bemerkungen. Frankfurt, 1789.

‡ Hufeland's Journal. Band 33.

§ Siebold's Sammlung Chir. Beobachtungen, 1812.

and in consequence of the difficulties attendant upon the cure, he never repeated it. The case related by him* occurred five years after the publication of Michaëlis's memoir. The patient was between six and seven years old, suffering from simple talipes equinus: the first step of the operation consisted in passing a scalpel through both surfaces of integument covering the interspace between the tendo achillis and deep flexor muscles; a bistoury was then substituted, by which the section of the tendon was made from before backwards. A further modification in his plan consisted in allowing union to take place before extension was commenced; indeed for this purpose he maintained as much as possible the approximation of the divided extremities by an appropriate apparatus; and he further recommended that after the extension was completed, the improved position should be preserved until the newly-deposited material had become sufficiently consolidated to afford the requisite support in walking. M. Bouvier† tells us that he saw the subject of this operation at Paris, twenty years afterwards, and testifies to the completeness of the cure. This brings us down to the time of Stromeyer, who, in spite of the early want of support and the many severe criticisms on his reintroduction of this operation, has completely succeeded in establishing it as one of the most useful as well as certain improvements in modern surgery. We have already adverted to the chief peculiarities which characterize Dr. Stromeyer's general mode of operation, viz., the *small* and usually *single* puncture of the skin, the *complete* section of the tendon, and the *delay* in making extension, and have noticed the palliative and mechanical treatment applicable to some cases; we will now take up our subject more in detail, and consider severally and in order, the operative treatment of deformities of the *feet*, *knee* and *hip-joints*, *fingers*, *elbow*, and *neck*.

It is much to be regretted that the profession does not more readily fall into the adoption and employment of a conventional nomenclature, without constantly seeking to improve upon that which exists. It rarely happens that a fresh labourer enters upon any particular subject, whether new or old, without restlessly seeking to impress an extra stamp of novelty upon the results of his investigations, by calling old things by new names, forgetting in this, as in matters of far higher importance and of wider interest, that alterations are not necessarily improvements. We do not, however, consider this remark as applicable to Dr. Little, whose nomenclature we are willing to adopt. Our own compound word "club-foot" has always been, and always may continue to be, in common language, applied generically to all three forms of distortion, viz., the inversion, eversion, and the simple elevation of the heel. Synonymously with this term Dr. Little proposes employing the classical word 'talipes' (hitherto applied to one species only) as a generic term to include all those deformities of the feet produced by contraction of certain muscles, to restrict it to deformities from this source, and to use the terms *varus*, *valgus*, and *equinus* to designate the specific forms of these diseases. The least complex of the deformities alluded to (which are thus classified under three heads) is the *talipes equinus*. This form consists in a simple extension of the foot, by which the heel is elevated, and the subject of the affection rests and walks upon the phalanges, or rather the heads of the metatarsal bones; an angle is thus formed between the phalanges and metatarsus, which varies according to the perpendicularity of the latter, and posterior to which no portion of the plantar surface touches the ground: in the most aggravated cases, the angle is rarely produced beyond a right angle. In this, as indeed in all the forms, the habitual disuse of the limb impedes the development of both hard and soft parts; the bones are usually shorter and more slight than on the healthy side, and the muscles are small and flaccid; and Dr. Little remarks that the whole vitality of the limb is below par. Although the preceding is the least complicated form, the *talipes varus* is the most frequent of occurrence. This variety combines extension with adduction of the foot; and to these two characteristics a third may be added, viz., "a rotation of the foot somewhat analogous to supination of the hand taking place to a greater or less extent, according to the severity of the disease;" the inner edge of the foot is thus raised from the ground, forcing the sufferer to walk on the outer margin only. In the *talipes valgus*, which is comparatively rare, the distortion is of an opposite character; we describe it in Dr. Little's words:

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* Chirurgie Clinique de Montpellier. Tom. i.

† Mémoire, p. 427.

> "It may be regarded as the opposite state to talipes varus, and like it consists of a threefold alteration of the position of the foot, there being partial bending of the ankle, *ab*duction, and a rotation of the foot; but this rotation foot takes place in the opposite direction to that in talipes varus, as in talipes valgus, the external *edge* of the foot is removed from the ground. The rotation in a complete case of talipes valgus is so great that the patient, in the act of walking, does not touch the ground with any part of the sole of the foot, but treads entirely upon the inside of the instep, and upon the malleolus internus. In short, the sole of the foot is directed completely outwards and a little backwards, the ankle is held in a state of semiflexion, the anterior half of the foot (the metatarsus and toes) not touching the ground." (p. 4.)

Although each of these forms frequently exists independently, the *degree* of adduction produces modifications of various kinds between the talipes equinus and varus.

We have already noticed the alteration in bony configuration accompanying these deformities; we shall now make some enquiry as to the muscles which are implicated in the disturbance of the normal relation of the parts affected. This is a point of great practical importance, one indeed on which the successful treatment of the case essentially hinges. With regard to the simple extension of the foot constituting talipes equinus, all our authors agree in referring the elevation of the heel, as one might readily predicate, to a permanent approximation of the origin and insertion of the gastrocnemii muscles. This shortening may, however, arise from various causes; it may be the result of continued tonic spasms, referrible either to lesion of a nervous centre, or to functional derangement of some important organ,* or it may be the consequence of actual loss of structure, as from a large sloughing ulcer, &c.,† or again, the soft parts may contract unnatural adhesions, or the muscles become permanently shortened from the restraint of a protracted unnatural position, or from paralysis of their antagonists. We have indicated in an earlier part of this article that some of these cases may be benefited by a palliative mode of treatment, but all may, and frequently do, require the knife. As section of the tendo achillis alone is required for the restoration of the foot, in most cases of talipes equinus, we may here notice the mode adopted to effect that object by Stromeyer and Little. Having selected "a small, curved, sharp-pointed bistoury, with a concave edge, the cutting part of the blade seven tenths of an inch in length, and the greatest width one tenth of an inch," in order that the external puncture may be as small as practicable, the surgeon, according to Dr. Little, proceeds as follows:

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* The reader may consult, in the Appendix to Dr. Little's work, cases ix. xxvii. xxviii. xxxi. This author states that he has treated one case of non-congenital talipes which was distinctly referrible to the irritation accompanying teething.

† Id. cases x. and xi.

> "The patient being seated, an assistant supports the knee, whilst another, drawing downwards the patient's heel with his left hand, and pressing upwards the toes and front of the foot with his right, produces the necessary tension in the tendon proposed to be divided. The operator, after feeling the outline of the tendon with the left fore-finger and thumb, passes the bistoury through the skin, one or two finger's breadth above the malleolus internus, with one of its sides turned towards the tendon, and the other directed towards the deeper muscles and the tibial vessels and nerves. On being satisfied that the point of the knife has been passed beyond the external edge of the tendon, and has nearly reached the skin of the opposite side, the knife is turned so as to bring the cutting-edge to press against the anterior surface of the tendon, which is then divided by the action of withdrawing the knife from the limb, and commonly, by a single stroke. The complete division of the tendon is known by the immediate cessation of the tense resistance, by hearing a distinct snap, and by feeling, before the knife is wholly withdrawn, that nothing remains undivided except the flaccid integuments. The operation does not occupy a quarter of a minute, and is almost bloodless, as usually not more than a single drop of blood is effused." (p. 30.)

The small size of the wound is particularly and justly insisted upon by all our authors, as an essential and most important improvement upon the earlier mode of operating; and we believe that the want of success of all the operators, down to the time of Delpech inclusive, was attributable to the large extent of external wound, which they considered a requisite part of the operation. In confirmation of this position, Dr. Little states that "he has operated on seventy-three cases of contractures of the ankle and knee-joints, treated by division of various tendons; amongst which there was no instance of the puncture not having immediately united by adhesion." This result he ascribes to the smallness of the wound, and "the non-disturbance of the healing process by precipitate attempts to straighten the limb." (p. 29.) The wound, then, should be allowed to close before extension is commenced; for this purpose two or three days are generally sufficient, during which interval the limb should be laid on its outer side, on a pasteboard splint; and even after that Dr. Stromeyer recommends that the obstacles offered by resisting ligaments and fasciæ should be very gradually conquered, as their shortened condition is no trivial additional impediment to the reduction of the foot. Two apparently trifling but really valuable hints of Dr. Little, in relation to the after mechanical treatment, are worth citing; the one is, "that the skin should be protected from friction and uneven pressure by the application of an elastic cotton-web bandage, and by filling up with wadding those inequalities of the surface of the tarsus which arise, in many instances, from unequal projection of the bones;"

the other is, where the foot is very uneven and tender, "to place an air-cushion between the sole of the foot and the foot-piece of the apparatus, in order to produce an equal distribution of the pressure over the whole sole." It is requisite to continue the use of the extending apparatus, even after the cure is effected, to obviate the natural tendency to contraction.

Many of the foregoing remarks are equally applicable to the two other forms of talipes, more particularly the varus, many cases of which require the same treatment as the equinus, viz. simple division of the tendo achillis; whereas, on the other hand, Dr. Little states that in some of the more obstinate cases of pure talipes equinus he has "found it necessary likewise to divide the tendons of the tibialis posticus and flexor longus pollicis muscles:" of this exception his cases vii. and viii. afford illustrations. The treatment of talipes varus, therefore, consists, as in the form equinus, in the division of the tendons of the opposing muscles, which may be either limited to the gastrocnemii, or may extend, in some of the severer and longer-standing cases, to section of the tendons of the tibialis anticus and posticus, with the extensor and flexor proprius pollicis;† for exemplifications of these the reader may refer to the appendix of Dr. Little's work.* In several of these cases, which are characterized by marked inversion of the foot, the dependence of the deformity in great measure on contraction of the tibialis posticus has been indicated by Dr. Stromeyer; he has, therefore, frequently divided this tendon, and in one case (viii.) this only; although he admits that his more recent experience inclines him to believe that as much benefit may be obtained for the relief of this distortion by the proper application of mechanical means alone as by the operation; and this opinion is confirmed by some of his cases, and his own summing up and inferences at p. 81. In one case (vii.) it was found requisite likewise to divide the plantar aponeurosis.

That muscles which are naturally antagonists of one another occasionally require division in one and the same case, may be better comprehended by reflecting on the altered bearing of their action, resulting from their abnormal course in cases where the deformity is of an aggravated kind. This remark is peculiarly applicable to the muscles of the anterior tibial region, and is referred to by Dr. Little, and delineated after one of his dissections. (p. 12.) For an account of the best mode of dividing the muscles last alluded to, we again turn to the description given by Dr. Little.

> "The division of the tendon of the posterior tibial muscle is in my opinion best accomplished at the distance of two or three fingers' breadth above and behind the internal malleolus. The point of a strong and straight bistoury should be introduced through the skin at the outer edge of the tendon, and passed between it and the tendon of the long flexor of the great toe, directed towards the tibia. As soon as the knife reaches the bone, the handle should be depressed outwardly, and the point carried internally beneath the posterior tibial tendon, and continued outwards, until the surgeon is satisfied that the point has passed beyond the inner edge of the tendon. He may then feel that he has the tendon upon the edge of the knife, when, by a few slight cutting motions, he may divide it without difficulty. No snapping sound, similar to that which follows the division of the tendo achillis, is heard, when the

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* Cases xiv. xviii. xxiii. and xxiv., and the remarks appended to case xvii.

> section of the posterior tibial tendon is accomplished; as the flesby fibres of this muscle take their origin so low towards the malleolus internus, that they prevent the occurrence of any considerable retraction of the superior end of the tendon. The most favorable situations for dividing the tendons of the tibialis anticus and flexor longus pollicis muscles are where the former passes in front of the ankle-joint, and where the latter is felt most prominently in the sole of the foot, in those cases where division is required. . . . . The division should be made from within outwards, in order not to endanger the neighbouring structures. The recoil of these muscles, on their tendons being divided, is distinctly felt and heard." (p. 31.)

In the third form, *talipes valgus*, Dr. Little's experience induces him to refer the deformity chiefly to the obstruction offered by the peronei muscles; it is likewise usually requisite to divide also the tendo achillis (as exemplified in Dr. Little's case xxviii.), and even, strange as it may seem, the tendon of the tibialis anticus, before the foot can be restored to its natural position. The comparative rarity of these cases is evidenced by the fact that Dr. Little has witnessed but two of this form of distortion, whilst he has had presented to him more than one hundred instances of the other two species. The section of the peronei tendons may be effected in the same position, and (where both are the subject of operation) even by the same punctured opening through which the tendo achillis is divided. Dr. Stromeyer states that he has treated this form by section of the tendo achillis only, as in case xxii., related in his work; and again, the succeeding case is an exemplification of the successful employment, at a very early age (three months), of mechanical means without operation, for the cure of talipes valgus: the apparatus consisted of a small boot, with a long spring, adapted so as to operate in a direction opposed to the abnormal eversion of the foot; the cure was completed in a few months.

The notice of this case leads us to remark upon the *age* at which it may be deemed advisable or most desirable to employ the operative treatment in cases of club-foot. The youngest patient amongst Dr. Stromeyer's was a boy of eight months old, in whom the tendo achillis was successfully divided for talipes varus.* A case of spasmodic congenital varus was likewise cured by Dr. Little, in a child, aged twenty months, by section of the same tendon.† Of Mr. Whipple's nine cases,‡ the youngest was fourteen months old. With regard to the other extreme, we may presume that a cure is practicable at any period of life; for Dr. Little narrates a case (xxxiv. p. 258) of "non-congenital distortion from contraction of the gastrocnemii and other muscles on the posterior aspect of the right leg, converted by constant exercise into a deformity resembling, in external appearance, talipes varus, originating from paralysis: cured by division of the tendo achillis, and subsequent mechanical extension, after the distortion had existed *forty-eight* years." The subject of the operation was in his fiftieth year. A similar case is related by Dr. Stromeyer (p. 82), of a female whose deformity had existed since her fourth year, and who was operated on at fifty; but her habits had been comparatively sedentary, and she had employed crutches, by which an aggravation of the distortion was, to a certain extent, prevented. Dr. Little's case is, therefore, the more remarkable of the two. We had almost omitted to observe that this latter gentleman notices a fourth form of talipes, in the production of which the tibialis anticus and extensors of the toes are the active agents. This deformity consists in abnormal *flexion* of the foot, the heel alone touching the ground; and it is therefore opposed to talipes equinus, bearing the same relation to the talipes valgus as the talipes varus and equinus respectively hold to each other. Dr. Little has treated a case of this form by simple extension, but thinks that the tibialis anticus or extensor communis may, in aggravated cases, require division. He has named the deformity *talipes calcaneus.*[*]

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* Case xi. p. 81. † Case xvi. p. 159. ‡ Noticed in our Fifth Vol. p. 235

In closing the subject of deformities of the feet, Dr. Stromeyer devotes a brief chapter to the notice of what he terms "Plattfuss," a word with which our *duck* or *splay-foot* to a certain extent corresponds. This affection, when aggravated in form, becomes both troublesome and painful, and therefore deserves the attention of the surgeon; yet but little heed appears to have been paid either to its pathology or treatment. Dr. Stromeyer remarks that the condition of splay-foot appears clearly to depend upon "atony of the plantar aponeurosis, and the ligaments which bind the different bones to one another, and connect them to the tibia and fibula. "These tissues yield," he adds, "to the superincumbent weight, and thus not only is the arch of the foot annihilated, but the foot also gives way in an outward direction, because the ordinary action of the gastrocnemii and tibialis posticus muscles is so far interfered with by the relaxed and unsteady condition of the articulations, as to produce a drawing or pressing of the tibia inwards." A shrunken state of the limb is rather to be regarded as an effect than as a cause of the affection. Dr. Stromeyer relates four cases of its successful treatment; and, as they vary but little, we will select one as an example of the rest.

> "H. K. ætat. 16, of lymphatic diathesis, whose employment had been for half a year previously in a merchant's office, presented himself with a splay-foot to Dr. Stromeyer, in December, 1831. This affection, which had existed for some months, was palpably the consequence of standing continually behind the counter, on a cold floor. He was in pain when either standing or walking, the foot was bent outwards, and the plantar arch quite destroyed; moreover, distinct fluctuation might be perceived between the bones of the tarsus, and a sensation of heat was communicated to the hand. Twelve leeches were applied, and the foot afterwards bathed with Goulard's lotion. In four days the heat and fluctuation were thus dispersed, and a blister was then applied to the inner border and part of the arch of the foot, and kept open. After the healing of the blister the foot was found to have regained its normal form. The patient was ordered to wear a bandage and lace-boot, by the use of which the cure was rendered complete." (p. 101.)

Dr. Stromeyer adds, that the aid of splints and spring-supports is occasionally desirable in the more obstinate forms of this complaint.

The next division of our subject relates to contractions of the knee-joint, and that which Dr. Stromeyer terms "false anchylosis" of this articulation. For the materials whence to compile a summary of the treatment of this affection, as well as of those which follow, we shall be principally indebted to Dr. Stromeyer's work, as both Dr. Little and M. Bouvier take leave of us here. Many of our preceding *general* remarks may be equally applied to the subsequent complaints which we have to notice; therefore, we trust to keep our observations within the limit of tediousness, by allotting a comparatively brief space to the consideration of the individual sections which compose the remainder of our subject.

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[*] This form of talipes is also noticed and described by Mr. Whipple, in his paper already referred to. See Med. Gazette, vol. xx. p. 826.

Knee-joint contractions are more frequently the result of disorganization consequent on chronic or long-standing inflammation and suppuration, than dependent, as in the instances we have already discussed, on a "dynamic influence," such as muscular spasm. Be that, however, as it may, the operation of dividing the hamstring muscles is neither less essential, nor has it proved less efficacious in the cure of this affection. These cases are sometimes complicated with contraction of the feet; and Dr. Stromeyer informs us that he has seen a "sort of chronic tetanus," affecting not only all the limbs, but even extending to the muscles of the face. In a case where both knees and ankles were implicated, M. Duval, of Paris, divided the tendons of the gastrocnemii and hamstring muscles on either side, and the result was satisfactory; but Dr. Stromeyer does not express himself as favorably disposed to this wholesale way of operating, but rather recommends the employment of palliative measures, such as perseverance in warm-bathing, &c. Cases xxviii., xxix. and xxx. of Dr. Stromeyer's book are exemplifications of the beneficial application of the operation to the radical cure of contracted knees. The first is termed "false anchylosis" of the knee, and occurred in a lad, twenty-one years of age, whose complaint originated in scrofulous inflammation of the joint four years previously. The articulation formed an angle of nearly 110 degrees, and was only slightly moveable by the employment of considerable force. An extending apparatus (delineated in Tab. v. fig. 1 of the work,) was employed advantageously up to a certain point; but an attempt to push its use further, produced so much local and constitutional disturbance, as rendered it necessary to give up the hope of obtaining any permanent benefit from its continuance. An operation was therefore had recourse to: the tendon of the biceps cruris was first divided, with the same sort of knife. and in the same simple way as section of the tendo achillis is performed; the extending apparatus was then reapplied, but it was found indispensable, at the end of a fortnight, to divide in like manner the semi-membranosus and semi-tendinosus muscles. After the lapse of three months the limb was perfectly straight. In case xxix., the nature and cause of which were similar to the former, it was found necessary to divide only the inner hamstring muscles, and the result was equally favorable. Case xxx. was somewhat more complicated, the contraction. at an acute angle, having succeeded scrofulous suppuration of the joint, which lasted for eight years. The fascia was, in this instance, likewise implicated as a cause of the abnormal position of the articulation; it was, therefore, found requisite to divide portions of it, more especially near to the intermuscular septa, as well as all the hamstring muscles; and this was effected at various operations, the apparatus being applied in the intervals between each: the treatment proved ultimately successful. The remaining cases, illustrative of these affections, were either benefited or cured without operation, as by mechanical extension, friction, warm bathing, &c. We may notice, by the way, that Dr. Little relates one case at the end of his work, to exemplify the operation of dividing the hamstring muscles; the instance was analogous to those we have cited, and its issue moderately successful.

Our space will not allow us to analyze the half-dozen pages which Dr. Stromeyer gives to the consideration of *hip-joint* disease and the resulting deformities, especially as it is a little foreign to the object of our present article; we must, however, find room for a brief sketch of one case, in which the pectineus and sartorius muscles were divided for contraction at this articulation. The patient, who was a girl of nine years of age, was placed under Dr. Stromeyer's care in September, 1837. A twelvemonth previously she had had measles, and going out earlier than was prudent, she was almost immediately attacked with disease of the hip-joint, accompanied by pain and contraction in the knee. Active antiphlogistic measures, to a certain extent, arrested the symptoms; but flexion from contracted muscles of the right hip-joint was ultimately so decided that the thigh touched the abdomen. Attempt at extension aggravated the pain; and as an apparatus had been for some time worn with very little improvement, it was discontinued, together with the other palliative measures, and an operation was decided on. We may premise the description of this, by observing that attempts to *extend* the limb clearly indicated that contraction of the pectineus and sartorius muscles, especially the former, was the obstacle that interfered and prevented it.

> "On the 5th of March, 1838, the division of the pectineus muscle was effected. The patient was placed on a sofa, and whilst one assistant fixed the pelvis, a second extended the bent extremity, so as to put the pectineus on the stretch; the left index-finger being then placed, hook-fashion, behind the outer border of this muscle, about one inch and a half from its origin, a well-curved bistoury was made to pierce the skin and its upper half. This portion was then first divided beneath the integument, and a similar operation then performed on its under and inner half, so that only a few drops of blood escaped by the external aperture. To accomplish the section of the sartorius, the knee was adducted, so as to render the muscle tense; it was then seized between the finger and thumb, and divided beneath the skin at a distance of about two inches and a half from its origin. The extremities retracted about an inch. After the completion of both steps of this operation, the thigh was gently straightened and without any pain; an extending apparatus was then adjusted and worn for a fortnight, at the end of which time the limb exhibited no tendency to recontraction." (p. 120.)

This case was still under treatment when the report was made; but Dr. S. anticipated perfect restoration of the limb from careful exercise and warm bathing during the ensuing summer.

Dr. Stromeyer next informs us that he has relieved *permanent flexion of the fingers*, by removal of hardened and contracted cicatrix, or division of the flexor tendons. Case xlii. illustrates the successful adoption of the former treatment: the subject was a child, five years old, who had burnt the side of her hand; and the little finger was bound down to the palm by an indurated cicatrix; this portion was excised and the hand extended on a splint applied to the dorsum: the wound healed in ten days, but after a time the tendency to contract again declared itself, and it was necessary to resume the employment of the extending apparatus, which was continued at night for some time before the cure was completed. Of the latter form of cure (viz. by section of tendon), case xliv. is an example; this we shall quote, because we think it valuable as a precedent, the imitation of which may at any rate be attempted before a case is given up as incurable, or a finger sacrificed as not only useless but an incumbrance.

"A butcher, forty years of age, had, fifteen months previously, abscess of the fore-finger in the right hand, which required several openings: suppuration took place also on the dorsal aspect of the little finger. After two months treatment the parts healed, but the thumb, fore and little fingers, remained permanently flexed; and it was sufficiently evident, when extension was attempted, that the contraction of the flexor muscles was the source of resistance. The thumb was at once righted when its long flexor tendon was divided beneath the skin opposite to the first phalanx: but the superficial and deep flexor tendons of the index and little fingers being completely separated from one another, it was found necessary, after section of the former, to turn the knife towards the phalanx in order to divide the latter. After this double operation the fingers yielded to extension: the little finger was placed for a short time on a splint, and after employing passive motion for a fortnight, the patient was enabled to return to his business." (p. 126.)

Dr. Stromeyer notices also the permanent flexion of the little finger from contraction of the palmar fascia, a condition occasionally met with in individuals, such as those afflicted with club-foot for instance, who are accustomed to walk with and lean heavily on a stick; for the removal of this form of contraction, our author has found it requisite to divide integument as well as fascia. For the cure of *permanently flexed elbow-joint,* Dr. Stromeyer has never met with a case requiring division of the tendon of the biceps, an operation which is recorded as having been several times performed; he has, however, successfully treated many cases, which, he observes, are most commonly the consequence of inflammation of the articulation, by the employment of an extending apparatus, similar to that before alluded to as applicable to the knee-joint. Should section of the biceps cubiti tendon be desirable, the operation would, with the requisite precaution of avoiding the neighbouring artery and nerve, be simple and without hazard.

The affection denominated "*wry-neck*" is one of considerable physiological interest and practical importance; and the operation of dividing the sterno-mastoid muscle for its cure, is one of the earliest belonging to that class of operations of which we are now treating. As long since as 1670, Roonhuysen* divided this muscle together with the superjacent integument; and more recently the operation was performed by Tulpius and Sharp, the latter of whom gives a description of it in his "Treatise on the Operations of Surgery."† Boyer, Dupuytren, Chelius, Dieffenbach, Brodie, and many other surgeons, have adopted this radical means of cure successfully. It appears doubtful whether the modification of merely piercing the integument and dividing the muscle, in the same way as recommended for division of the tendo achillis, is due to Dupuytren or Dieffenbach; but be that as it may, it is doubtless the most desirable mode of operating, where it is practicable. The best spot to select for the section is, as Dr. Stromeyer remarks, "that which can be isolated with security, and that is at the tendinous portion near to the external extremity, where also the local retraction is comparatively trifling, as is the case in division of the gastrocnemii tendon; whereas section of the fleshy part of the muscle tends to the production of greater swelling about the divided extremities." (p. 129.) In severe cases,

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* Gerardi Blasii, Observ. Med. rarior. Amstel. 1700. Para 2.

† London. 1740. Chap. xxxv.

Dr. Stromeyer admits the propriety of commencing extension immediately after the operation, as the tendency to retraction is in some instances so great, that directly reunion has taken place, the abnormal flexion of the neck recurs; especially where the disease has been of long standing, and the energy of the constitution is much impaired. Mechanical extension and stimulating embrocations may then be serviceable. "A very singular circumstance," remarks Dr. Stromeyer, "and one which has not, within my knowledge, been heretofore noticed, is the coincidence of wry-neck with abnormal position of the fœtus in the uterus, so that either a buttock presentation results, or it is found requisite to turn in delivering." (p. 131.) Several of these cases have been made known to Dr. Stromeyer by his friends, some of which he afterwards relates, where wry-neck appeared to be the consequence of protracted and difficult labour, requiring the assistance of the forceps and considerable force for the extraction of the child. Dr. S. does not mean to ascribe the above condition to misplacement of the fœtus in all cases, but believes that, like other similar affections, it may result from inflammation, or be the consequence of primary irritation in the cervical portion of the medulla spinalis. The latter explanation would, indeed, appear to be the most rational mode of accounting for this sudden spasmodic contraction of a muscle which occasionally occurs in healthy adults, from an overstrained exertion, a condition to which other muscles are obnoxious besides the sterno-mastoid, as the biceps cubiti for example. Tonics, sea-bathing, &c. aid, to a certain extent, in relieving some forms of this painful complaint, for painful it sometimes is as well as troublesome; but both medicinal and mechanical treatment generally prove useless, except in cases where the affection is of recent date, and is distinctly traceable to functional disturbance of some important organ, such as the uterus; in support of which opinion we may cite the authority of Travers,* Brodie,† and others. The operation, although not quite so simple as the section of some of the tendons to which we have referred, is, nevertheless, neither difficult nor perilous. It may be performed, according to Dr. Stromeyer, in two ways, either from without inwards or from within outwards, (i. e. as regards the superficies of the muscle, not the median line of the body;) the former section is only to have the preference where the muscle stands boldly out in relief; it would otherwise be hazardous. We will cite an exemplification of each mode of operating, and for the former we select case xlv. The patient was a lad, eight years old, who had been for five years the subject of wry-neck, probably the result of indurated tonsils and the irritating treatment (locally) employed for their reduction. The sternal portion only of the right sterno-mastoid was shortened by one inch and a half. Mechanical extension had been fruitlessly employed for more than a year. The operation was thus accomplished:

> "An assistant depressed the right shoulder and drew the head towards the opposite side, whereby the muscle was put strongly on the stretch. I now raised a fold of integument of a finger's breadth over the muscle at its attachment to the sternum, and pierced it at its base.‡ After two thirds of the blade had been made to perforate

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* A further Enquiry concerning Constitutional Irritation, p. 283.

† On Local Diseases of the Nervous System.

‡ Reference is here made to an engraving representing the form of a knife employed, which was long and bistoury-shaped, but with the *convex* edge cutting.

the skin, the section was effected and readily recognized by the cracking noise which accompanied the separation of the divided ends; and the opposite corresponding muscle being now allowed full play, was immediately brought into action, and drew the head nearly straight. In this posture the head remained for a few seconds only and then resumed even a more bent position than before the operation. Three days subsequently the extending apparatus[*] was affixed, and the cure was completed in four weeks." (p. 133.)

The severer forms of wry-neck are not, however, solely dependent on or curable by division of the sterno-mastoid muscle; the clavicular portion of the trapezius is sometimes implicated, and may, therefore, also require to be divided, in order to effect a radical cure of the complaint. Such was the case in the instance we are about to cite in illustration of the other mode of operation, viz., from behind forwards. As the case is too long to quote verbatim, we select the more important passages only.

"Miss N. N., about thirty years of age, was affected with *habitual spasm* of the sterno-mastoid muscle, which had existed in a modified condition for some time, but had during the last two years assumed a more aggravated form. Having resisted all palliative treatment, the operation of dividing the sterno-mastoid was decided on. As it was supposed that the sternal portion alone was the seat of spasm, that part of the muscle was divided in the same manner as in the former case; but after the lapse of a month, it was deemed requisite to repeat the operation on the clavicular portion, which was accordingly done from behind forwards. A similar shaped knife was employed for this purpose, but with its *concave* edge cutting. The portion to be divided was seized between the finger and thumb of the left hand, and the blade was passed beneath the raised muscle, and the section accomplished in withdrawing the former, without further wound of the integument than the one point of perforation. Although the patient was much benefited by this second operation, a third was necessary to complete the cure; this was performed four months subsequently, and consisted in dividing in the same manner the anterior border of the trapezius muscle." (p. 137.)

Dr. Lehmann, of Torgan, has successfully treated a similar affection by section of the sterno-mastoid of the right side. The case in question was congenital, and had resisted the mechanical treatment. After section, the divided extremities separated for two fingers' breadth; the wound healed in two days, but considerable pain all along the course of the muscle lasted for three days; in twelve the patient was well.[†]

The remaining four cases which conclude Dr. Stromeyer's volume are related to illustrate the treatment of spasm-affected muscles by medicinal means. The first (case li.) was "spasm of the sterno-mastoid and scaleni muscles of one side;" the second, third, and fourth, (cases lii. liii. liv.,) "wry-neck, consequent on inflammation of the cervical vertebræ;" and lastly (case lv.), "retraction of the muscles of the neck following inflammation of both cervical and dorsal vertebræ." The first case was much relieved by large doses of stramonium and subsequently of opium, but the patient ultimately died in great suffering, and the only morbid appearance that presented itself was abnormal firmness of the brain and spinal cord. The other cases yielded to the exhibition of mild mercurial doses internally, and the use of mercurial friction, with leeches and blisters: the last case also required the employment of a moxa.

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[*] For a perfect comprehension of this somewhat elaborate and complicated apparatus we must refer our readers to the engraving in Tab. vii of Dr. Stromeyer's work. The patient is placed horizontally on a couch, and the body is fixed by a girth round the waist and straps extending thence to the bed-foot: the shoulders are also fixed. The head is then made to repose on the sound side, and a broad strap or collar is placed so as to catch the chin on the opposite side, whence proceed straps which pass backwards and are made fast, through the medium of a ring, to the extremity of a short, curved, metal rod, springing vertically from the head of the bed, so that its summit extends a little above the head of the patient.

[†] Berlin. Medicinisch Zeitung. No. 2. Jan. 1839.

Our subject is now nearly brought to a close; indeed, we have only to add, in a few words, what we trust to have already rendered almost superfluous, a summary of the relative merits of the works, the contents of which we have been analyzing. The order and arrangement of matter adopted by Drs. Stromeyer and Little are very similar, and certainly present many advantages to the reader who may wish to study his subject, without the interruption offered by extended illustrations of each form of disease and its appropriate treatment; we allude to the separation of the cases from the body of the treatise. Dr. Little is in most points a disciple and close follower of Dr. Stromeyer; the principal merit in the labours of the former consisting, as we have already noticed, in his careful and commendable researches into the anatomy of distorted feet. The drawings which are interspersed throughout Dr. Little's work, exhibiting the relative appearance of the various forms of talipes, both before and after operation, are cleverly executed, and form a very valuable addition to his treatise. We hope, however, that when Dr. Little publishes, as we trust and expect he will ere long, a more comprehensive second edition, he will see the advantage of giving his cases a little more concisely; we even think that his volume would not lose much of its practical value by the entire omission of some of the cases, the lengthened details of which unnecessarily increase its bulk. The majority of the cases are, however, interesting exemplifications of the principles laid down, and we therefore commend them, together with the remarks appended, to the careful perusal of our readers.

The greater portion of Stromeyer's cases, though even more numerous, are given more succinctly. As we have already seen, the work of our German author comprises a larger field, and is, therefore, more complete as a systematic treatise on the subject we have been reviewing.

Bouvier's memoir, which is limited to the discussion and treatment of distortions of the feet, presents but little, calling for any particular comment. His claims to improvement in the operation are trivial: we have shown that the utility of employing *immediate* extension, a principle which our French author strongly advocates, is at the least questionable; while his fear lest the intervening new deposit should, after a few days, become too solid and resisting to yield, is without foundation.

Before concluding, we must say one word in favour of Mr. Whipple, whose merits ought not to be forgotten in our review of the more extended researches and labours of others. The paper of this gentleman, read before the Medico-Chirurgical Society, as well as his subsequent communication to the Medical Gazette, containing the detail of several cases, exhibit, without any assumption of merit on his part, a practical knowledge of his subject, both in a pathological and surgical view, which, taken in conjunction with his unquestionable claim to priority in the introduction of the operation amongst us, entitle him to the thanks and acknowledgments of the profession generally.

Art. VI.

Recherches Medico-physiologiques sur l'Electricité Animale. Par J. F. Coudret, m.d., &c.—*Paris*, 1837. 8vo, pp. 496. *Plates.*
Medico-physiological Reseurches upon Animal Electricity. By J. F. Coudret, m.d., &c.—*Paris*, 1837.

The subject of organic electricity, or of electricity in connexion with organized bodies, may be considered in two points of view: 1st, as it relates to the influence of electricity considered as a purely physical agent upon organized living bodies; and 2dly, as respects the electric force generated within or manifested by the organized structure itself. These two heads of enquiry are, to a certain extent at least, inseparable from each other; and the treatise before us would have been more satisfactory, as well as conceived in a more philosophical spirit, had it comprehended both branches of the subject, instead of being limited to the latter alone. The object of M. Coudret seems to have been partly to make known a new method of applying electricity as a therapeutic agent—for such in fact, allowing full force to his statements, is the correct exposition of the process adopted—and partly to develope the principles upon which, as he thinks, this remedial application rests. That he is disposed to extend these principles to a very high degree of generalization may be inferred from the motto prefixed to the *Considérations générales*, which, with singular näiveté, he quotes from himself: "*Si le sang est le véritable aliment de la vie, le fluide électrique en est le premier moteur.*" But, however he may be disposed to felicitate himself upon the announcement of this law, the readers of the works of Abernethy and Wilson Philip will scarcely recognize M. Coudret as the founder of the electro-vital physiology. The results of previous investigations, which appear to M. Coudret as the more essential to be borne in mind, before entering upon those which are especially his own, are, "1st, that the nerves are actual organic conductors; 2dly, that electricity is to be considered as the motive principle or agent thereof; 3dly, that they present, as in the case of galvanic apparatus, two entirely different and distinct orders of currents; lastly, that one of these currents, appropriated to the functions of sensibility and intelligence, passes from the internal and external senses to the brain; the other, devoted to the nutritive and locomotive functions, is directed, on the contrary, from the brain or, if you will, from the spinal marrow to the several parts of the muscular system and of the vast sanguineo-capillary apparatus." (p. 8.) Passing over the first, third, and fourth of these propositions, the third of which we may, however, remark is unnecessarily complicated with the introduction of the loose analogy attempted to be inferred between the nervous system and a galvanic apparatus, we proceed to the examination of the second—that electricity is to be considered as the motive principle or agent of the nerves.

After stating the modern discoveries of the separate functions of the nerves of sensation and the nerves of motion, which he attributes entirely to M. Magendie, without the slightest reference to Sir Charles Bell or the other numerous cultivators of the same field of experimental research, M. Coudret brings forward his fundamental and essential facts, upon which, as he conceives, these propositions, and more especially the second of them, are established. These facts are, 1st, the well-known experiment of Galvani on the frog; 2d, the excitement of muscular motions by means of a galvanic current passed through the nerves, by which the muscles are respectively supplied; 3d, the production of muscular motion, in animals recently deprived of life, by mechanical irritation of the spinal marrow or the trunk of a motor nerve; and, lastly, the effects produced on aliment received into the stomach by division of the pneumogastric nerves, and the subsequent reestablishment of the connexion with the brain by apposition of the divided extremities of the nerves or by the interposition of a small metallic plate. This experiment has been modified by the introduction of a non-conductor between the divided ends of the nerves, in which case the food remained undigested as before, though when a metallic plate was used as the means of communication, the process of digestion went on with little or no interruption. Here again we have no mention made of Dr. Wilson Philip, to whom we owe the experiment in its original form; we must, however, do M. Coudret the justice to believe that the omission is entirely the effect of ignorance, since, in his relation of the experiment, he omits the only circumstance which renders it of any avail for the purposes for which it is referred to. The effects resulting from the renewed contact of the divided ends of the nerves only show that the nervous connexion is reestablished, and the effects of the interposition of the metallic plate merely tend to prove that the nervous influence, whatever that may be, is capable of being transmitted along the surface or through the substance of the metal, not that it is identical with or analogous to any other agent of which metals are conductors. Dr. Philip's experiment of the substitution of a galvanic current for the nervous influence and the effects thereof upon the aliment contained in the stomach are much more to the point, as they lead to the establishment of an analogy, at least, between the nervous and galvanic forces. We cannot, however, dwell further upon these points; nor need we quote the very hasty, unauthorized, and erroneous conclusions deduced from them by M. Coudret. The last of these is thus enunciated: "If in the question," he observes, "before us, as in the analysis of all possible phenomena, we must rigorously judge of the identity of the causes by the identity of the effects, we cannot henceforth doubt that the nervous agent and the electric agent are two principles perfectly identical." (p. 15.)

M. Coudret briefly alludes to the recent researches of MM. Dutrochet and Donné upon animal electricity, from which these gentlemen are disposed to conclude the existence of electric currents within the organs of the body; but he makes no mention of the many curious particulars to be found in ancient and foreign authors. The development of electricity upon the surface of certain animal bodies in consequence of friction, is a well-ascertained fact, and has probably given rise to some of the fictions of classical writers. Instances of the emission of sparks on stroking or combing the hair, and on friction of the surface of the body by the clothes, are mentioned by Cardan, Scaliger, Faber, Bartholinus, Fortunius Licetus, Gesner, Beccaria, &c. Ezekiel di Castro relates the case of a lady of Verona, resembling, in several respects, the singular one quoted from an American Journal in a recent number of this Review, (*Br. and For. Med. Rev.*, Vol. VI., p. 249.) "As often as she touched her body, even in a slight manner, with a linen cloth, it emitted sparks in great abundance, which could be perceived by every person standing near her, and were attended with considerable noise. Her maids were often deceived by this phenomenon, and believed that they had, through carelessness, dropped some coals between the sheets, as she always caused her bed to be warmed in winter, at which time the sparks were most abundant and strongest.*" These are, obviously, instances of the development of electricity without the body; and, accordingly, Saussure, who has investigated the subject of animal electricity, found that he could obtain no indications of electricity when undressed; but Hemmer, who instituted a lengthened series of experiments, both upon himself and upon several other individuals, has arrived at a different conclusion; and from these experiments it would appear that the development of electricity, although varying in intensity in different individuals, and in the same individual at different times, is yet a general property of the animal economy. For an account of these investigations we must refer to the sixth volume of the Transactions of the Electoral Society of Manheim and to the Philosophical Magazine for 1799.

To return to M. Coudret. We find him stating the following propositions as the groundwork of his subsequent investigations: first, "that every painful or inflamed part disengages a notable quantity of electricity;" and secondly, "that every means fitted to withdraw or to neutralize directly this fluid, produces the most salutary and most evident antiphlogistic and sedative effects." It appears that in January, 1833, M. Fozembas, finding himself affected for several days with severe erysipelas of the face, which he had in vain attempted to subdue by the usual means, bethought himself of having recourse to electricity. "Long entertaining the suspicion," says M. Coudret, "with us and with other physiologists of our epoch, that electricity could scarcely be otherwise than concerned in the development of similar phenomena, he resolved to make some experiments with the view of satisfying himself upon this point. Assuming that electricity is in effect one of the essential elements of vital activity, and that by its development in excess it is the principal cause of the morbid phenomena which constitute inflammation, the best means of promoting a return to the state of health should be to seek the establishment, as quickly as possible, of the natural equilibrium of the animal electricity." These means are stated to be bleeding, repose, and regulated diet, which are conceived to act as either directly or indirectly impeding the generation of electricity, and more especially the withdrawing or direct neutralization of the electricity from the inflamed part, whether by means of metallic points in communication with the ground, by topical humid applications, or by immersion of the affected part in a warm bath. All these measures are stated to have been had recourse to without benefit, with the exception of immersion in the bath, which, the face being the part affected, could not be had recourse to, and the application of the metallic points. Having contrived a suitable apparatus, which we shall presently describe, M. Fozembas proceeded to subject himself to a trial of this method. The metallic points of the apparatus were placed opposite to, but not in immediate contact with, the most painful and burning parts of the face.

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* Castro, De igne lambente.

> "After some minutes' application he thought he perceived the commencement of an amelioration; but what made him soon comprehend that this opinion was not an illusion of the imagination was that, at the end of some hours, he could, for the first time, open the eyelids a little and support the impression of light, which until then he had been unable to do. Encouraged by the result of this first trial, he repeated, many times in the day, the application of the apparatus, with the precaution of causing it to act in succession upon the most irritated parts of the face. The effect was so salutary, so sensible, and so quick, that the patient and those by whom he was surrounded could scarcely credit it. Each time that the apparatus was applied for an hour to the same part, it appeared neither so red nor so tense as before, and the heat as well as the sensibility were very much lessened. At length, under the influence of this new method, the irritation and pain diminished with extreme rapidity, and not only was their rapid decrease not injurious to the patient in giving rise to some internal repercussion, but, on the contrary, from this moment the general condition continued to improve, and in a few days the restoration to health became complete." (p. 26.)

The apparatus employed in the case of M. Fozembas, to which the name of the *medical electromotor* is given, is described as follows: It consists of a box, which must be of glass or of some other isolating material, varying in size and form according to the configuration of the surface on which it is to be applied and the amount of effect intended to be produced. Deeply placed within the box is a continuous double metallic surface, of which the lower surface only is visible. This lower is set (hérissée) with a great number of very sharp points of steel. A small opening in the summit of the box affords a passage to a conducting cord several feet in length, and intended to effect a communication between the upper metallic surface and the ground. The base of the box by which it should be in connexion with the affected parts, projects a little beyond the steel points, in order that the skin may be constantly protected from the action of these points; and that this protection may be still more effective, a small network of very fine silk is extended across the framework of the base. The apparatus is attached by one or more bands of silk, intended to retain the instrument in exact apposition with the affected parts.

The mode of action of this instrument, admitting the assumption of MM. Fozembas and Coudret that a disturbance of the electric equilibrium is the principal cause of the morbid phenomena which constitutes inflammation, is sufficiently evident, although it is not equally clear that the construction of it above described is either the most philosophical or the most convenient which could have been devised. A much more important consideration here is the ascertaining of the actual existence of the alleged excess of electricity in inflammation. The method adopted by M. Coudret to prove this point is by the application of one of his electromotors upon the inflamed part, the steel points with which the under surface of the instrument is set being brought as near as possible to the skin without actual contact. An electric condenser of Volta is placed at a little distance from the patient and the conductor of the electromotor, made as short as possible and terminated by a little ball of sealing-wax, brought into contact with the superior plate of the condenser, whilst the inferior plate of the same apparatus is placed in communication with the ground. In a few minutes, interrupting the communication of the condenser with the ground and preserving at the same time the connexion of its superior plate with the electromotor, the two plates of the condenser are to be carefully separated; at the moment of this separation, the gold leaves of an electrometer which is connected with the inferior plate of the condensing apparatus will diverge, "at least," says M. Coudret, "provided no accidental cause should arise to disturb the operation and neutralize its results." The operation is one of considerable nicety and liable to numerous sources of fallacy, not the least of which is the generation of a sufficient amount of electricity to cause the divergence of the gold leaves by the friction of the plates of the condenser upon each other in the act of separation. M. Biot remarks that great attention also should be paid to avoid friction in bringing the plates of the condenser into contact, since the friction will develope a certain portion of electricity in the plates themselves which adheres strongly to them and may subsequently occasion errors in delicate experiments. Another source of fallacy is that the connexion between the inferior plate of the condenser and the ground is established by means of an assistant who places one of his fingers, "slightly moistened with saliva," in immediate contact with the lower surface of this plate. "The assistant," says M. Coudret, "destined thus to perform the functions of a conductor should be in good health, have his hands clean, and stand upon a perfect conductor of electricity, consequently by no means waxed," as is generally the case with the floors and much of the furniture in French houses; and again, "it has been demonstrated to us by a great number of observations, that if the hands of the assistant thus performing the office of a conductor are hot and dry, he may himself furnish electricity to the condenser instead of withdrawing it, and consequently vitiate the experiment." In such cases an ordinary metallic conductor is recommended to be substituted, but it must be observed that, as far as the theory is concerned, a doubt is thus thrown over the whole of M. Coudret's experiments and observations, since we have no means of ascertaining in what proportion of them these extra precautions were had recourse to. It is perhaps scarcely necessary, keeping these considerations in view, to relate any of the experiments; but as the second series narrated by M. Coudret was performed in the presence of M. Piorry, who, it may be presumed, would have pointed out any notable source of fallacy, it is but justice towards the author to mention the most striking of them, although even here we notice that the communication of the electric condenser with the ground was kept up "au moyen du doigt mouillé d'un aide." Exp. 7. Erysipelas of the face: electromotor applied upon the right cheek and placed in contact with Volta's electrometer. At the end of two minutes and a half, separation of the gold leaves to the extent of a full inch. Exp. 8. The same result from the left cheek; venesection immediately performed and the electromotor reapplied. At the expiration of two minutes and a half, separation of the gold leaves to the extent of five lines only. Exp. 10. Electromotor applied to the epigastrium of M. Grossetête, a student of medicine enjoying good health, but having the epigastric region very hot. The gold leaves of the electrometer separated to the extent of four lines.

Exp. 11. Performed on M. Belonius, also a medical pupil, possessed, as he stated, of strong magnetic powers (d'une force magnétique prononcée.) Electromotor applied for two minutes on the right hand, which was excessively dry but of a temperature but slightly elevated. Separation of the leaves to nearly four lines. On the epigastrium of the same subject the separation was nearly the same as on the hand, though the temperature seemed rather less, at least to the touch.

From a careful examination of the experiments above referred to, as well as others related by M. Coudret, it does not appear to us, setting aside the obvious sources of fallacy to which we have already alluded, that the effects upon the electrometer are necessarily to be attributed to the generation or accumulation of electricity in inflamed parts; or, indeed, that they were at all connected with a purely electric condition, even of the mere surface of the body. The recent researches into thermo-electricity would seem to afford sufficient grounds for attributing the electric effects observed to the induction of electricity by heat alone. M. Becquerel conceives that "whenever a particle of a metal is heated, part of the neutral electric fluid which is attached to it is decomposed, the vitreous fluid being retained, and the resinous driven off and passing into the adjoining particles. In proportion as the heat extends by communication from particle to particle, similar effects take place in each of those that are acquiring heat, and the contrary in those that are losing it. Thus the first effect is only to produce an oscillatory movement of the electric fluid between the adjacent particles; but if the source of heat be permanent, the retrograde movements are prevented, and a continued current takes place."* In the second volume of the Annales de Chimie et du Physique is a paper by M. Dessaignes, in which several curious experiments are related, also tending to prove the development of electricity in metallic bodies, merely by change of temperature, some of which are remarkably analogous to the recent investigations connected with thermo-electricity. In confirmation of the view here advanced, we may state that a similar idea seems to have suggested itself to M. Piorry; for we find M. Coudret "wishing to show to M. Piorry, first, that the electromotor employed to prove the presence of animal electricity, did not of itself develope electricity; secondly, that the action of the temperature of the human body upon this apparatus could not be considered as the cause of the phenomenon:" and then proceeding to perform certain experiments upon other heated bodies, which, however, do not appear to us by any means calculated to meet the objection.

Assuming the explanation which we have here given to be correct, the electromotor of M. Coudret is in fact a thermo-electric apparatus, and being placed in connexion with the heated surface of the human body, induction of electricity by heat ensues. The electricity thus developed is conducted to the condenser, and, according to the preceding statement of the views of M. Becquerel, should be of the resinous or negative kind. Now this is precisely what M. Coudret found to be the case: "everything,' he says, "leads us to consider as certain that the electric fluid condensed in our inflamed tissues is always negative;" or, in other words, that the electricity manifested by the electrometer in connexion with an electromotor placed over inflamed parts is in all cases negative. According to these views then, the electricity is generated in the apparatus itself by means of heat, the negative fluid being conveyed off towards the ground, and, if any interchange takes place between the surface of the body and the instrument, the vitreous or positive fluid being communicated to the part over which the instrument is applied, affording thus, as we stated at the commencement of this article, a new method of applying electricity, as a therapeutic agent.

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* Report on Thermo-electricity. By Professor Cumming. Proceedings of British Association for 1832.

One other observation may be made respecting what may be termed the physiological effects of electricity before turning our attention to the practical part of the subject. "In a vast number of cases," says M. Coudret, "but especially when the instrument is applied to the head, a general moisture arises, and at the same time a drowsiness or sleep more or less profound; in some circumstances this moisture does not extend much beyond the small space occupied by the instrument and is not accompanied by sleep." How far the drowsiness or sleep here alluded to is the effect of the application of the remedy, or whether it is to be attributed to a strong impulse on the imagination of the patient is perhaps questionable, but its analogy with the coma developed in the course of the manipulations of animal magnetism cannot fail to suggest itself. The moisture or perspiration, whether general or local, is no more than what has been noticed in the communication of electricity by other modes. The Abbé Nollet found, by correct statical experiments, that an electric current communicated to a living animal for a considerable time greatly increased the amount of perspiration; and Van Marum, Read, and others have noticed the same. From the consideration of these and other facts, we are led to conclude that the communication of electricity in the mode recommended by M. Coudret, tends to increase the capillary circulation, and therefore that in accordance with the views of the intimate nature of inflammation entertained by Dr. Hastings* and others, it enables the capillary vessels to unload themselves of their contents, thus relieving that state of inaction and consequent congestion, which, according to these authorities, constitutes the proximate cause or rather the very essence of inflammation. Upon these principles we shall have no difficulty in understanding and accounting for the therapeutical effects of M. Coudret's method of applying this remedial agent.

A considerable portion of the treatise before us is occupied with the narration of cases of ophthalmia, erysipelas, cephalalgia, hemierania, and other cerebral affections, catamenial disorders, rheumatic, neuralgic, and other painful affections, &c., in the greater number of which the electromotor is extolled as a heroic remedy. It appears, however, to occupy in these cases precisely the place which stimulant applications to the eye do in certain states of ophthalmia, as the subjoined note of the author on the case of M. Fozembas sufficiently testifies. The rapidity of the cure in this case was owing to the patient having been prepared for the remedy by the debilitating means and regimen previously employed. "*In every case where sanguineous plethora exists, it must in the first instance be removed, either by bleeding or starvation* (*diète sévère*); as soon as the irritation becomes merely local, the electromotor produces the most remarkable effects." We had marked one or two of the cases for quotation, but as they are mostly similar in their general features to that of M. Fozembas, already noticed, we have the less hesitation in passing them over. The concluding pages, amounting to about one fifth of the whole work, are occupied with hypothetical considerations founded on the electro-medical system. They relate to the classification of medicines upon electro-medical principles, and embrace certain views in connexion with the same upon sensibility and sensation, on the nature and origin of cholera, on temperaments, aliment and drinks, atmospheric air, clothing, &c.

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* Treatise on Inflammation of the Mucous Membrane of the Lungs.

ART. VII.

1. *Handwörterbuch der gesammten Chirurgie und Augenheilkunde.* Herausgegeben von ERNST BLASIUS, Professor der Chirurgie an der Universität zu Halle, &c. Erste Band. Erste Hälfte. — *Berlin,* 1836. 8vo.

Dictionary of General and Ophthalmic Surgery. Edited by Dr. ERNEST BLASIUS, Professor of Surgery at Halle, &c. Vol. I. Part I.— *Berlin,* 1836. (Art. *Aneurism.*)

2. *The Cyclopædia of Practical Surgery.* Edited by WILLIAM B. COSTELLO, M.D. Parts II. and III.—*London; August,* 1837, and *July,* 1838. 8vo. (Art. *Aneurism,* by JAMES WARDROP, M.D.)

IN taking up the last part (Pt. III.) of Dr. Costello's Cyclopædia, for the purpose of noticing the article on Aneurism, by Dr. Wardrop, we could not but think that its editor and publishers must be blessed with a more than ordinary share of that stoical philosophy which sets at defiance all the thunders of the press. Each number of this work, as some of our readers know to their cost, and as others will be amused to hear, professes to follow its immediate predecessor after an interval of two months, " Published in Parts every alternate month." Yet so little has this announcement been fulfilled, that " every alternate year" would be nearer the truth. In fact, judging from the past progress of the work, we think the term *Century* might properly supersede that of *Cyclopædia,* since, by a moderate computation, not less than an hundred years will elapse before it can be completed. That its editor and contributors may live to see this desirable consummation, we earnestly hope; but as we ourselves cannot, in the ordinary course of events, expect to be in at the death, we must be content to catch, as we can, its juvenile numbers, leaving its maturer pages to the consideration of our successors, to the third or fourth generation, in the editorial chair.

It is not our purpose here to enter into an investigation of the various modes in which aneurism may be formed; these are to be found detailed in most elementary works, also in numerous treatises more expressly devoted to the consideration of this subject. On the present occasion we are rather desirous of illustrating the great practical principles which must be kept in view in the attempt to remedy the mischief occasioned or threatened by an aneurismal dilatation, and to show how far the measures recommended are calculated to effect the proposed object. The cure of aneurism may be effected spontaneously, that is, by certain changes brought about in the local condition of the affected vessel, without the assistance of the physician or surgeon; by certain methods of constitutional or internal treatment; and by local surgical operations. The one great principle, however, upon which all these modes of effecting a cure act, whether by the unassisted operations of the system or through the intervention of art, is the inducing of such a degree of remora in the local circulation as shall lead to the ultimate formation of a coagulum in the dilated portion of the diseased artery, thereby reducing the caliber, or producing complete obliteration of the affected vessel.

Dr. Wardrop says that the spontaneous cure of aneurism may take place in no less than five different ways.

> "1. The first and the most common of these spontaneous processes of cure is effected by the aneurismal sac being so filled up and strengthened with concrete fibrine that all danger of a rupture of the sac is removed, whilst, at the same time, the original canal of the vessel remains pervious, and carries on the circulation of the blood. 2. In the second process, by which an aneurism may undergo a spontaneous cure, the sac is not only filled with concreted fibrine, but the canal of the artery is obliterated. 3. An aneurism may likewise undergo a spontaneous process of cure by the tumour acquiring such a size and position that, by its pressure on the trunk of the artery, either on the cardiac or on the capillary side of the tumour, the sides of the artery are brought into contact and adhere. 4. An aneurism may also be cured spontaneously by a process of suppuration in the sac, and the artery above and below the tumour having previously been filled with coagulum. In such a case the integuments inflame and adhere to the sac, and then ulcerate, and those portions of the coagulum which cannot be removed by the process of absorption are ejected through the ulcerated opening, allowing the cavity of the sac to be filled up and obliterated by the process of granulation. 5. The fifth mode of spontaneous cure has been observed from the bursting of the tumour underneath the common integument, and the artery becoming obliterated by the pressure of the effused blood." (p. 206.)

It cannot be doubted that four out of the five methods here enumerated are merely illustrations of the principle which we have stated above. The first, however, and most common is not exactly parallel to these, and accordingly Dr. Wardrop would seem, with respect to this individual process, to have felt some difficulty as to including it within a general law, applicable to all the other processes of cure, whether spontaneous or artificial, with which we have hitherto become acquainted. "The principle or [of?] the processes which Nature employs for the cure of the disease," he observes, "is in all of them the same, the parietes of the sac being strengthened by, or the whole cavity filled with a fibrinous concretion." This *strengthening* of the parietes of the sac by fibrinous concretion we hold to be a mere assumption, fitted to meet a presumed difficulty, and of no value whatever in the true rationale of the process. It is true that Dr. Wardrop subsequently admits that this "fibrinous concretion is caused, or is permitted to be formed, whenever the circulation of the blood within the sac becomes preternaturally languid;" but by taking up with the intermediate condition of a presumed strengthening of the walls of the sac, he has obscured the expression of the leading principle, and introduced an element into his reasoning which a closer examination will show to be incorrect.

It is probable that Dr. Wardrop, in making this statement, may have been influenced by his peculiar views as to the formation of this concretion, the precise nature of which, he says, has not been accurately pointed out by pathological enquirers.

> "When the fibrinous concretion is examined," he observes, "it is found to consist of numerous concentric laminæ, which are more or less easily separable from one another and firmer in proportion as they approach nearer to the coats of the sac. The laminæ of fibrine which are in immediate contact with the blood circulating in the sac have generally a flocculent appearance, and have coagula of red blood mixed with them. But in some aneurismal tumours the concreted fibrine has its interior surface smooth and polished, being lined throughout by a membrane which has the appearance, as already mentioned, of being continuous with the lining membrane of the artery. . . . . Though this fibrinous concretion can be easily separated from the sac, yet it adheres closely to its internal surface; and I have every reason to believe that there is a vascular connexion existing between them. . . . . There is evidently a great difference, however, in the anatomical characters of a common clot of blood and a fibrinous concretion; and the coagulated lymph or fibrine, which is deposited in an aneurism appears to me to bear a strong analogy to the internal clot found in an artery on which a ligature has been placed, and likewise with those polypiform concretions which are formed within the cavities of the heart. Besides the coagulum of blood which is formed within the canal of an artery, immediately after the application of a ligature, and which acts merely as a temporary barrier to the flow of the blood, there is subsequently an exudation of fibrine from the internal coat of the artery, commencing from the place of the ligature, and extending as far up the canal as that point from which the first branch is sent off from the trunk of the artery. This fibrine adheres intimately, and appears to have a vascular connexion with the internal coat of the vessel. . . . . That the fibrinous concretions found within an aneurismal sac are organized also appears extremely probable, from the masses of coagulable lymph which are found within the cavities of the heart, in many instances adhering to, and having a distinct vascular connexion with, the endocardium or lining membrane of the heart; but what appears to me to confirm the opinion of the fibrinous concretion in an aneurismal sac being organized, and being an effusion from the vasa vasorum, is the fact of the coagulum formed in an obstructed vein having been distinctly injected by Mr. Kiernan. A branch of the vena porta had been compressed by a tumour and plugged up by effused lymph, and the arteries having been filled with fine injection, the coagulum was reddened at some points, and a very considerable sized vessel is exhibited in the preparation passing from the internal coat of the vein into the coagulum. Whilst, therefore, red blood may coagulate in an aneurismal tumour, merely by the diminution in the force of the circulation, as shall hereafter be pointed out, the formation of an organized fibrinous concretion must be the result of a different process, and can only be explained by supposing fibrine to be effused from the internal surface of the sac itself, in like manner as coagulable lymph is effused on the surface of an inflamed serous membrane." (pp. 208, 209.)

We have no objection to the adoption of Dr. Wardrop's views, could we see but the shadow of a reason brought forward in proof of their correctness; but without taking into consideration the inapplicability of Mr. Kiernan's preparation, which is the only fact adduced in support of them, it may at once be said that, had these views been founded in what actually occurs, the author would have been at no loss to have confirmed them by instances passing daily under his own observation and that of his professional friends. The distinction of the contents of an aneurismal sac into layers of fibrinous concretion and mere coagula we believe to be based on correct observation; but the theory of the formation of these concretions and the idea that they contribute in any appreciable degree to the strengthening of the parietes of the sac are not only unsupported by facts, but also, as it appears to us, of a very questionable nature.

In such of the larger aneurismal swellings as we have had an opportunity of examining after death, the layers of fibrinous concretion have been of no firmer consistence than so many pieces of well-soaked brown paper, and have appeared thoroughly macerated, as it were; and, so far from presenting any impediment to the impulse of the heart's action, were easily lacerable, yielding readily to the application of the slightest force. The preparation of Mr. Kiernan, we have said, is inapplicable; for, independently of the fibrinous deposit in this instance having occurred under entirely dissimilar circumstances, the subject of vascular connexions existing between lymphy exudations and the parts with which they are in immediate contact is involved in too much obscurity to found any very definite conclusions upon. The fibrinous exudation which takes place from the internal coat of the artery subsequent to ligature is also by no means a parallel to what occurs in an aneurismal sac. In the preparation in the Hunterian Museum, referred to by Dr. Wardrop, in which the internal clot was reddened by the injection of the vasa vasorum of an artery to which a ligature had been applied, John Hunter was himself doubtful whether the red colour of the clot was owing to injected vessels or the effect of effusion. But, admitting that this preparation proves the fact of vascular connexion in the class of instances to which it especially refers, it can scarcely be considered that more than a very remote analogy exists between the state of an artery rendered impervious by ligature and rupture of the internal coat, and aneurismal dilatations in which the inner coat of the vessel is often preserved intact and the passage remains free.

We must, however, be contented with a mere indication of these objections, and proceed to the consideration of the internal and external methods of cure adopted in the treatment of aneurism. It is unnecessary to enter at any length into details upon this point, and it will be sufficient to state that the principle before laid down is that upon which all of them are founded. The chief means employed in the internal mode of treatment are the hunger-cure of Valsalva, by which it is attempted so to reduce the force and lessen the amount of the circulating fluid as to favour the formation of a coagulum in the dilated portion of the vessel. To this the repeated abstraction of blood and the exhibition of digitalis, hydrocyanic acid, and tartarized antimony, materially contribute; and there can be no doubt that many cases of aneurism have yielded to the steady and well-directed employment of these measures.

The external and local methods of compression and ligature of the diseased vessel above the aneurismal swelling are obviously mechanical measures, at least in their primary operation, for the attainment of the same object. The various modes of applying compression are well pointed out and described by Dr. Blasius; but, besides being inapplicable to a vast number of instances, compression is very uncertain in its effects and tedious in operation in many of those in which it promises a favorable result. The operation by ligature, then, is that which is chiefly trusted to in the present day; and of this several modifications have been devised. The method at one time followed was to lay open the aneurismal swelling, turn out the contents of the sac, and place a ligature on the artery, above and below the dilated part, the cavity becoming gradually filled up through the suppurative and granulating processes.

This mode of operating, however, is now rarely if at all had recourse to in this country, although, from the observations of Dr. Blasius, it would still seem to be occasionally practised in Germany in certain forms of diffuse aneurism and of aneurism by anastomosis. That which is now most generally followed, as being the most simple and certain in its operation, and of the most general application, is the Hunterian operation, in which, as our readers well know, a ligature is placed on the arterial trunk, between the aneurism and the heart. The rationale of this operation and the subsequent progress, together with the measures requisite to ensure success, are now too generally known and appreciated to render it necessary to allude to it at any greater length.

Another method of operation by ligature was proposed by Brasdor, which consists in the application of the ligature beyond or on the capillary side of the tumour, and is considered by some surgeons of eminence to be applicable in certain cases, in which, from want of room to apply the ligature on the cardiac side of the aneurism, the Hunterian operation is impracticable. Brasdor's operation was never performed by himself, and two cases, the one of femoral and the other of iliac aneurism, operated upon respectively by Deschamps and Sir Astley Cooper, after this method, having terminated fatally, it was not again tried until the year 1825, when Dr. Wardrop practised it in a case of aneurism of the right carotid artery. This case was published in the Transactions of the Medico-Chirurgical Society for that year; and its successful result led to the adoption of the practice in other instances. One of the results of this method of operating, and which could scarcely have been anticipated, is the immediate diminution of the size of the tumour. This diminution in Dr. Wardrop's case was progressive, "so that on the fourth day after the operation it (the tumour) seemed to have diminished nearly one third, the upper and tracheal portion had lost all pulsation, and only the scapular portion retained an obscure undulatory thrill."

A modification of this operation has been since devised by Dr. Wardrop, and successfully put in practice by himself and other eminent surgeons. An objection to Brasdor's operation was stated by Mr. Hodgson, in alluding to the unsuccessful case of Deschamps: "If a stream," he observes, "continued to pass through the aneurism into branches which originated below it, the blood contained in the tumour was *not at rest*, and consequently did not coagulate; a cure *could not*, therefore, be expected to ensue on the principle which led to the performance of the operation." And again, "It must be acknowledged, however, that it will be almost impossible in any case to ascertain that *no branch* arises from the sac, which, by continuing the circulation through the latter, may defeat the objects of the operation." The failure in this reasoning arises from its not having been recognized—what, however, might have been inferred from the success of many of the cases treated upon Valsalva's method—that the real principle of treatment is the inducing of a certain degree of diminution in the force of the circulation rather than its immediate and entire stoppage.

Dr. Wardrop, in quoting Mr. Hodgson's remarks, observes, "From the important pathological fact that in the processes which are employed by Nature for effecting the 'spontaneous cure' of an aneurism, and likewise from the circumstance that in some cases of popliteal aneurism, where the Hunterian operation had been performed, a complete stoppage of the circulation of the blood within the tumour had not been required, it appeared to me a legitimate conclusion that the progress of an aneurism might be arrested, or even the tumour consolidated, by placing a ligature *on one of the branches* of a diseased trunk. It being admitted that a diminution of the force of the circulation of the blood, through an aneurismal sac, was sufficient to cure the disease, the important question still remained to be determined—to what degree is it required to diminish the momentum of the circulation through an aneurism, in order to permit a fibrinous concretion to be formed within the sac?" The importance of keeping in view the distinction between mere coagulum of red blood and layers of concrete fibrine is then insisted upon; and it is attempted to be shown that while the operations of Hunter and Brasdor are contrived upon the principle of putting a complete stop to the circulation in the tumour, the modification of the latter of these, introduced by the author, is more on the principle of the spontaneous cure.

As it appears to us, however, setting aside the questionable opinion of a strengthening of the parietes of the dilated artery by these layers of fibrinous concretion, the same principle includes both the spontaneous cure, the method of Valsalva, and the operation of Wardrop, on the one hand, and the methods of Hunter and Brasdor on the other: the check to the circulation in these last, as also in some of the instances of spontaneous cure, being complete instead of partial. In aneurisms of the lesser arterial trunks the disadvantages attending the carrying out of the principle to its *extreme*, by putting an immediate arrest on the course of the circulation, though occasionally felt, are still of trifling import, compared with the greater certitude attending the operation; but in the dilatations of the larger vessels, of the carotids and subclavians near their origin from the arteria innominata and arch of the aorta, and in aneurism of the aorta itself, the case is widely different. Much credit is due, therefore, to Dr. Wardrop, for the devising and carrying into operation the method which he has proposed; and the success which, in a certain number of instances at least, has attended it affords encouragement for the following out of the principle, whether by operation or by other modes of treatment, in all cases in which the Hunterian method or that of Brasdor is, from whatever cause, inapplicable. Dr. Wardrop relates six cases of aneurism of the arteria innominata, operated upon after the mode which he advocates: the first by himself, that of Mrs. Denmark, already before the public; the second by Mr. Evans, of Belper; the third by Professor Mott, of New York; the fourth by Dr. Morrison, of Buenos Ayres; the fifth by M. Laugier; the sixth by Mr. Fearn, of Derby—all treated by placing a ligature *on one of the branches* of the diseased artery. He refers also to two others, in which a similar principle of operating was adopted, one by Mr. Scott and the other by Mr. Key. The results of these operations are thus commented upon:

> "In five of the cases the operation was followed by relief of all the symptoms, and by a diminution of the bulk and consolidation of the tumour. The three other cases were unsuccessful; but this militates not against the principle of the operation itself, but the injudicious selection of the patients on whom it had been employed. In M. Laugier's patient the consecutive hemorrhage was a proof of the diseased state of

the artery at the place where the ligature was applied; and the appearances after death pointed out that the operation, in this instance, where the aneurismal disease was so extensive, was by no means judicious. Neither can the unsuccessful result of the case operated upon by Mr. Key, where the patient died a few hours after the operation, be considered as any proof of the disadvantages of this mode of operating, but of its misapplication; and in the case which was operated upon by Mr. Scott there is every reason to believe that the aneurism was too far advanced, and the vessels too extensively diseased to admit of any benefit from the operation." (p. 236.)

With this extract we must conclude our notice of Dr. Wardrop's able article, regretting that we are unable, on account of their length, to transfer any one of the cases to our pages. For a further development of the views of the author, and for the consecutive advantages of his mode of operating in the instances of aneurism of the primary branches of the arterial system, we refer to the essay itself. The article of Dr. Blasius, though inferior in some respects, is yet of equal if not superior utility for the purpose for which it is intended. It affords a succinct account of the nature, varieties, etiology, and development of aneurisms; of their symptoms, diagnosis, and progress, with a good summary of the several modes of treatment; and is calculated to be of especial assistance to the young surgeon, from the fulness with which the directions for facilitating the mere manual part of the operations are given.

Art. VIII.

1. *Diagnostisch-praktische Abhandlungen aus dem Gebiete der Medicin und Chirurgie; durch Krankheitsfälle erläutert.* Von Dr. Löwenhardt. Zweiter Theil.—*Prenzlau*, 1838. 8vo, pp. 425.

Diagnostic and practical Essays on Medical and Surgical Subjects; illustrated by cases. By Dr. Löwenhardt. Part. II.—*Prenzlau*, 1838.

2. *Versuche für die praktische Heilkunde.* Von Ferdinand Jahn. Erste Heft.—*Eisenach*, 1835. 8vo, pp. 216.

Essays on Practical Medicine. By Ferdinand Jahn. Part I.—*Eisenach*, 1835.

We join these works together because they are composed on the same plan and have the same object of conveying information in practical medicine. As they are also both productions of eminent practitioners, they may serve as illustrations of the actual pathology and therapeutics of our brethren in Germany. We shall notice them separately.

I. In the Fourth Number of our Journal we noticed the first part of Dr. Löwenhardt's essays, and especially directed the attention of our readers to a very valuable paper on inflammation of the ovaries. The contents of the present volume are more diversified than those of its predecessor, and all of the papers present, amid some superfluity of words and trifling details, so much interesting matter, that we will endeavour to lay a short abstract of them before our readers.

Dr. Löwenhardt commences his first essay—*On the employment of large mercurial inunctions in various diseases*—by some general observations on the use of mercurial ointment. He is in the practice of directing from two to four drachms of the unguentum hydrargyri to be rubbed in every two, sometimes every four hours, until a gentle salivation is produced. The production of profuse ptyalism was never aimed at, nor does it appear to have often occurred; but two cases of fatal mercurial eczema are related, one of which took place in Dr. Löwenhardt's practice, and the other was met with in a young man who had made a few applications of mercurial ointment to the pubes, in order to destroy pediculi. After the preliminary remarks, the author proceeds to describe the diseases in which he has reaped benefit from this method of introducing mercury into the system.

The fatal termination of many cases of *apoplexia sanguinea*, in which the patient gradually sinks into a state of coma, whence all efforts to rouse him are unavailing, induced Dr. L. to make a trial of mercurial inunction. One instance is related in which venesection was performed, stimulant enemata were given, and the usual remedies were employed during twenty-four hours without any improvement in the patient's condition, and in which subsequent inunction of three ounces of mercurial ointment, producing salivation, was followed by amendment of all the symptoms, and ultimate recovery. The second case is still more striking. It occurred in a man seventy-eight years old who had already experienced two apoplectic seizures. He lay in a state of coma during forty-eight hours, from which (says Dr. L.) neither venesections (two in number) nor any other means could rouse him. Dr. Löwenhardt ordered two drachms of ung. hydr. to be rubbed in every two hours; and on the following day, ptyalism having been produced, consciousness began to return, and the patient eventually recovered.

Several cases of *acute hepatitis* are detailed, which were completely cured by mercurial inunction; all the symptoms giving way immediately on the appearance of salivation. The following is one of the cases:

A lady, thirty-six years old, of a delicate frame and weak constitution, was attacked by acute inflammation of the left lobe of the liver. Two days passed before the real nature of the affection was discovered; venesection was then had recourse to, a grain of calomel was given every two hours, and half an ounce of mercurial ointment was rubbed upon the abdomen daily. This treatment was persevered in for three days without any amendment following. Dr. Löwenhardt then discontinued every other remedy, and directed two drachms of ung. hydr. to be rubbed in every two hours; in less than forty-eight hours the gums began to be tender, and signs of amendment appeared, and perseverance for another day in this treatment was followed by complete restoration to health.

In recommending mercurial inunctions in *febris nervosa gastrica* (the *gastro-entérite* of the French writers), Dr. Löwenhardt is reviving the use of a remedy which was originally employed with great success in malignant fevers by Ambrose Paré, and has since been occasionally resorted to with good effect by some of the Italian physicians. He states that until very recently he was in the practice of treating this affection according to the rules usually laid down, but having observed that the mortality was almost always the same (about 1 in 7), whatever treatment was employed, or even when the cure of the patient was left almost entirely to nature, he was induced to search for some remedy likely to exert a more decided influence upon the course of the disease. In order fairly to test the value of mercurial inunction, no internal remedy was administered in those cases in which it was employed, though venesection or the application of leeches was premised when necessary; and when the disease took its usual course, the inunction was not commenced before the seventh or ninth day of the fever. In most cases, from two to three drachms of ung. hydr. were rubbed in every two hours, and the first symptoms of salivation usually made their appearance when from three to five ounces of the ointment had been used. A very evident diminution in the severity of the symptoms generally showed itself with the appearance of salivation, though in some cases, where the remedy had been resorted to late, the amendment was more gradual; all the patients, however, who were salivated recovered.

Of forty-eight persons thus treated, all were cured within from fourteen to twenty-one days; but one little girl, only seven years of age and of a sickly scrofulous habit, died nine weeks after her apparent recovery. Five other patients in whom cerebral affection predominated, and in whom the disease ran a very rapid course, died before salivation could be induced.

Smallpox is another disease on which Dr. Löwenhardt has tried the effect of mercurial inunction. He first sought by means of it to prevent the development of the smallpox pustules, and thus to cut short the disease; but in this he failed, and only succeeded in retarding the progress of the pustules by two or three days. He now proposed it merely as an occasional substitute for the cauterization of the pustules with nitrate of silver, which, though very efficient, is an exceedingly tedious process, and especially troublesome and difficult when children are the patients. After the pustules have been opened, he recommends their being smeared with ung. hydr., which, he assures us, produces the same effect of preventing pitting and cutting short the secondary fever, which follows the application of lunar caustic.

We have not room for a detail of Dr. Löwenhardt's experiments with regard to the nature of the eruption which appears after exposure to variolous infection, in those who have been previously vaccinated. They go to establish the fact that this eruption does not essentially differ from variola, and that smallpox is produced by inoculating with matter from its pustules.

The second memoir is devoted to a subject involved in considerable obscurity, namely, the periodicity of disease; it is entitled *On intermittent diseases, and in particular, on intermittent inflammations,* and opens with some observations on the difficulty of distinguishing inflammations with periodical exacerbations from intermittent fevers. Local inflammations, the daily exacerbations of which commence with rigour, are, in Dr. Löwenhardt's opinion, very often confounded with *quotidian intermittents;* and he thinks that practitioners just as frequently mistake the *febris gastrica subinflammatoria,* in which there exists an irritable or inflammatory condition of the glandular system or of the mucous membranes, for tertian intermittent. Several pages are devoted to the diagnosis between these two affections; but the points on which Dr. Löwenhardt most insists are the absence of those critical signs, the sweat and the deposit in the urine, which follow each paroxysm of real intermittent fevers, and the continuance of pain and uneasiness in the inflamed part during the intervals of the attack. In the treatment of these affections, though an antiphlogistic method is at first necessary, and local depletion is in many cases requisite, yet the subsequent employment of preparations of bark is often needed to complete the cure.

The third essay treats *Of the employment of mercurius vivus in introsusception.* The author considers that quicksilver has in these cases been far too hastily condemned by those who have had no personal experience of its effects; he regards it as a remedy well deserving a further trial; and in one of the cases which he relates, the preservation of the patient's life certainly appears to have been entirely owing to its administration. We shall give an abridgment of this case.

L———, a journeyman blacksmith, twenty-seven years old, a healthy man, but afflicted with a double inguinal hernia, as also with an abdominal hernia about two fingers breadth above the right groin, was attacked on the 1st of April, 1837, without any evident cause, with nausea, vomiting, pain in the abdomen, and tenesmus. It was first ascertained that there was no strangulation of intestine in any of the usual situations; purgatives were then administered, but no relief having been experienced, venesection was performed on the evening of the same day, and purgative enemata were given. The night was passed without relief; venesection was repeated and tobacco clysters were given on the following day. On the 3d, venesection was again performed and leeches were applied. The symptoms increased in severity during the 4th and 5th; the bowels could not be induced to act, feculent vomiting set in, and on the 6th the patient's recovery appeared impossible. As a last resource, Dr. Löwenhardt now administered three doses of quicksilver, an ounce each time, at intervals of one hour, followed by half-ounce doses of castor oil. At noon, immediately after taking the third dose, all the symptoms were much aggravated, the vomiting became very violent, and the castor oil was rejected, though the quicksilver remained on the stomach and the patient seemed to be dying. At four P.M., however, he experienced a sudden inclination to empty the rectum, and so violent a diarrhœa came on, that, within a very short time, the patient passed eighteen evacuations. The same evening he felt better; on the following day diarrhœa and a sense of weight in the abdomen were the only symptoms from which he suffered; and on the 9th, after having voided the quicksilver, he felt perfectly well, and has continued so ever since. (pp. 273-8.)

Want of space will not allow us to do more than to quote, in Dr. Löwenhardt's own words, the indications for the employment of this remedy; and we must refer our readers to the work itself for the history of several other cases, as well as for some interesting remarks on the diagnosis of internal strangulation (*incarceratio intestinorum interna*).

The cases in which the author conceives the administration of quicksilver to be justifiable are the following:

"1. *Volvulus.* Even in instances of the retrograde kind I cannot conceive that quicksilver should produce the inevitable fatal results which some have represented, although in such cases it would probably do no good; but there is no medicine of the unsuccessful employment of which we do not almost every day see some instance. Such is the case related by Willan, in which, though the quicksilver passed the bowels, it failed to relieve the *volvulus.*

"2. *Incarceratio interna.* In this case, if it should fail in producing its effect within twenty-four or thirty-six hours, it would be proper to have recourse to gastrotomy.

"3. *Ileus*, especially *ileus spasticus*. If the treatment, suggested by the nature of the exciting cause, should have proved unsuccessful.

"4. *Ileus inflammatorius*. If, after the symptoms which occur during the inflammatory stage have been subdued, the antiperistaltic motion of the intestinal canal should still continue, either from mere irritability or from exudation within the intestine.

"It is possible that, if the intestine were in part gangrenous, the quicksilver might cause laceration of its coats and extravasation into the abdominal cavity; but such cases would have a fatal result independently of the administration of quicksilver.

"5. If the symptoms of strangulation should continue after the performance of the operation for hernia, and reason should exist for supposing them owing to impermeability of the intestine from exudation upon its lining membrane, and agglutination of its walls.

"6. Lastly, violent vomiting, which other remedies have failed to relieve. Here I should employ it without hesitation, in the conviction that, at least, I could do no harm. Metallic mercury was also often employed with success, (*Panzoni, Description of a Disease which prevailed at Piarno, in Italy, in the year* 1786, *Lübben*,) during an epidemic, of which violent vomiting was one of the most prominent symptoms." (pp. 301-3.)

In the fourth essay—*On inflammation and caries of the lower jaw, produced by the appearance of the dens sapientis*, many novel and interesting facts are mentioned, relating to an affection hitherto overlooked or confounded with *cynanche parotidea*. Dr. Löwenhardt describes it as an inflammation of the *processus coronoideus* of the lower jaw-bone produced by the cutting of the wisdom tooth, and commonly attacking only the right side of the face. This inflammation soon extends to the ramus and sometimes to the body of the jaw, and terminates in the production of caries of the bone, having involved the soft parts at a very early stage: the parotid and submaxillary glands become inflamed and sometimes suppurate, and the severity of the disease in them often misleads the practitioner, inducing him to overlook that of the bone and to regard the case as one of simple inflammation of the salivary glands.

The remote causes of this affection are to be sought for, either in peculiarities in the form of the jaw-bone itself or in the arrangement of the teeth. Sometimes the body of the lower jaw is so small as to be completely filled up by the fourteen teeth which first appear, and no space is left for the *dentes sapientiæ;* in such a case they either remain completely within the jaw or, as sometimes happens, they take so great an inclination inwards as thus to avoid the crowding they would otherwise occasion. The last molar teeth in the lower jaw generally take more or less of an inward direction; but when that is not the case all the inconveniences of a small jaw-bone are much aggravated, and the cutting of the wisdom-tooth becomes a source of great irritation. In some cases, too, the coronoid process is found to deviate from its natural inclination even so far as to form an acute angle with the body of the jaw, while, at the same time, it loses its natural convexity outwards, and thus diminishes the space for the teeth in two directions. Where such a conformation of the jaw exists, the appearance of the wisdom-tooth produces the train of symptoms about to be described, the severity of which is, as might be expected, proportionate to the size of the crown of the tooth and the divergence of its roots.

The affection may be divided into two stages, the inflammatory and the suppurative. The first symptoms experienced by the patient are pain in the ear and in the articulation of the jaw on one side, commonly the right, and the movements of the jaw become constrained. The pain is increased towards evening or whenever the patient bites any hard substance; and it soon extends to the swollen parotid and submaxillary glands, and even to the muscles of the neck. After a time all the symptoms are aggravated, the swelling increases, deglutition is rendered difficult, the pain becomes more severe, and the part is so tender as not to bear the slightest touch. The patient continues with the neck stiff and the head turned somewhat to the affected side, for the slightest motion gives pain, and even becomes impossible as the disease proceeds. On examination of the mouth, the tonsil and the gum about the last molar tooth and about the articulation of the jaw of the affected side will be found red, swollen, and painful, and very often the top of the wisdom-tooth may be seen just making its appearance. By degrees the jaw becomes quite immoveable, so that the patient can take nothing but fluids; his general health suffers, and pain in the head, heat, thirst, and other febrile symptoms are produced by his sufferings. A sure sign that the caries of the bone has commenced, and that the disease has reached its second stage, is the loosening of the molar teeth of the affected side, which after a time can be removed without difficulty. Suppuration now takes place in the parotid and sometimes in the submaxillary gland; the matter burrows, forming sinuses among the muscles which sometimes are extensively destroyed, and at last escapes by various fistulous openings. This is followed by a diminution in the violence of the pain, the tension in the joint becomes less, and the patient finds himself able partly to open his mouth. When the disease has reached this height, the wound cannot heal until either nature or art has removed the dead bone; but if the case should fall under the eye of the practitioner at an earlier period, the extraction of the last molar tooth but one will often make room in the jaw, and remove the sufferings of the patient. Occasionally, however, the employment of leeches and of antiphlogistic treatment will also be necessary, and in some cases the practitioner is compelled to trust entirely to them, owing to the patient being unable to open his mouth. This treatment, however, leaving untouched the real source of the disease, is often insufficient, though it is frequently the only one adopted, owing to the practitioner confounding this affection with simple inflammation of the salivary glands, such as sometimes occurs sporadically in persons of a rheumatic constitution after exposure to wet; but it is very important that the practitioner should learn to distinguish between these two diseases.

This disease (*mandibulitis adultorum*) begins without any assignable cause: it attacks the right side of the face almost exclusively; its symptoms increase in severity in proportion to the duration of the affection, but the swelling of the glands is never very considerable, and at first deglutition is unaffected: the first stage lasts but for a short time, and with the commencement of the second stage the patient begins to recover the power of moving the jaw. Attacks of *angina rheumatica* can, on the contrary, be attributed to exposure to cold or to some similar cause; they usually begin acutely, and afterwards assume a more chronic character; they are accompanied by great swelling of the external parts, and deglutition is from the first much impeded; both sides of the face are affected, either at once or successively, and the disease has a great tendency to assume a chronic character, and be very tedious in its course.

The paper concludes with the history of several cases of this affection, in three of which partial amputation of the lower jaw was necessary.

Instead of quoting any of Dr. Löwenhardt's cases, we shall extract a similar one from an American journal, which shows that the disease has not been overlooked by our Transatlantic brethren. It is contained in a paper by Dr. Trudeau "On the varieties of deviation in the wisdom-teeth," originally published in the Medical Examiner, but taken by us from the Boston Medical and Surgical Journal for Aug. 1838.

"*The Wisdom-tooth arrested in its growth by the base of the coronoid process.* In this case the right cheek was swelled to an enormous size, the swelling extending from the eyes to the clavicle. The face and neck were covered with numerous abscesses. For twenty months the man had not been able to open his mouth, and had been fed on liquids, passed through an opening caused by the absence of a tooth. He had, besides, a fistula, at about three inches from the lower angle of the inferior maxilla; a little lower on the neck there was another. A probe being introduced into the first fistula, penetrated obliquely from before backwards, and was stopped by a hard body, supposed to be the root of the third large grinder. From the beginning of the affection, the man's health had been greatly impaired; he was much emaciated, his skin was of a leaden hue, and he suffered much from colics. For a short time past, his digestion had been disordered and attended with acidity; this, probably, depended on the mixture of his food with the pus, with which his mouth was constantly filled. Various means were resorted to, to open the patient's mouth and extract the tooth. Leeches, poultices, mercurial frictions, blisters, and compression were used with no better success. Dr. ——— now thought of trying a mechanical agent, which succeeded perfectly. It was a conoidal piece of wood, introduced between the dental arch, and pushed in slowly by the patient himself. The following day the mouth presented an opening of about four lines. A week afterwards, the man's mouth was opened wide enough to allow of the tooth being easily extracted. A few days after, a necrosed piece of bone was extracted. It proved to be a portion of the base of the coronoid process, on which was moulded or cast a portion of the crown of the tooth. This evidently showed that the tooth had been stopped in its growth by this bone. Since that time the inflammation rapidly disappeared, and in a month the patient was perfectly cured."

Besides the above essays, the work contains a few short memoirs upon obstetric subjects, which we have not now space to allude to, but accounts of which we may hereafter lay before our readers.

II. Dr. Jahn is also known to our readers as the author of a singular work on Pathology reviewed in our Seventh Number. The present is a collection of short treatises on various subjects connected with practical medicine, on which it has been the author's object to throw additional light. His observations are valuable, and are exceedingly interesting, from the enthusiasm and learning stamped on almost every page, and from the many bright suggestions with which they abound. In perusing them we are struck with surprise to find no allusion made in any part to the author's novel system of pathology, or *physiatrics*, as he has termed it, noticed at full length in our Seventh Number. The present volume is of a totally different character, and its chief aim is to communicate practical knowledge. Theory, however, is not altogether neglected, but facts and cases are detailed in general simply as they occurred; and when hypothesis is added, it does not detract from the value of the observations on which it is founded.

1. The first and most important article is upon acute hydrocephalus. During the spring of 1834 a very fatal epidemic occurred at Meiningen, and in a space of seven or eight weeks, twenty-one cases came under Dr. Jahn's notice, of which twelve proved fatal. Our author enters very fully upon the nature and treatment of the disease, and details at considerable length the appearances observed after death, analyzing and criticising at the same time the opinions of other authors. He urges the practitioner to continue his exertions to save his patient even after indubitable signs of effusion have shown themselves, and relates four cases of complete recovery after transudation had taken place. In two of these, the favorable termination appeared to be owing to the application of large blisters to the nape of the neck and scalp, and in the other two, to the almost unaided efforts of nature. There exists, we are aware, very often considerable difficulty in determining whether effusion has or has not taken place, and many physicians, and among others Gölis,* have expressly stated their opinion that this state, when once it has occurred, precludes all hope of recovery. The first two cases detailed by Jahn are given so fully that they have left the impression on our mind that in them effusion really had taken place, but in the latter two it is merely stated, that transudation having ensued, the case was considered hopeless and given up by the physician, and that nevertheless the children recovered. A case, we hold, should never be given up; we have seen cases of hydrocephalus given up both in public hospitals and in private practice; and although death invariably ensued in such cases, we are not warranted in assuming that such would have been the result, had some energetic means been resorted to.

2. In several cases of paralysis, more or less complete, dependent on spinal affections, Dr. Jahn had recourse to the spirituous extract of nux vomica, but in none of the cases did he derive any benefit from its use. Indeed, in his hands the medicine seems to have been totally inert. In one case he carried the dose to twelve grains daily, in another to seventeen, in a third to twenty, and in a fourth to thirty grains daily, but in none of these cases had it any effect in diminishing the paralysis, nor does it in any way affect the nervous system. We cannot help suspecting that the extract used by Dr. Jahn had either been intentionally adulterated, or had suffered in some way by long keeping.

3. The ninth article is one of considerable length, and is devoted to the consideration of the secondary symptoms of gonorrhœa. This has long formed one of the vexed questions of medicine, and is one on which those who have the best means of coming to a decision are still not agreed. Ricord denies in the strongest terms that secondary symptoms ever follow on gonorrhœa unconnected with chancre, whilst another surgeon, Cullerier,† enjoying the same opportunities of study, freely admits

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* Von der hitzigen Gehirnholenwassersucht. Band i. p. 195, et seq.

† Dict. de Méd. et de Chir. prat. Art. Blennorrhagie et Syphilis.

the possibility of general lues from simple gonorrhœa. But Jahn, in accordance with the opinion of several German physicians, among whom we may mention Autenrieth and Schönlein, believes that gonorrhœa is occasionally followed by secondary symptoms peculiar to itself, and different in nature from those which follow on chancre. The catalogue of evils which Dr. Jahn enumerates as liable to follow upon gonorrhœa is quite appalling, but we must do him the justice to say that he acknowledges never having met with them in his own practice. He mentions acute and chronic inflammation of the mucous membranes of the air-passages and intestines, stricture of the œsophagus, scirrhus of the pylorus, excrescences on the intestinal mucous surface, ulcers on the skin, inflammation of the joints, condylomata of the heart, tubercles in the various glands, and many other affections of equal importance. Dr. Jahn relates three cases of gonorrhœa which lately came under his notice and which were followed by secondary symptoms, but these showed nothing different from the common symptoms of secondary syphilis; and five cases of chronic disease which he cites at the end of the article, in illustration of the doctrines contained in it, afford no reason for supposing that they were a consequence of gonorrhœa. It is maintained by some surgeons that secondary symptoms, which make their appearance after an apparent simple gonorrhœa, must owe their origin to a chancre which had escaped notice by being situated in the urethra, or from some other cause, whilst others contend that secondary symptoms may follow upon what they term syphilitic gonorrhœa; that is, upon an inflammation of the mucous membrane dependent on syphilitic contagion. We are not aware, however, that any distinction has ever been drawn in this country between the symptoms which follow on the one affection or on the other, so as to mark them as two distinct diseases, and there are no arguments in Dr. Jahn's paper which tend in any way to convince us that such is the case. The paper displays great erudition, but it possesses little practical value; and Dr. Jahn does not detail a single case as having occurred to himself with which he could fortify his hypothesis.

4. In the seventeenth paper the author notices the results of an investigation which he conducted with occasional interruptions during a space of two years, in order to satisfy himself of the truth or falsity of the homœopathic doctrines. He was led by these experiments to put faith, if not in the infinitesimally small doses of the homœopathists, at least in doses small in comparison to those to which he had been accustomed. Goitre is an endemic and frequent disease in the neighbourhood of Meiningen, and experience has amply proved that it yields only to iodine, burnt sponge, and similar remedies. It was therefore with surprise that Dr. Jahn saw the swelled thyroid gradually diminish in size under the use of powders, containing each $\frac{1}{60}$th of a grain of iodine, administered either daily or every second day. Thirteen cases of goitre were thus treated; fourteen days was the shortest space, and five weeks the longest space required to effect the cure. Smaller doses he found inefficacious. Our present space does not allow us to notice any of the other papers particularly, but we can recommend them as containing much interesting and instructive matter.

ART. IX.

1. *Deux Mémoires, en réponse à cette question—" Faire connoître les Analogies et les Differences qui existent entre le Typhus et la Fièvre Typhoide."* Par MM. C. E. S. GAULTIER DE CLAUBRY et J. J. H. MONTAULT. Mémoires de l'Académie de Médecine. Vol. VII. 1838.

On the Analogies and Differences between Typhus and Typhoid Fever. By MM. GAULTIER DE CLAUBRY and MONTAULT.

2. *A Report founded on the cases of Typhoid Fever, or the common continued Fever of New England, which occurred in the Massachusetts General Hospital, from the opening of that Institution, in September,* 1821, *to the end of* 1835. By JAMES JACKSON, M.D.—*Boston*, 1838. 8vo, pp. 95.

3. *A Short Treatise on Typhus Fever.* By G. L. ROUPELL, M.D.—*London*, 1839. 8vo, pp. 301.

4. *Etudes Cliniques sur divers points de l'Histoire des Fièvres Bilieuses et Typhoides.* Par le Docteur H. C. LOMBARD.—*Genève*, 1838. 8vo, pp. 40.

Clinical Remarks on certain points in the History of Bilious and Typhoid Fevers. By Dr. LOMBARD, of Geneva.

5. *Der Typhus und dessen Erscheinungen, oder die Typhoseptosen, pathogenetisch und therapeutisch erläutert.* Von L. BUZORINI, M.D. &c. &c.—*Stuttgart*, 1836. 8vo, pp. 303.

Typhus and its Phenomena, or the Typhoseptoses; pathologically and therapeutically illustrated. By L. BUZORINI, M.D.

6. *Das Blutfieber, vorzuglich in seiner Verbindung mit einigen Krankheiten des Darmkanals; pathogenetisch und therapeutisch erläutert.* Von F. KEHRER, M.D. &c.—*Mainz*, 1837. 8vo, pp. 128.

Blood-Fever, especially in its Connexion with certain Diseases of the Alimentary Canal; pathologically and therapeutically illustrated. By F. KEHRER, M.D. &c.

THE seventh volume of the Mémoires de l'Académie de Médecine is almost entirely occupied by two prize essays on the subject of Fever. By the majority of the profession in this country "the analogies and differences" between typhus and typhoid fever, will be regarded but as variations of form and manifestations of the same disease. There is a defect in the mode of putting the question as proposed by the Academy. which considerably prejudices it. It assumes, as it were, that there are two diseases, actually as well as nominally distinguished, whereas this distinction is the point at issue. The two French authors take opposite views of the subject: M. de Claubry contending that the terms typhus and typhoid fever include one disease, M. Montault taking an opposite view, and regarding the terms as properly applicable to two diseases. There is a question which necessarily suggests itself to enquiries of the kind before us, i. e. "what is to be considered as constituting distinction of diseases?" And it is likewise necessary to be well guarded against the influence of names in studying the complicated phenomena of disease. We can easily conceive that if it were proposed to show that under the term variola, two or more diseases were comprehended, it would be no difficult matter to make out a strong case for the affirmative of the proposition, by an ingenious classification of causes, of varying symptoms, of different degrees of morbid changes, occurring during life and detected after death; and in our view of the subject, this is what has by some been effected for typhus fever. With the influence produced by the trammels of nomenclature and ineffectual attempts at definition where definition is almost useless, without a due regard to the analogy of disease, without a comprehensive view of the particular series of morbid phenomena under consideration, as influenced by climate and season, by national or individual characteristics, certain phenomena have been selected as justifying distinctions which, we think, are unfounded in nature. But it is not without feeling the great difficulties attendant on the subject, nor without an acknowledgment of the respect which is due to many whose views are opposed to our own, that we thus state our opinions. It must be admitted that one cause (the contagion of fever, for instance,) acting on two different organizations will not produce similar results; and if the external cause or causes be varied and modified in their proportions and combinations and facilities of action, and the organizations on which they operate are susceptible of their influence in all possible varieties, differences of symptoms and of morbid changes will exist in all possible degrees, the diseases still being identical. There may be extremes of these differences, at one of which certain phenomena are more constantly present, at the other of which they are rarely found to exist, and in the intermediate links the phenomena are associated in all conceivable degrees. Such, we think, are the views that our present knowledge justifies us in taking of typhus fever. We shall make no further allusion (as no attempt in the volume before us is made to prove it) to the following bold and truly characteristic generalization. "I have shown," says M. Lombard, "that there exist in England two kinds of continued fever, well distinguished the one from the other: a sporadic fever, but little contagious, existing in all parts of England, and which does not differ from our typhoid fever; and an exceedingly contagious fever, which commences and has its nucleus in Ireland, whence it extends to England and Scotland, following with great exactness the migrations of the labouring Irish."

It is not our intention to follow the French authors through their most elaborate compositions. It was necessary for them to write what, happily, we may very greatly condense without damage to the question treated of. We shall collect from their papers, and comment upon the chief points which have been regarded as constituting a distinction between typhus and typhoid fever; the minor matters being such as, even if very precise information could be gained respecting them, are not worth the trouble of noticing. Having briefly alluded to the works of the French authors, we shall consider those which follow in the order above given.

The chief points which have been regarded as distinguishing typhus from typhoid fever are mentioned in Dr. Marshall Hall's late lectures. He says, "it seems probable that the *real typhus* is distinguished from *typhoid* fever by being contagious, by attacking old as well as young subjects, and by not presenting the phenomena of disease of Peyer's and Brunner's glands of the intestines." If to this we add the rosy exanthematous spots which by some are also mentioned as belonging to typhoid fever, we shall have selected all the important points on which to rest the decision of this question. If, then, it can be shown that the disease termed typhoid fever is contagious, that all ages are affected alike by it and by typhus, and if neither on the disease of the intestines nor on the existence of any particular exanthema any distinction can be maintained, the inference must be that it is incorrect to speak of two diseases, distinguished as typhus and typhoid fever.

The general evidence on contagion is one on which we have neither room nor is it necessary to enter. There seems no doubt that in the particular case before us, the same kind of evidence can be adduced in favour of the contagiousness of what is termed typhoid fever as of typhus. Our readers will not think the following facts, quoted from the essay of M. de Claubry, unimportant:

> "A man and his wife, aged each thirty years, and their two children, aged seven and nine years, were attacked with a *typhoid fever* in this village. A young woman, seventeen years of age, their servant, took the disease, and went away to her own village to be cured. This place was considerably distant from the former, and between them was no direct communication. The mother of the young woman, aged forty, and her sister, aged twenty-two, and a child, aged five years, caught the same disease whilst attending to the young woman. . . . . An epidemic broke out among the scholars of the school of Saumur, a town where typhoid fever had existed some time. Of twenty-eight scholars, who left school for the holidays and went home, and who then became very ill, eight communicated the disease; among other instances, one of these transmitted the disease to her sister, the latter to her servant, and the servant to a friend who came to see her. . . . A servant left his master to visit a relative, affected with typhoid fever, who was living at a distance of two leagues. Shortly after his return he was attacked with the same disease. A person who was nursing a child in the house visited the patient and caught the contagion. At the same time, a woman of the village, who washed the linen of the latter individual, but who had had no direct communication with either of the sufferers, complained of the exceedingly disagreeable smell of the linen which she had washed, was affected with rigors, headach, and eventually with all the phenomena of typhoid fever." (p. 120.)

In the work before us there are many other instances of this kind. They have been explained on other grounds than that of contagion; but as it regards the argument respecting the identity or non-identity of typhus or typhoid fever this is insignificant; it suffices for us that the evidence in either case is precisely of the same character, and if accepted for one must be accepted for the other. Now, having these facts before us, we may (nay, we must,) neglect any inferences which are drawn from what are called *negative facts*. It is a waste of time to consider them, and we shall therefore pass them over. But we cannot omit to quote, from M. Lombard's essay, some remarks bearing on this subject, wherein he speaks of typhoid fever. Whilst enumerating the reasons which induce him to believe in the contagiousness of this fever, he says:

> "First of all, during the four years that I have acted as physician to the civil and military hospital of Geneva, two of the male attendants on the patients have been attacked with typhoid fever. Secondly, numerous instances have occurred of the transmission of the disease to different members of the same family or to different inhabitants of the same house. Thirdly, I have followed the propagation of the disease among fifteen individuals successively affected by it. The particulars of the above fact are as follows: During the autumn of 1835, I attended the daughter of a carpenter who lived in the suburbs of the town. She was suffering from typhoid fever, together with one of her aunts who had nursed her during the early period of her disease. As their room was very small they were moved to another room

adjoining a corridor. One of the two speedily became convalescent and frequented this corridor, which belonged also to another family. In a few days, a little girl, aged twelve years, belonging to the latter family, and who was frequently in the corridor, was attacked with typhoid fever. A servant who attended on her was likewise affected with the fever, which then attacked another servant who had succeeded to the duties of the former. The sister of the last patient, who spent two nights with her, next had the fever. She was admitted to the hospital and died of the disease. A priest, who had visited the little girl previously mentioned, caught the disease and died, and from him it passed to a servant who attended him, and who likewise died. A female who had visited the girl previously alluded to, and who was circumstanced very differently, both as it regarded fortune and habitation, from the latter, was attacked with the same disease, which lasted seven or eight weeks. An English lady who lived in the same house with the last person, and was frequently in her company, had the fever, which also attacked two daughters of this lady in succession. Two other ladies living in the same house had the fever and recovered, but another was affected by it and died." (p. 17.)

In considering the question of the *age* most liable to be affected by either what is termed typhoid fever or typhus, we must bear in mind that certain ages are liable more than others to certain diseases; but that when such diseases occur at unusual periods of life, they are not on that account regarded as different diseases. On this subject M. Montault observes:

> "Youth appears favorable to the development of typhoid fever; but M. Bretonneau has seen the disease in a child of four years, and it is not rare in the Parisian hospitals to see the disease at the ages of four, six, eight, and ten years. And, on the other hand, MM. Petit and Serres have seen an old man die of typhoid fever aged sixty, Andral has witnessed it after seventy years. It is, however, particularly between the ages of twenty and thirty that the typhoid fevers have been most frequently observed. Thus of 88 cases, Louis records 54 between the ages of twenty and thirty. Chomel states, that of 117 there were 91 between the ages of eighteen and thirty, and of 74 cases observed by the author, 43 were between twenty and thirty years old." (p. 190.)

The above quotation is decisive on the question of distinction from age.

We think that the condition of Peyer's and Brunner's glands can, as little as the previous circumstances of contagiousness and period of life, be referred to as any ground for the distinction of typhus and typhoid fever. We know well, in this country, that individuals lie in the same fever wards, at the same period, having contracted a disease to all external appearance similar, with the same or nearly identical symptoms; and that, after death, in some of these cases there may be traces of intestinal and mesenteric disease, in others none at all: and this difference has fairly admitted of explanation, on the principles already alluded to, without having recourse to the theory of distinct diseases. To obtain thoroughly satisfactory *evidence* on this head requires a very extensive geographical consideration of fevers; and we believe that such a consideration would tend to destroy both the theory of symptomatic fever, and of the diversity of what is called two diseases, the fever under consideration. On this part of our subject we shall make some extracts from the essay of M. de Claubry, conveying, as they do, our own views on the subject.

In *typhus*, the inflammation of the surface of the intestines, the rosy colour of the peritoneal membrane of the small intestines in many parts, the deep redness of the same membrane, have been remarked by Pringle, Delbosq, Gilbert, Pellerin. Livid, brownish, violet, and almost black spots have been found upon the peritoneal surface of the small intestine by Fauverges, Reveillé-Parise, Thouvenel, Ducastaing, and Pellerin; and these last observers, in those parts of the internal membrane corresponding to these spots, and chiefly towards the extremity of the small intestine in the cæcum, have found erosions with elevated borders, in the centre of which the peritoneal coat was exposed by the destruction of the other intestinal coats. Others, less exact in the use of terms, and less exact in their descriptions of alterations which they have observed, speak of having found the mucous membrane inflamed, gangrenous in certain points; but M. Tort states positively that he found the small intestines scattered over with gangrenous scars. There can be no doubt that in these instances a similar change in appearance, situation, and pathological character is spoken of. Ducastaing and Pellerin speaks also of swelling, softening, and the reddish-gray appearance of the mesenteric glands, in those parts of the mesentery corresponding to the intestinal ulcerations." (p. 80.)

After having described the changes said to characterize the *typhoid fever*, and which are taken from the well-known works of Louis and Chomel, M. de Claubry continues, "if now we compare these results, are we not necessarily led to the conclusion, that in the *typhus* of camps and the *typhoid fever*, there is not merely a greater or less analogy but an incontestible identity of organic lesions?" We do not know that any additional facts from the morbid anatomy of the intestinal canal are required. The author, who takes up the opposite view to M. de Claubry, omits the evidence just alluded to, and consequently his remarks are valueless. In our own country we do not require any arguments to persuade us of the identity of these supposed distinct diseases; the evidence is constantly before our eyes. The last circumstance to which we shall allude is the occurrence of *a rash*, which has by some been regarded as distinctive of typhoid fever. On this head it is remarked by M. de Claubry:

> "This rash is constant in *typhus*. If it has not been always described, because not observed, it may be said, as an excuse, that the same eruption, so constant also in *typhoid fever*, is not once mentioned in upwards of forty cases of this disease collected by M. Petit; his attention, as well as that of M. Serres, having been directed to another series of phenomena. And it is important to observe that this rosy exanthema of typhus, sometimes limited to a small number of distinct spots upon the abdomen, exists also and in greater abundance on the chest, back, and limbs. Eighteen of the twenty-four observers whose works we have quoted speak expressly of this eruption; four speak of it as occurring after the fourth day; two after the fifth; eight in the course of the sixth or seventh; all speak of its appearance between the seventh and tenth day." (p. 75.)

We shall make no further remarks on the works of the French authors, the facts and arguments which we have adduced being such as to satisfy our own minds that other than the reasons generally given must be brought forward to prove that typhus and typhoid fever are terms properly applicable to different diseases. And on this point it is satisfactory to find that Dr. Roupell's opinion coincides with our own. He says "the impression on my mind, arising from the correspondence between the leading features as already detailed, is that the two fevers are identical."

We shall here shortly allude to some remarks of Lombard, if it be but to show the difficulty which attends the attempt to subdivide, on any satisfactory grounds, various forms of fever; and by applying to these remarks the observations which we have already made, we believe that the most satisfactory solution of the differences is obtained.

> "In arriving at the conclusions that two distinct diseases exist, a bilious fever and a typhoid fever, I have been brought to recognize that in mild cases it is exceedingly difficult to distinguish them, and that a great number of cases, commonly designated gastric disturbance (embarras gastrique) or bilious fever, are in fact cases of typhoid fever, presenting the majority of symptoms characteristic of this affection. And moreover, in one case which terminated in violent death, I found all the characteristic lesions of typhoid fever in an individual who had worked up to the moment of his death." (p. 38.)

The questions which it is especially the object of M. Lombard to answer, are: Is there such a disease as a bilious fever independently of any gastro-hepatic inflammation, and different from typhoid fever? And are there any well-marked characters by which to distinguish a bilious from a typhoid fever, or are these fevers only different degrees of the dothinenteritis? The cases, falling under the observation of any single individual are very few by which this question can be decided. But M. Lombard has met with some, having an important bearing on the question. We shall here give an epitome of his remarks.

> "A simple gastric disturbance, febrile or non-febrile, is not difficult to distinguish from well characterized typhoid fever; but if the latter is mild, and the characteristic symptoms are partly absent, the distinction becomes more difficult, and if a gastric disturbance is prolonged, is accompanied by nervous symptoms, by diarrhœa, and typhoid eruption, it is difficult to distinguish this from typhoid fever. Does pathological anatomy afford any means of distinction? Bilious fever in these diseases rarely terminates in death; but cases where the series of symptoms must be regarded as characteristic of bilious fever, have terminated fatally. It is difficult to determine absolutely the absence of gastro-hepatic inflammation in the cases called bilious fever; but if it is remembered that diuretics and purgatives must necessarily aggravate the disease, if it depended on such inflammation, one is compelled to admit that cases do exist in which the bilious element of the disease constitutes a more important part than the inflammatory. Besides, the progress of bilious fevers, which frequently last two or three weeks, without marked aggravation or diminution, may be regarded as constituting a marked difference between them and inflammatory diseases. And in the cases which have terminated in death there is occasionally found no trace of hepatic or gastro-intestinal inflammation, so that one is compelled to admit the existence of a bilious disease differing from hepatitis or from gastro-enteritis." (pp. 4,5.)

Two cases of bilious fever are here mentioned by the author. The latter ended in death, and as it is from the examination after death more complete than the former, we shall quote it entire.

> "A female, æt. fifty-eight, sent for me after having been ill fifteen days; she was then suffering from abundant bilious vomitings, frequent yellow stools, great prostration, dry, red tongue, slight headach, but no vertigo, no tinnitus aurium, no deafness; the abdomen was somewhat tender on pressure of the epigastric region, the pulse rather slow, the skin hot and burning; there was no trace of typhoid eruption. The vomiting and diarrhœa resisted all the medicines employed, and there was not a typhoid symptom during the six weeks that the disease lasted. After death, no appearance could be found by which to account for death. The mucous membrane of the stomach was neither thickened, nor injected, nor softened, but of completely normal appearance. Neither the gland of Peyer nor the agminated glands were apparent; there was neither ulceration nor recent cicatrix around the valve or in the interior part of the ileum. The large intestine was likewise free from morbid change; the liver was normal, the gall-bladder distended by liquid green bile." (p. 5.)

On these cases the author remarks that in the first, the age of the patient (seventy-four years) and the absence of every symptom of disease of the nervous centres would not allow the disease to be regarded as typhoid fever; and in the second case there could be no room for doubt, for on examination after death, no disease of the glands of Peyer was detected. The second case could not be referred to an inflammation of the liver, stomach, or duodenum, because in none of these organs was there any appreciable change attributable to inflammation; and if it is remembered that the disease continued six or seven weeks, ending in death, the anatomical appearances should have been such as to leave no doubt of their inflammatory origin; but nothing was found but a superabundance of bile in the intestinal canal: whence it must be inferred that the "polycholie" or bilious disease actually does exist, and ought to be distinguished both from typhoid fever and from hepatic or gastro-duodenal inflammation.

Having decided the first question by morbid anatomy, we come to the second: Are there symptoms of bilious fevers distinct from those of typhoid fevers? The cases related show that there are instances of bilious disease, the symptoms of which have no resemblance with those of typhoid fever. These are well-marked cases; the distinction consisting in the absence of typhoid symptoms, the continuance of vomiting and diarrhœa, and the absence of the lenticular eruption upon the abdomen. But it is far from being the case that all bilious fevers are thus marked and distinct, and in certain circumstances the reunion of the different symptoms renders the diagnosis somewhat difficult. M. Lombard says he has verified the observation of Louis, that there exist cases of typhoid fever of so light a character, that individuals so affected are not confined to their beds, but continue their usual occupations; and from all which he knows of the subject, he concludes that there are insensible degrees between the most slight cases of gastric disturbance and the most serious typhoid fever. Cases illustrative of this opinion are given, in which the gradations are exemplified, and concerning which it is remarked that it is difficult to establish between the two orders of disease any other distinction than one of degree. On this subject, and after having quoted other cases illustrative of the above views, M. Lombard concludes:

> "The facts collected justify the inference that there are insensible degrees between a simple "embarras gastrique" and the most severe typhoid fever; but it does not thence follow that there are no true bilious diseases and no true gastric derangements, because we have cited cases of this kind which have terminated by death without presenting any of the lesions characteristic of typhoid fever; only it appears very difficult to distinguish if a mild case of gastric derangement arises from a simple derangement of the alimentary canal, or if, as in a case related of a suicide, it is accompanied by a development of the glands of Peyer. Perhaps in the lenticular eruption may be found the distinctive sign of the intestinal eruption and of the bilious disease. But further observations are necessary to determine this in at all a satisfactory manner." (p. 16.)

Of the truth of this last remark we are fully convinced, and we are therefore only disposed to ask other questions in reply to the questions and statements of M. Lombard. How is typhoid fever to be defined? Is it the same as typhus? What number of exceptions is to be admitted in applying the definition? Is it necessary that there should be any absolutely uniform morbid appearances after death? In what proportion of cases is the rash to be looked for, so as to justify any precise inference from its absence alone, or from its absence together with some other symptom of equally frequent occurrence? May it not happen (supposing the disease, typhus or typhoid, to be produced by a poison,) that in the fatal case quoted above, the poison may have expended itself upon one organ in particular, "the bilious element," as it is termed, being the predominant element in the disease? But in this case, it must be remembered that, although it is said that there were no affections of the nervous centres, there was "great prostration" and "some headach." What are the cases termed by the author "bilious sub-typhoid" fever? Are they not examples of the combination of effects of the same causes? Much that we have said in a previous part of this paper may be applied in reply to the above queries and in answer to M. Lombard. But we consider that the question of a bilious fever as distinct from a typhoid fever is one concerning which we require much further information than we now possess, to enable us to decide.

The very excellent "Report on Typhoid Fever," written by Dr. Jackson, is founded on 303 cases, considered according to the numerical method. It must, as is stated, have cost its author "an amount of labour to arrive at its details, which could hardly be credited by one who never engaged in a similar work." Dr. Jackson entertains the views of Louis respecting typhoid fever, and believes in its being distinct from other forms of idiopathic fever. He says "it is plain that there are at least two species of continued fever, both in Europe and in this country; and further researches may very possibly show more." But there are no sufficient reasons given in Dr. Jackson's work for maintaining the distinction. The point mainly relied on by him is the affection of the intestinal glands and mucous membrane. We have already considered the weight which should be allowed to this condition in deciding on differences between typhus and typhoid fevers. In the course of our remarks we shall have other opportunities of referring to Dr. Jackson's work, which, however, does not contain much matter of interest to the reader. We should, however, not omit to mention that the usual affection of the glands of Peyer is mentioned as having been found in all the fatal cases of typhus mentioned by Dr. Jackson.

In the preface to Dr. Roupell's work, it is stated that its objects are "to assert the claim of the prevailing epidemic to be ranked among specific fevers, to separate it from some with which it has long been improperly confounded, to show at the same time its analogy with others, and to improve the pathology of all." Dr. Roupell remarks that, "it is owing to a certain specific cause. For, when closely observed, it has been found to pursue a certain definite course, passing through its stages with regularity, spreading by infection, and being marked in its progress by a distinctive rash. Here then we have all the characteristics of the genuine exanthemata of authors, to which class it seems correctly and exclusively to belong."

"Improving the pathology of all fevers" is an object well worthy of the efforts of any physician; for, much as we do know of them at present, there remains very much with which we somewhat vainly seek to become acquainted.

There is much of Dr. Roupell's book confirmatory of an opinion, not originally but somewhat decisively, stated by Dr. Peebles, in the forty-fourth volume of the Edinburgh Medical and Surgical Journal, and which we are surprised that Dr. Roupell did not notice. Dr. Peebles writes as follows:

> "I have endeavoured to trace the history of the petechial contagious fever which has been the subject of our enquiry, and I trust that I have shown that such a disease existed from the earliest ages, that the fever is of a peculiar type similar to that of exanthematous diseases, and particularly that it is accompanied with an eruption as distinct and specific as any other *exanthema*, and which should be considered as pathognomonic of the disease; that it is produced by a miasm or poison *sui generis*, and is capable of propagating itself like other contagious diseases. In bringing our observations to bear on the contagious fever of this country, we shall find that the leading characters of the petechial disease and the sources from which it flows are very much the same with those of our typhus fever. The sthenic or irritative sthenic type of the fever early in the disease, the tendency to the asthenic and putrid state towards the latter stages, the sudden and great prostration of strength, the condition of the tongue and eyes and the expression of countenance, the constant invasion of the cerebral and pulmonary system, and particularly the exanthematous eruption which always accompanies the fever, are characters common to both. This eruption has been already noticed occasionally by some practitioners in this country when the fever became epidemic; but, as I have stated, I have found it as constant as any exanthema of other eruptive diseases, and during two years of attentive enquiry I have found the contagious fever of this country to correspond very closely in the above characters, with the difference that the exanthematous affection is less profuse in a majority of cases. This may arise from the disease not being epidemic at present, for it is then much more distinct and copious. But the character of the eruption and the period of its appearance are still precisely the same. Dr. Lorimer, Physician's Clerk at the Royal Infirmary, who has assisted me for a year and a half in examining fever patients on their admission, informs me that he has observed the exanthema in every case, with a few exceptions, when the patient was brought into the hospital before the eighth day of the fever. (p. 373.)

All the remarks of Dr. Roupell are confirmatory of the above view. And that in certain localities (in the two instances just alluded to, in large and crowded cities,) the fever termed typhus may be marked by the chief characteristics of an exanthematous fever, must be admitted. It remains to be seen whether, in other parts of the country, the same phenomena characterize the disease; and, as the general attention has been more called to the subject, it is to be hoped that the condition of the skin and the series of symptoms occurring in the disease will be more accurately noticed and defined than has hitherto, too frequently, been the case. Dr. Roupell states that in his note-book "the rash is recorded in 70 out of 100 cases promiscuously taken, no mention being made of it in the remaining 30; again, out of 100, all of whom had the rash, it was present in 86 on their admission, and showed itself subsequently in the rest." We quote the following from Dr. Jackson's Report as valuable evidence, especially because he has no theory to support.

> "Rose spots were not noticed until the year 1833. In that and the two following years they were noticed in 70 cases; in some of these, however, they were very few in number. Sometimes only three or four were seen; but in a few cases they were numerous, covering the trunk, and appearing even on the limbs. In the years 1833-35 inclusive, there were 106 cases, so that (106 ÷ 70 = 1·51), 1 in 1·51, or about 2 in 3 had rose spots. I am not at all satisfied, however, that they did not escape notice in some cases."

The following is the description given by Dr. Roupell of the rash, which must be carefully distinguished from petechial spots.

> "It is of a red colour, the shades of which are various: in some cases bright and vivid, more generally, however, dusky and undertoned; if the rash is fully developed, the cuticle is slightly elevated; and when the vessels are very turgid, the eruption is sensible to the touch. It is most commonly found on the chest, trunk, and limbs; sometimes on the face, and was noticed in a recent case to have reached and occupied the scalp; nor is it confined to the outer surface of the body, but extends itself to the lips and lining membrane of the mouth..... It appears in spots or patches, circular in form, and varying in size from that of a pin's head to that of a pea. No itching attends its presence on the skin, nor is desquamation of the cuticle an ordinary consequence.....The duration of the rash in typhus, according to M. Chomel, is from three to four days; should any mottling of the skin appear after this period, he attributes it to a fresh eruption. It may here be observed, that in some cases, when the rash could not be perceived, but in which it was deemed advisable to bleed the patient, and a ligature was applied for that purpose to the arm, the eruption appeared below the tape. Acting upon this idea, I have at other times been able to exhibit it by artificial congestion of the vessels." (pp. 35-37.)

That the rash should be occasionally or even frequently absent need not surprise those who are conversant with exanthematous fevers; especially if they are considered in their analogies with other diseased actions produced by poisons upon the organism. The frequent occurrence of sore-throat, during the prevalence of scarlatina, with the scarlet injection of the mucous membrane of the fauces is, amongst others of a similar kind, a fact of great importance in reasoning on the nature of exanthematous fevers; and a little reflection will convince us that the condition of the skin in these diseases may have had more attention paid to it than equally uniform changes in other parts.

We are far from determining on the universal occurrence in typhus fever of those peculiarities mentioned by Dr. Roupell. Our disposition is to believe that the disease described by Dr. Roupell, Dr. Peebles, and others, is essentially the same as that which is generally denominated typhus, modified according to circumstances to which we have already alluded; but it is an inference which cannot, at present, rest on any satisfactory grounds. Before determining the question as to the propriety of classing typhus among exanthematous fevers, we must obtain far more extensive information than we already possess. From what we do possess we are disposed to admit that, with probably greater irregularity as to the course of the disease, less protection against its recurrence, a greater variety of morbid phenomena, and probably a more extensive sphere of action, typhus is closely allied to exanthematous fevers. Dr. Roupell's experience is amply confirmatory of the infectious character of the disease. Numerous cases are related of typhus with hemorrhage from various organs, with erysipelas, with purulent deposits in the cellular membrane, with carbuncle, with extensive sloughing, with sloughing phagedæna, and other morbid changes, both organic and functional. Dr. Roupell is disposed to account for many of the symptoms of typhus in its worse forms by supposing that pus is mixed and circulates with the blood.

> "We see that inflammation of a vein and admixture of morbid secretions from the inflamed part with the blood causes, on ordinary occasions, typhoid symptoms, and gives rise to a fever, attended by subsultus tendinum, low muttering delirium, and a

> black tongue. The similarity of the symptoms in phlebitis and in one stage or period of typhus gives reasonable ground for belief that the corresponding appearances in the two diseases arise from a not dissimilar cause, and hence we see an explanation of one great train of effects in typhus, and learn how typhoid symptoms, appearing in various diseases, both chronic and acute, may easily and naturally be confounded with real typhus. Hence we see how readily what is called spontaneous typhus can be generated." (p. 124.)

This quotation, which affords another instance of the necessity, now more generally felt, of conceding to the fluids a large share in the production of various diseased states, receives further confirmation from the observations of Dr. Ferguson, recorded in the last Number of this Journal. We particularly allude to his remarks on that which he terms the complicated form of puerperal fever. Dr. Roupell has taken great pains to establish the fact of an inflammation of the minuter vessels, and the results of such inflammation, as the cause of the phenomena above alluded to. We recommend to the perusal of our readers the sections wherein, both from facts directly bearing on the subject and arguments from analogy, he endeavours to establish his theory. Indeed, throughout Dr. Roupell's volume there is much ingenious speculation, the result of much thought on the intricacies of fever, and which are well deserving of attention. But we must confess that—seeing the many objections which may be brought forward against all existing theories of fever, and, as we are about to occupy the attention of our readers with a very ingenious speculation by Dr. Buzorini on the nature of typhoid diseases, which, however, we offer chiefly as an ingenious curiosity,—we are little inclined, at present, to expend more time and space on mere speculation.

There is a useful chapter in Dr. Roupell's work, on the morbid appearances in typhus; and we refer with much satisfaction to some of his remarks on the treatment of the disease. Especially do we regard it as a truth which cannot be too strongly impressed upon the mind that the disease has a specific origin. The following remarks are important:

> "The importance of ascertaining the origin of a complaint is of the highest value with respect to treatment. The blood-shot eye, the pain and increased temperature of the head, attended by delirium, offer inducements to depletion, which it is very difficult to resist, and which it would be most unpardonable to overlook, if the disease had not a specific origin. But when once we are satisfied that all the above symptoms arise from the introduction of a poison into the system, we feel that much more important objects are to be obtained than the mere subduing of inflammation, even although we should feel satisfied that such a condition did actually exist. We know that a cause affecting the whole system is now in operation, and that the reduction of action in one part is doing very little, whilst the causes giving rise to it still remain and are capable of producing it elsewhere. A specific disease cannot be cured by depletion; over-excitement can, indeed, be controlled, but the disease still remains in operation." (p. 230.)

We need not allude to the contradictory opinions entertained respecting bleeding in typhus. Dr. Roupell is an advocate for bleeding in the earlier stages of typhus; but this advocacy is modified by the caution which, respecting the same remedy, should be applied to all specific infectious, eruptive diseases. Emetics he considers as valuable.

> "The combination usually employed by me is ipecacuanha with antimony; but should the patient be weak and feel such nausea as to induce the belief that emesis will speedily be induced, ipecacuanha may be given alone, its effects being aided by

diluent drinks.... The result of this method of treatment is often to relieve the head-ach, induce sleep, provoke perspiration, stop at once all disordered action, or mitigate the formidable indications. Few remedies, in truth, exert such beneficial influence over the disease as emetics, and, with the precaution mentioned above, they are almost universally applicable, and open to no objection whatever." (p. 242.)

Purgatives, carefully employed, are considered by Dr. Roupell as equally valuable with bleeding and emetics. Mercury in ulceration of the bowels is spoken very highly of.

> "There is no remedy which can compete with the hydrargyrus c. creta, when ulceration has taken place in the bowels.... The good effects of this remedy in inflammation of mucous membranes is well known, and, when combined with Dover's Powder, as employed at St. Bartholomew's Hospital,* constitutes an excellent formula; given at intervals of four, six, or eight hours, it allays abdominal irritation, checks diarrhœa, and calms general constitutional disturbance." (p. 247.)

The principle laid down by Dr. Roupell for the administration of stimulants is "to maintain strength enough in the heart for the circulation to continue;" to give them only during the existence of real debility. The state of the pulse and coldness of the extremities are the only signs mentioned as indicating the condition when stimulants may properly be administered; but this is not satisfactory as a guide. It is by no means necessary that the extremities should be cold when stimulants are imperiously called for; and the state of the pulse, apart from that of the heart, would give very inadequate assistance in deciding on this most difficult point of practice. Dr. Roupell was, no doubt, unacquainted with the highly important observations of Dr. Stokes on this subject, noticed in our last Number; but we must call the reader's particular attention to them. If, even after Dr. Stokes's remarks, it were true, as Dr. Roupell remarks, that there is no positive sign to go by in this matter, still he might have made the section on stimulants far more useful by mentioning certain conditions, very misleading to the young practitioner and tending greatly to perplex his mind, with which stimulants are not incompatible; and especially should proper cautions have been laid down as to the mode of their administration, and the effects from which we may safely conclude whether or not they should be continued or abandoned. The following remarks on this subject, contained in Dr. Jackson's report, are worthy of attention:

> "When a patient is induced to take cordials (vinous liquors) reluctantly, they seldom benefit him, and are often followed by injury. When he is greatly enfeebled, at a late stage of the disease, he may be safely asked if he wishes for them, and if he does, he may try them; they will seldom hurt him then, if he takes no more than is grateful to him. When he spontaneously demands them, as late as the third week, they will almost always be found useful."

On the employment of opium we must quote some observations from Dr. Roupell, which greatly coincide with our own opinions.

> "If there is one remedy of which the advantage is undoubted—if the immediate subsidence of untoward symptoms follows the exhibition of any medicine—if future amendment be the test of utility, then is opium a valuable remedy; but it is not to be employed largely at any period, nor resorted to in the first stages, nor when stupor is present or approaching; but if given at that period of the disease when there is

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* This consists of equal parts of hydr. c. creta and Dover's Powder, made into a pill with conserve of roses.

increased action without fever, and all the signs which indicate irritation, good will invariably result." (p. 266.)

We should be disposed somewhat to extend the limits here prescribed to the beneficial action of opium, having found great benefit from its use even in the early stage of fever, when an intensity of headach has existed quite out of proportion to the febrile symptoms generally. And we believe that the advantages derivable from the use of opium in the practice of medicine are not at all sufficiently understood; but that certain fears, founded on its supposed physiological action prevent its being employed in many instances where it might be productive of very beneficial effects. On this account we have read with much satisfaction some remarks in the volume lately published by Dr. Holland, which coincide in the main with opinions which we have long entertained. There is nothing of which we are more convinced than that the usual teachings of lecturers and writers on materia medica will, as far as opium is concerned, be found contradicted by actual experiment.

We now come to the consideration of the works of the German authors, which are widely different from those which we have hitherto noticed. It tends to make a reader very tired of theories, when he finds that almost every writer on the subject of fever has his own to support. Dr. Kehrer has attempted to establish a distinct form of fever, which, when existing in its simple state, consists in a condition of irritation of the blood. Dr. Buzorini has devoted a large part of his work to a consideration of the morbid condition of the blood in typhus fever, and in diseases allied to this, and to point out analogies, of which he conceives the existence, between it and blood when deprived of life. He has revived the doctrine of crises, and considers them as essential to the cure of all typhoid diseases: but, as such crises may occur gradually or accompany the whole course of the fever, they are not necessarily very obvious, and may escape any other than the most minute attention. We shall present our readers with a short analysis of his views, with which they cannot fail to be pleased, from their ingenuity, however little they may be satisfied of their truth. It becomes necessary, in fulfilling our office as reviewers of foreign medical literature, to notice occasionally speculations of some ingenuity, but which are open to many objections; and these objections are sometimes so obvious that it is unnecessary to particularize them. There is another reason why we analyze the work of Dr. Buzorini. The electric condition of the animal-body, and its electric relations with all that surrounds it, are subjects of considerable observation with some foreign physicians, as will be seen from our account of M. Goudret's work, in the present Number. Some Germans also believe that, with respect to erysipelas, rheumatism, and other diseases, very opposite electrical states of the body have been found to exist: and, in the history of the causes of disease, the electric condition of the atmosphere forms, among some of them, a very prominent subject of speculation. Many have attributed the sudden appearance and rapid extension of influenza to such a cause, and have endeavoured, with perhaps more ingenuity than success, to associate these with planetary influences and certain mundane phenomena, attended with the development and concentration of electricity. Dr. Buzorini has, both in explaining the development of typhoid diseases and their nature, made considerable use of such electric phenomena. Some, at least, of his observations on this subject will not be unacceptable to our readers. It requires but little reflection to be convinced that our bodies are in a certain relation with electricity, with which we are little, if at all, acquainted. The sensations of oppression which precede a storm attended with lightning and thunder, enable many to predict its approach with certainty, and the sense of lightness and activity which follows such a storm are very generally acknowledged. Few cannot but have noticed the development of electric sparks which takes place when, in certain states of the weather and (we believe) under certain conditions of bodily feeling, we rapidly remove a flannel garment from our bodies. But it is not with electricity in such abundance that Dr. Buzorini and others among his countrymen have to do. The quantities which become the subject of some of their experiments are only appreciable by the electro-magnetic multiplicator; but, by means of this instrument, it is stated by them that they become very perceptible. Without, however, any further preface, we shall briefly notice the works before us.

Dr. Kehrer has endeavoured to establish, under the name of Febris hæmatodes, or blood-fever, a distinct species of this disease. Its characteristic symptoms he states to be *a frequent, large, and soft pulse; general heat of body; abundant evacuations from various organs;* and the negative characteristic, that *no tissue is visibly changed in its structure.* The nature, essence, or proximate cause of this fever is regarded as evinced by the above symptoms, which are considered as indicating a condition of direct irritation of the whole mass of the blood, of which the globules are said to be especially affected; their morbid change consisting in apparent turgidity. The relations borne by this fever to other forms of fever are as follows: *Synocha* (it is said) affects the solids; *nervous fever* affects the nervous substance; *blood-fever* affects the blood. In synocha, there is inflammation of tissues, with its characteristic effects; the local actions call forth the constitutional; there is a considerable manifestation of power, and a coating in the blood. In nervous fever, there are symptoms of disturbance of common sensation, the nervous system being involved. The Febris hæmatodes is opposite in its character to both: it commences by irritation of the general mass, with local congestions of the blood. The simpler the course of this fever, the less do the nerves and other solids participate; but it is sometimes connected with nervous and synochal fever, and in such cases its characteristic symptoms become variously modified. The *largeness* of the pulse is especially alluded to as characterizing this fever, and as depending on the condition of the blood which belongs to it. A sufficiently fanciful explanation is given of this somewhat undefinable characteristic of *largeness.* The parietes of the arteries are supposed to be very extensible and extended; the blood itself to be in a state of turgidity; a condition of independent vital extension, distinct from the state of plethora in which the pulse is said to be *full.* This condition of the blood, it is well observed, does not admit of demonstration, because it ceases with life, or when the blood is drawn from the body.

We have given the above outline of the fever of Dr. Kehrer, which, however, we do not regard as grounded on correct observation. The notion of a simple condition of irritation of the blood, manifesting itself by turgidity of the whole mass, and this blood circulating through the body without affecting the solids, and so constituting a fever which is capable of distinction as blood-fever, is too hypothetical to merit any serious contradiction. We are told, also, that a characteristic of this fever is the absence of any visible structural changes; but that, during its course, the tissues of various organs are implicated,—this implication, however, being only *secondary*,—i. e. *the effect of too violent a critical concentration*. This may or may not be, but the affirmative is not proved; and, as it is neither agreeable nor profitable to stir the question of the origin of fever which is involved in the above, and without a consideration of which (for which we have no room) we could not arrive at any satisfactory conclusion, we shall pass on to the second treatise.

The work of Dr. Buzorini is characterized by very considerable ingenuity, if it is worthy of no higher commendation; and, although its perusal may not throw any more light on the essential nature of fevers, it will afford an example of the mode of speculation in which many German physicians indulge, and of the connexion, of which it is endeavoured to prove the existence, between vital, physical, and chemical processes in the production and phenomena of disease: and it may not be without its advantages in directing attention more to the condition of the blood in fever; the prevalent disposition to regard the solids as the parts chiefly affected therein, either primarily or secondarily, having unquestionably too much drawn away the attention from changes in the blood. We shall therefore present our readers with an analysis of Dr. Buzorini's work, and in as few words as possible.

The word *typhus*, Dr. B. remarks, was applied by Hippocrates, not only to fever with stupor and coma, but to other diseases attended with diminution or loss of sensibility. It is here applied in the same sense; and, by adding *sepsis* (from σηπω, putredinem accerso,) he forms the word *Typhosepsis*, which is intended to designate a family of diseases termed *Typhoseptoses*, and of which typhus (so called) is only one species. *Typhosepsis* is defined as "a morbid process constituted by a tendency to paralysis of the nervous system and of the blood, by the phenomena of reaction of the organism, and by the effects of this combination." Under this definition Buzorini includes various diseases as *local* Typhoseptoses. Typhus fever is regarded as a universal Typhoseptosis; so that it offers all the morbid phenomena and changes which separately constitute the local forms of this diseased process.

There are two constituents of this Typhosepsis, *a lesion of the nervous system* and *a lesion of the blood*. The various known changes in the nervous system are here alluded to, such as injection, change of colour, consistence, &c.; and an altered specific gravity of some parts of the nervous system is said to be one of the most frequent changes observed therein. That some alteration has not always been found is ascribed to want of care in conducting the examination; but it is added, that the most constant sign is change of specific gravity of the brain. The character of the nervous symptoms is feebleness and tendency to paralysis, termed *Neurasthenia typhica*. In addition to the affection of the nervous system, morbid changes in the blood are required to constitute Typhosepsis. The following appearances, in various degrees, are observed in typhoid blood: In the dead body, it is fluid, dissolved, remarkably dark, and little disposed to coagulate. If the disease be much developed, gas is found in the blood, and small vesicles containing air are formed in the veins. The blood smells offensively during life. If death occur during the later stages of typhus, the quantity of blood is greatly diminished in proportion to the size of the body. If blood drawn from a vein be examined, it is seen to be darker than in the healthy state. In the early part of the disease it coagulates quickly, but slowly when the disease is more advanced; and the coagulum is always soft, loose, and easily torn, small in proportion to the serum, and it more readily putrefies than healthy blood. The solid constituents of the blood are diminished. The serum of healthy blood contains, according to Prevost and Dumas, 10·0 albumen, 90·0 water, in 100 parts. In 1000 parts of blood taken from a typhoid patient, Buzorini found from 48 to 60 parts only of albumen. There is likewise a diminution in the quantity of fibrin and colouring matter in the crassamentum. On the other hand, the watery constituents exist in a larger proportion, and the salts are diminished. Buzorini's experiments accord very closely with the following table, published by Dr. Clanny.

Constituents of Blood.	In the Healthy State.	In Typhus Fever.		
		First Stage.	Second Stage.	Third Stage.
Water . .	678	729	772	732
Colouring Matter	160	136	122	130
Albumen . .	121	98	75	101
Fibrin . .	28	25	22	26
Salts . . .	13	12	9	11
	1·000	1·000	1·000	1·000

And he has likewise found, he assures us, that the proportion of solid constituents of the blood increases with the favorable progress of the case. The specific gravity of the blood is lowered with the loss of its solid constituents. When the blood of a fatal case of typhus is examined by the microscope, the globules are seen to have lost their normal form, to have increased in size, and to be without the proper sharpness of their edges. Now, in putrid blood, the globules become rounded, and break or are dissolved; and this circumstance is regarded as pointing out the condition of the blood in typhus: that there is a diminution of the vitality of the blood, shown by its resemblance to blood undergoing decomposition after death. This condition of the blood has been termed increased venosity, scurvy, putridity. It is a tendency, more or less, to *paralysis* of this fluid, a consequence of which is that the blood becomes subjected, in various degrees, to the chemical laws of unorganized bodies. Typhus is regarded as a combination of the above lesions of the nervous system and of the blood. Instead of reaction (such as occurs in simple vascular irritation,) there is a condition of passive vascular irritation, diminishing still more the life of the nervous system of the part. This acts again, by increasing the paralysis of the blood. From this disposition to mutual increase arises the impossibility of a return to a normal condition, until the dead constituents of the blood are thrown off. The changes produced by the congestions in various organs are, if superficially examined, like those of inflammation; but, from the diminished influence of the nervous system and the septic state of the blood a true inflammation is not possible. There is no effort to form new vessels; no organized formations or secretion of pus. The less the typhoid character of the disease is developed, the nearer is the approximation of the phenomena to those of inflammation. As the blood becomes more fluid, organs become softer and more disposed to decomposition,—e. g. softening of mucous membranes, gangrene, putrescency, &c. The more life that remains in the blood and nervous system the less influence has the chemical action, and *vice versâ*. Thus there are two actions constantly going on. The decomposition of blood drawn from a vein is first mechanical,—i. e. coagulation. So also in typhus, when the influence of the nervous system is diminished, the blood separates into fibrin, colouring matter, and serum. The vital power, we have seen, is in proportion to the quantity of fibrin and albumen: so, in typhus, these are diminished; for, as in dead blood, the fibrin and albumen separate, and nature tries to expel them by stool and by membranous secretions in the canal. The fibrin cannot become organized: it is dead. The mucous membranes of the intestines are the chief excretors of albumen in typhus: sometimes this is exuded between its coats, causing ulcers. If the exudation does not take place by the mucous membranes, it occurs by the serous membranes and the cellular tissue. There is an attempt to separate the serum by the skin and serous membranes; and the latter excrete albumen when it does not pass off by the mucous membranes.

The salts of the blood pass off by the kidneys, and also in the excrements; but, in the more developed typhoid cases, the salts do not appear in their original form, but they are separated into acid and base; as, when a stream of electricity is passed through the blood, the acid appearing at one pole, the alkali at the other. In typhus, much free electricity is found on the skin, and the electro-galvanic multiplicator shows this to be galvanic electricity; and hence the reason that the acids appear on the skin at the positive pole, and the alkalies in the intestines at the negative pole; and *vice versâ*. Thus we find acidulous saliva, ammoniacal urine. This is the point of relationship between rheumatism and the Typhoseptoses: in the former, acid secretions (uric, purpuric, rosacic acids,) are frequent; and rheumatic phenomena are not unfrequent at the commencement of typhus. The sudamina, so frequent in typhus, are explicable, together with their acid contents, by the above electric acid. The diminished quantity of salts in the blood admits of a similar explanation. The changes alluded to in the globules of blood occur with the increased quantity of water and the diminished quantity of salts, and evince most remarkably a decomposition similar to that which is undergone by dead blood. The dark sordes on the tongue and gums are owing to excretion of the separated colouring matter. The condition of the blood explains the remarkable prostration in typhus. As the disease advances, all parts of the body become implicated, the bones as well as the softer parts: hence not only are there animal matters in the excrements, but a quantity of phosphate of lime. If the paralysis of the blood is more advanced, gas is developed in it: gas is also sometimes developed beneath the skin, under the form of emphysema; and, in a body dead from typhus fever, an emphysematous condition frequently occurs in a few hours.

The *calor mordax* is the result of an electro-galvanic process, whereby the latent heat in the blood tending to putrefaction, is disengaged. That electricity can develop warmth from blood is shown by the experiments where the temperature of blood drawn from a vein is increased, as soon as the wires of a small galvanic battery are inserted. We know, from the experiments of many physiologists, the influence of electricity on vital phenomena; and it is also known that a constant galvanic process attends vital action; and we likewise know that, in every chemical process, and especially in the transition from an organized to an unorganized state, electricity is the operative power: and thus we observe, in this morbid process, that the organic electricity is changed or increased. The animal organism develops more or less electricity, positive on the skin, negative on the mucous membranes; as has been shown by the electrometer and electro-magnetic multiplicator of Schweigger and Poggendorf. In typhus these electric relations are variously modified. The electrometer of Bohnenberger frequently shows much free negative electricity on the skin; and this is the case where the disease has a tendency to develop itself externally, and when violent sweatings and exanthematous phenomena occur. On the electro-magnetic multiplicator the action of the electricity which is present is very great and very remarkable. We find also that the kind of electricity is connected with the development of the diseased process: thus, when decomposition is great, the electro-galvanic prevails; whilst, in less advanced forms of the disease, the organic electricity shows greater physical power: as in other cases we find that a quantity of electric fluid, capable of communicating a violent shock, possesses very little chemical action, and *vice versâ.* Among the prevailing physical actions, we see exanthemata originate in typhus, and the form is dependent on polarity; whilst among the chief chemical actions are strongly developed putrescencies, and which, according to the electro-galvanic polarity, decide the acid or alkaline nature of an excretion. Thus, for example, the skin, in a normal state, is positively, and the mucous membranes are negatively electric; but in typhus these poles are sometimes reversed, and the ordinarily alkaline secretions of mucous membranes and salivary glands become acid, and the usually acid secretions of the skin alkaline or neutral.

The degree of reaction depends on the energy of the blood and of the nervous system. If the termination be favorable, it is by crisis, by sweating, eruptions, or evacuations from the bowels, &c.; but, as the *vis medicatrix* is constantly acting to restore the proper condition, from the commencement of the disease, the effect is sometimes slowly produced, and crises are not always witnessed.

Atmospheric influences must be regarded as among the most important causes of typhus, and first among these *atmospheric electricity.* Chemical processes, evaporation connected with chemical action, vegetation, &c., are productive of positive electricity. As the vapours ascend, positive electricity ascends with them, and leaves the earth in a negatively electric state. Hence the quantity of positive electricity increases with the height, whilst in the lower strata a portion of the positive electricity is connected with the negative electricity of the earth: but, as the atmosphere is a bad conductor, the whole of it may be regarded as a Leyden phial, the positive extremity being above, the negative below: whilst we live in the isolator, in the intermediate air. Hence the organism is more or less protected from the influence of electricity. Atmospheric electricity, which discharges itself by showers, has less effect upon the organism: but, if the cloud is so near the earth as to touch it, and we are in that cloud, the electricity may act upon us: and such is a mist or fog. The dampness of the respiratory apparatus enables it to conduct this electricity; so that, by every inspiration, free electricity comes in contact with the blood, and, by a continued action, influences the chemico-vital composition of that fluid. Mists, damp places, are therefore one among the causes of typhus. Diminished *atmospheric pressure* (a low condition of the barometer) diminishes also nervous influence, and becomes, therefore, very favorable to the development of the nervous constituent of typhus. Moisture of the air alone does not develop typhus, but only in connexion with increased atmospheric pressure and electricity.

Applying what has now been said to forms of disease, typhus fever is regarded as a universal Typhoseptosis; the neurasthenia typhica and the lesion of the blood being so far developed as to implicate the whole organism with its power of reaction; whilst, in the local forms, the whole blood is disturbed, (although in a less degree,) but the nervous affection is limited to a single organ. There is a morbid reaction, approaching in its character to inflammation. We also see a disposition to concentration in typhus. From what we have already said, it is unnecessary to allude to the symptoms in typhus fever, which the author quotes as illustrative of his views. The following is his explanation of the affections of the glands of Peyer and Brunner:—The *vis medicatrix* commences (with the development of the lesion of the blood) its endeavour to throw off the albuminous parts; but, if these be in too great quantity, albumen is deposited in the intestinal follicles or between the coats of the intestines, and hence the enlargement of Peyer's and Brunner's glands. If death happen within five days of the attack, and these glands are cut through, the upper layer is found to be mucous membrane perfectly normal, except that, from being stretched by what lies beneath it, it is somewhat thinned. Beneath this membrane is a layer, from one to three lines in thickness, which consists of fibrin, some phosphate of lime, lactate and hydrochlorate of soda, and traces of other salts. A similar process occurs in the glands of the mesentery and mesocolon. From the fifth to the ninth day, ulceration of the mucous membrane takes place, and leaves the whitish matter exposed. This deposition often takes place beneath the vascular membrane; likewise ending in ulceration and discharge. The mucous membrane and ulcerated surface secrete fluid, partly serous, partly membranous, which comes away as diarrhœa: the flocculent part consists of albumen, fibrin, phosphate of lime dissolved in the serous part, which consists of water, salts, and some uncoagulated albumen. As these pathological phenomena are the consequences of reaction of the organism, the nervous power being oppressed, it is clear that they may occur in other diseases where these conditions exist, although not exactly in the same degree as in typhus. Thus are there similar phenomena in Asiatic cholera, in scarlet fever, in phthisis.

Typhus, in its tendency to concentration, shows a great disposition to effect the destruction of an organ. The less developed the Typhosepsis, the nearer is the action to true inflammation. The highest degree of its development is the destruction of the organization of a part,—gangrene. Almost all organs become softened, and some to such a degree that their texture is lost. Such is gelatiniform softening. Typhus presents various appearances, from the greater prevalence of one or more of the phenomena of general Typhoseptosis; so that every epidemic has in it something peculiar to itself. The pathogenic characteristics are, however, never absent, but in various degrees form the groundwork of all its varied phenomena.

According with Buzorini's views of the pathology of typhus are the indications of treatment: 1, to counteract the paralysed state of blood and nerves; 2, to encourage the excretion of deleterious matters from the blood, by the most natural channels; 3, to moderate reaction of the organism; 4, to oppose concentration on individual organs. We shall quote from the author's remarks on remedies those only which relate to ipecacuanha. He says that the radix ipecacuanhæ possesses a specific power of exciting the nervous system without any injurious secondary action, which is contraindicated by the typhoid state; and that it is, consequently, a very valuable remedy in typhus. It acts little, if at all, on the circulation of the blood, whilst it somewhat excites and strengthens its vitality: it consequently increases the activity of the normal excrementitious organs, such as the skin, mucous membranes, liver, salivary glands, and kidneys, and somewhat retards the evacuations by stool. It is proper in all stages of typhus. It moderates the nervous symptoms. cuts them short,—puts an end to mild cases in the beginning,. by encouraging excretions,—and acts so favorably on the nervous system that restlessness and delirium often speedily cease after its use, or are very much diminished. Its beneficial action is less evident in worse forms of the disease. Buzorini has employed ipecacuanha in typhus for a period of ten years, with the most favorable effects. The form of its administration is an infusion of from one to two scruples in from five to seven ounces of water, daily. It does not excite vomiting in typhus, as it would do in other conditions; and when this happens it is regarded as a favorable sign.

We have no room for what the author adds on local Typhoseptoses. What we have already given has been partly on account of the application which it contains of electricity to morbid phenomena; a circumstance which, if it be too much regarded by some German physicians, is far too lightly estimated by ourselves; partly because, unless we occasionally afforded our readers a specimen of this kind of foreign medical literature, we could scarcely be regarded as its impartial reviewers. There are so many obvious objections to the theory of Buzorini, that we have merely given it without note or comment: and if, in the application of the theory to typhus fever, there is a want of evidence, much more is this the case with the local forms of what are termed Typhoseptoses: for instance, hospital gangrene; stomacace; diphtheritis; cynanche trachealis; dysenteria; febris puerperalis; hydrocephalus; trismus neonatorum, &c.

Art. X.

A Treatise on the Diseases of the Heart and Great Vessels, and on the Affections which may be mistaken for them; comprising the Author's View of the Physiology of the Heart's Action and Sounds, as demonstrated by his experiments on the motions and sounds in 1830 *and on the sounds in* 1834-5. By J. Hope, m.d. f.r.s. *Third Edition, corrected and greatly enlarged.—London.* 8vo, pp. 639.

Since the period of the first publication of Dr. Hope's Treatise in 1832, it has been so widely disseminated, through the medium of two London editions and several foreign editions and translations, and by the transference of its most valuable matter to the pages of the Cyclopædia of Practical Medicine, that it is quite unnecessary, on the present occasion, to occupy our pages with any account of the general plan and character of the work. We shall therefore confine our notice to the new portions of the present edition. It is right to state, however, that this is by no means to be regarded as a mere republication of the previous treatise. From the careful revision which its whole contents have undergone, and from the addition of at least a third of new and valuable matter, the present may, in reality, be considered rather a new work than a new edition.

The following is the author's enumeration of the additions; and we shall find it convenient to examine them in the same order:

> "1. The natural sounds of the heart. 2. The sound of costal percussion, with or without tinnitus. (*Laennec's Cliquetis.*) 3. Murmurs from valvular disease, and the whole subject of particular valvular diagnosis, which will now, I confidently hope, be found one of the most simple and easy departments of auscultation. 4. Murmurs of the heart and arteries independent of organic disease. 5. Venous murmurs. 6. Musical murmurs. 7. Abdominal murmurs, both connected with pregnancy and otherwise. 8. Tremor or thrill of the heart, arteries, and veins. 9. Signs, general and physical, of pericarditis and endopericarditis. 10. Connexion of diseases of the heart with apoplexy, palsy, &c. 11. Partial dilatation or real aneurism of the heart. 12. The signs, physical and general, and the pulses of softening. 13. Signs of adipose disease of the heart. 14. Aneurisms of the aorta bursting into the pulmonary artery and right ventricle. 15. Abdominal aneurisms. 16. Anæmic, nervous, dyspeptic, plethoric, bilious, and other sympathetic affections of the heart, with their diagnosis. 17. Displacements. 18. The pulses of disease of the heart." (Preface, pp. v.-vi.)

The Natural Sounds of the Heart. The additional matter introduced into the present volume, under this head, consists of an elaborate examination of the immediate causes of the two sounds. No alteration we think is made in that part relating to the motions of the organ with which the sounds correspond; this part of the subject being so well illustrated and confirmed by experimental evidence, as to preclude all contradiction or doubt. We think also that there can hardly be any difference of opinion as to the cause of the *second* sound; the result of all the numerous experiments going to prove that this sound is caused by the reaction of the blood on the semilunar valves during the ventricular diastole, as first announced by Dr. Carswell.

Dr. Hope attributes the first sound to three causes: 1, "*the sound of muscular extension;*" 2, "*bruit musculaire ou rotatoire,*—the dull, rumbling sound of muscular contraction;" 3, "*the sound of valvular extension*," the most important, he says, of the three. We will briefly recapitulate the evidence on which Dr. H. grounds these opinions.

1. By the term "muscular extension," he says that "he means a loud, smart sound, produced by the abstract act of sudden, jerking extension of the already braced muscular walls, at the moment when the auricular valves close; in the same way that when the valve of a pair of bellows closes, its leather is put on the stretch, and if not rigid produces sound." This sound differs from *bruit musculaire*, inasmuch as it may be produced in dead muscle, in which bruit musculaire obviously could not exist. The grounds on which this sound of extension has been adopted are the following: *a.* In Dr. Hope's experiments, and in those of the Dublin and London Committees of the British Association, the walls of the ventricles, during each systole, became suddenly tense and hard to the touch, and the tension and hardness exactly coincided with the first sound. *b.* The impulse from lateral expansion was greatest at the margin of the auricular orifices, there throwing the finger out with a violent jerk. *c.* When the resistance of the auricular valves was removed, and the sudden jerk of muscular extension prevented, the first sound was dull and obscure, like the common muscular sound.

2. The existence of bruit musculaire, as one of the causes of the first sound, is proved by the experiment in which the auricular valves were destroyed, the sound then becoming dull and obscure.

3. Under the term valves, Dr. Hope includes the chordæ tendineæ. The grounds for assuming that valvular tension is one of the causes of the first sound, are: *a.* The evidence offered above in favour of muscular extension. *b.* The London Committee state that they *felt* the chordæ tendineæ of the mitral valve become *tense in systole*, and lax in diastole. (Exp. 12.) *c.* That sound can be produced by valvular reaction is proved by the second sound, which is universally admitted to be generated by smaller membranes, acted upon with inferior force. *d.* There are certain pathological conditions of the heart, as dilatation with attenuation, in which the first sound becomes as pure a *click* as the second, and is, therefore, probably due to the extension of the auricular valves and chordæ tendineæ alone, the feeble contractions being insufficient to produce bruit musculaire or muscular extension.

The first sound, then, according to Dr. Hope's view, is a compound sound, depending on several causes, the respective and relative influence exercised by which, in producing the modifications in its character, he thus describes:

> "The valvular click gives smartness and intensity to the commencement of the first sound, and in feeble hearts, in which the sounds of extension and of bruit musculaire are absent, the click alone is heard, causing the first sound to be identical in quality with the second. This occurs, for instance, in dilatation with attenuation. The sound of muscular extension superadds bluntness and loudness to the valvular click, and is probably a principal cause of the extraordinary intensity of the first sound, often observed in violent palpitation. The *bruit musculaire* forms a gradually diminishing prolongation of the sound to the end of the act of contraction; but when the heart acts feebly, either from disease or from mere temporary exhaustion or faintness, the *bruit musculaire* may be partially or wholly absent." (p. 63.)

This is all very ingenious; but it does not carry complete conviction to our minds any more than the explanation of Dr. Williams, which we shall here also introduce, in order that our readers may see at once the theories of the two highest authorities on this subject. We would at the same time remark that the two hypotheses seem to us to differ rather in terms than in point of fact; or to differ only in the relative degree of importance attached by the two writers to the different causes. We confess we cannot accurately discriminate Dr. Hope's "muscular extension," as above explained by himself, from Dr. Williams's "sudden tightening of the muscular fibres on the mass of blood." With whom the credit of the explanation rests is another question, on which we will not touch. Dr. Williams says:

> "By excluding the blood we are thus brought to the conclusion that the cause of the sound must be in the solid structure of the ventricles: it is our next question whether it be in any part of them in particular. Several writers have ascribed it to the auriculo-ventricular valves, which, when they close, are supposed to produce a flapping sound. But the act of closing these valves is momentary, and takes place only at the commencement of the systole; whereas the first sound of the heart is prolonged through its whole duration. Further, in some of our experiments the first sound continued, although impaired, when the auriculo-ventricular valves were prevented from acting, by fingers introduced into their orifices, or by some of their cords being cut. Still these valves may produce a part of the sound, for at each contraction they are suddenly tightened, in a manner calculated to generate sound. But are the valves the only parts which are tightened at each systole of the ventricles? Is not every muscular fibre in the ventricles suddenly tightened by this action? Here are the elements of sound, motion vigorous and rapid, suddenly resisted by the mass of blood to be urged forwards by the contraction, and the contracting motion and the resistance, although greatest at first, continue to act as vibrating forces during the whole systole; hence the prolongation of the sound. . . . . In my experiments there was the best proof that we could have, that the muscular contraction of the heart produced systolic sound, for we had the heart out of the body, without its blood, without valvular action, lying on the table, or on my hand, and its contractions were still accompanied with a sound, weak, indeed, but in character resembling its natural first sound. The walls of the ventricles appear to be peculiarly calculated to generate sound; their flaccid state when relaxed, the fineness of their fibres, and the harmony with which they suddenly contract on their contents, and become almost as hard as a stone (as we can feel in the living heart of a stunned animal), fulfil the conditions best calculated for the production of sound. The commencement of the systole, producing the tightening of the auricular valves, and thereby completing the resistance of the body of blood on which the contracting fibres have to act, is naturally its loudest part, and often has a flapping character; that which continues after is more dull, and is prolonged according to the quantity of blood to be expelled, and the continued strength of the contraction."*

The London Committee, after Magendie, have admitted the impulse of the apex against the thoracic parietes, as an occasional source of augmentation to the first sound. Dr. Hope denies that this can have any part in the production or modification of the sound. The following are the considerations by which he thinks this opinion is discountenanced: *a.* We are not in possession of any experimental evidence that the apex ever actually impinges against or strikes the chest. *b.* Since the organ is maintained in apposition with the walls of the chest, by a force equivalent to fifteen pounds to every square inch of its surface, by atmospheric pressure (as first remarked by Mr. Bryan), it is reasonable to infer, in the absence of proof to the contrary, that it never quits its contact with the walls, and, consequently, can never *strike against* them. *c.* The first sound is rendered louder by leaning forwards, and to the left side, and by a full expiration, which have the effect of determining the heart, if possible, to still more complete *apposition with* the walls of the chest, a condition incompatible with increased *impulse against* it. Although we think there is much force in these objections, we cannot admit that they *entirely* disprove the proposition, as maintained by the London Committee.

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* Lectures on Diseases of the Chest, delivered in 1836-7; reprinted from the *Med. Gazette*, Lond. xxiv. pp. 154-5.

Metallic Tinkling. Dr. Hope states that the motion of the heart creates an occasional sound by striking against the inferior margin of the fifth rib, which is attended with *cliquetis*, or metallic tinkling, when the impulse is smart. This, he says, may be prevented at pleasure by pressing the edge of the stethoscope, or anything else, into the intercostal space, by which that space is put, internally, on the same plane as the rib, over which the heart then glides without catching. This phenomenon, which Dr. Hope supposes to be identical with the metallic tinkling of Laennec, is only met with in meager persons (as remarked by Laennec and Bouillaud), because in the well-conditioned the intercostal spaces are full and resistent, and, consequently, the edge of the rib is not exposed. The following extract contains the result of Dr. Hope's observations on this subject.

> "Carrington consulted me, March 30, 1838, æt, 30, tall, thin, a footman; has hypertrophy, with palpitation, and dyspnœa on exertion. There is pretty strong impulse between the fifth and sixth left ribs, where the apex impinges. On placing the stethoscope immediately over this spot, a metallic tinnitus was heard, exactly like that produced by tapping the back of the hand with the finger, while the palm covers the ear. The first sound of the heart seems to be *double*, like that produced by tapping a table with two fingers at once; the second of the two sounds was the tinnitus. I have for many years noticed this double sound with tinnitus. I made the following series of observations on the phenomenon: 1. The tinnitus ceased and the sound was single when either the upper or lower edge of the stethoscope was pressed obliquely into the intercostal space. 2. The tinnitus ceased but the sound continued double when the stethoscope, with the stopper in, was applied flatly over the ribs. 3. I filled the hollow cone with cotton wadding, which, by its elasticity, pressed the intercostal space inwards, when the tinnitus ceased and the sound was single. 4. When I withdrew half the wadding and left the cone only lightly filled, the double sound and tinnitus returned, though rather diminished. 5. The tinnitus continued, though rather duller, when I placed a penny flat across the two ribs, and listened with the stethoscope upon it. 6. It ceased, as well as the double sound, on full inspiration; and was always strongest during expiration. 7. It was increased by leaning forwards during expiration." (p. 602.)

Dr. Hope devotes a short space to a refutation of the *alternating* theory of the heart's movements, originally proposed by M. Magendie, and adopted by M. Bouillaud, Dr. Bostock, and others. This view, which supposes that the contraction of the auricles alternates with that of the ventricles, and *vice versâ*, is obnoxious to the physiological objection of leaving no interval of perfect repose for the whole organ, and is refuted by Dr. Hope's first series of experiments. The source of fallacy consists in M. Magendie's having operated on *living* animals, in which the action of the heart is so "violent, convulsive, and rapid," as to encroach materially on the interval of repose, and consequently to prevent the deduction of accurate conclusions. Dr. Hope justly remarks that no safe inferences can be drawn from the movements observed, unless the animal be completely deprived of sensibility at the time. (p. 61.)

We are sorry to learn from the work of Dr. Hope that some difference has arisen between him and Dr. Williams, with regard to the experiments on animals on which their respective doctrines of the sounds of the heart are founded; Dr. Hope preferring an exclusive claim to the institution and first performance of these experiments, the justice of which Dr. Williams does not admit. We purposely avoid entering into the merits of this dispute, but we cannot help expressing our regret that Dr. Hope has not used more dignified language in the assertion of what he deems his rights. We cannot but think, also, that Dr. Hope betrays, on several occasions in this volume, greater sensitiveness for his reputation as a discoverer than becomes one whose great merits are so readily acknowledged by his professional brethren in every country where auscultation is known and practised.

Valvular Diagnosis. On this subject Dr. Hope has supplied us with more original and useful matter than, perhaps, on any other. If the grounds on which he has established his rules be sound, valvular diagnosis may be considered to be reduced to a degree of facility and precision unequalled by that of any other disease of the heart. We cannot withhold our expressions of admiration at the great attention which Dr. Hope has given to this subject, and the indefatigable perseverance with which he has worked out a problem so difficult and obscure. As these rules have not previously been published in a complete and authentic form, and are probably new to the great majority of our readers, we shall not deem a somewhat lengthened analysis of them an unprofitable occupation of our space.

Three questions have been raised on this subject, to which it is advisable briefly to advert in the first place: these have regard, 1, to the *possibility*, 2, the *facility* or *general practicability*, and, 3, the *utility* of differential diagnosis of valvular disease. We shall notice them in order.

1. Both Laennec (*Forbes's 4th Edit.* p. 519,) and Andral (*Spillan's Trans. of Clin. Med.* p. 280,) state that they have met with murmurs without diseased orifices, and with diseased orifices which produced no murmur; and M. Piorry says (*Bouillaud*, vol. i. p. 174,) that he has not detected a murmur in one twentieth of his cases of valvular disease. Drs. Graves and Stokes (*Dublin Journal*, vol. xiv. p. 180,) repeat the assertions of Laennec and Andral, term valvular diagnosis "the most difficult part of medicine," and state that physical signs, taken alone, are in no case sufficient for its accomplishment; and even promise hereafter to "bring abundant proofs" that "*all varieties* of valvular murmur may occur without organic disease." On the other hand, Dr. Hope states that the diagnosis may be made with "almost complete certainty," (p. 382;) M. Bouillaud has never met with a single instance in more than 100 cases of valvular disease, in which a murmur has not been detected by careful examination, (*Traité*, vol. i. p. 175); Dr. Corrigan has never met with an instance of "narrowing of the cardiac orifices" unattended by a murmur, (*Dublin Journal*, vol. x. p. 174); and Dr. Elliotson had, at the time of delivering the Lumleyan lectures, always detected a murmur, except in one instance of mitral contraction, (*Lum. Lect.*, p. 19.) Now, since negative evidence will not bear comparison, as to value, with positive evidence, and since the positive evidence is here so abundant and decisive, it is clear that there must have been some lurking sources of fallacy in the former class of these discrepant statements.

The errors of diagnosis have probably arisen from some of the following causes: *a*. It is very possible that a murmur may exist, but escape detection from imperfect exploration, ignorance of the exact situation of the valves, extraneous sounds produced by the process of immediate auscultation, &c. *b.* It is admitted by Dr. Hope and proved by his cases that contraction of the mitral orifice frequently exists without murmur. *c.* Mere dilatation of a cavity or contraction of the chordæ tendineæ, without disease of the orifice in particular, may cause regurgitation; cases of which are given by Dr. Hope. *d.* Even examination after death cannot always be depended upon unless it is conducted with especial care. Thus, in one of the cases of which Dr. Hope has given a plate, he foretold disease of the aortic valves, and the patient subsequently died under the care of a practitioner out of town. "The gentleman who made the examination informed me," says Dr. H., "that I had committed an error in diagnosis, the aortic valves being sound and the mitral diseased. The preparation was sent to me, and he had mistaken the aortic for the mitral valve." (p. 621.) *e.* The diagnosis of valvular murmurs from attrition, and other murmurs, has hitherto been imperfectly understood.—These are some of the circumstances which may account for the discrepancies in the statements of different practitioners in valvular diagnosis. The rules now offered by Dr. Hope will, we think, go far to prevent the occurrence of any such discordances among future observers.

2. With regard to the *facility* with which the diagnosis of valvular disease may be acquired, we give it as our decided opinion that there is scarcely any other disease in which accuracy is so attainable. We have seen so many illustrations of the truth of this assertion, that we wish the greatest confidence to be placed in it. As a demonstration of the accuracy with which even the most complex case may be unravelled, we refer our readers to the important case of Goff in this work, (pp. 610, 625,) in which the diagnosis of a young student, in a singularly complicated assemblage of signs and morbid alterations, was submitted to the infallible test of dissection, and perfectly confirmed in every particular.

3. As to the *value* of minute valvular diagnosis, we think no scientific physician can entertain a doubt, any more than he can entertain a doubt of the value of accuracy in the diagnosis of diseases in general.

Valvular murmurs are distinguished from other murmurs, and murmurs seated in different valves are distinguished from each other by the motion of the heart which they accompany, by their situation, their character, the pulse with which they coincide, by certain peculiarities in the disease from which they are to be distinguished, and by the general symptoms with which they are attended.

Each valve or orifice of the heart may be the seat of two distinct murmurs: one accompanying the systole of the ventricles, the *systolic murmur;* the other accompanying the diastole, the *diastolic murmur.* The systolic murmur in the semilunar valves is *direct;* that is, it arises from a contraction of the orifice, or an impediment to the onward progress of the blood through the orifice. The systolic murmur in the auriculo-ventricular orifices is *regurgitant,* depending either on enlargement of the orifice, so that the valve, of normal proportions, cannot completely close it, or upon corrugation, nodulation, ossification, or some other form of disease of the valve itself, preventing it from attaining perfect apposition, and producing permanent patency of the orifice, so that partial regurgitation of blood is permitted during the ventricular contraction. The *diastolic* murmur is, of course, the reverse of the systolic, being *regurgitant* in the sigmoid valves, and *direct* in the auriculo-ventricular;

which latter is the most rare of all murmurs. Any of these murmurs may result from the various diseases to which the valves are subject. The following is Dr. Hope's account of the situations in which valvular murmurs are most audible, by listening in which situations, he says, they may be distinguished from each other, and referred to their respective causes with facility and accuracy.

"Murmurs situated in the semilunar valves are best heard immediately over those valves, (that is, on the sternum, opposite to the inferior margin of the third ribs, when the patient is horizontal, and a little lower when he is erect,) and thence for about two inches upwards, along the diverging courses of the aorta and pulmonary artery respectively. [The former diverging to the right second intercostal space, the latter to the left.] A distinct murmur, high up the aorta, proceeds from the aortic valves, as a pulmonic murmur is only feebly and indistinctly transmitted in that direction. It may be known that the murmur proceeds from the aortic valves, rather than from the diseased ascending aorta itself, by its key not being higher than a whispered *r*, whereas a murmur from the aorta itself is commonly a tone or two higher, approaching towards an *s*, and also seems much nearer and more superficial.

"A distinct murmur, high up the pulmonary artery, proceeds from the pulmonic valves, as an aortic murmur is only feebly and indistinctly transmitted in that direction. The pulmonic murmur, whether situated in the valves or the artery itself (as when dilated), always sounds near and superficial, provided the current be sufficiently strong, because the valves and the artery are close to the surface, the valves being not only in front of the aortic valves, but half an inch higher up. A murmur in the pulmonic orifice is more audible down the tract of the right ventricle than the left, which is a corroborative circumstance.

"Thus, by listening *high up* the aorta [second intercostal space, right of the sternum,] and pulmonary artery, [second intercostal space, left of the sternum,] it is easily ascertained in which vessel the murmur is seated. This rule will even apply to semilunar regurgitations, notwithstanding that these murmurs are weaker and not so well transmitted *up* the vessels in consequence of the current setting *out of* them into the ventricles. There is a further and most important advantage in exploring murmurs of the semilunar valves high up the vessels; namely, that in these situations murmurs of the auricular valves are, from their remoteness, either wholly inaudible or very obscure; although, therefore, an auricular murmur should coexist, it would not prevent the auscultator from deciding that a loud and near-sounding murmur, heard high up the vessels, was generated in or above the arterial orifices.

"Murmurs seated in the auricular valves are best heard at that part of the precordial region where, from the heart being in contact with the walls of the chest, there is dulness on percussion—in short, about the apex; for the murmur is best conducted to the surface, through a solid medium. The upper and *left* side of the dull portion being nearest to the mitral valve, is the best point for exploring its murmurs; and this point will generally be found situated about the fifth rib or subjacent intercostal space, and a little to the right of the nipple: in females it is under the mamma, when pretty well raised, and a little to the right of its centre. If the impulse of the heart be perceptible, there is no better guide than this to the situation in question. The auscultator has only to place his stethoscope about an inch above the spot where the apex impinges.

"The upper and *right* side of the dull portion being nearest to the tricuspid valve is the best point for exploring the murmurs of this valve; and the point will generally be found on or near the sternum, at the same level as on the opposite side. If, in making these explorations of either valve, the stethoscope be placed half over the dull portion and half over the thin resonant edge of the lung, the object will be sufficiently answered.

"There is a further and most important advantage in exploring murmurs of the auricular valves in these low situations: namely, that the murmurs sound so near and distinct as to preclude the idea of their being generated in the arterial orifices, the murmurs of which always sound *remote* and obscure when explored near the apex of the heart.

"When both the semilunar and auricular valves are diseased it is perfectly easy to ascertain this by observing, according to the above rules, together with those for the pitch of murmurs, that there are two distinct sources of sound.

"When two murmurs are situated in the same orifice, this is readily ascertained by tracing them up to the single source, and noticing that one attends the first and the other the second sound." (p. 90.)

In order perfectly to comprehend these rules it is requisite that the auscultator possess an accurate knowledge of the relative anatomy of the parts. The plate facing the title-page and the diagrams (fig. 4, A, B, and C, p. 615,) are admirably adapted to show the sources and relations of these murmurs, and will tend not a little, we think, to simplify this apparently complex and difficult subject.

The *pitch* or *key* of the murmurs in the several valves varies so greatly that, in a great majority of cases, it constitutes a valuable aid in diagnosis. The following is the author's description of these varieties:

"The bellows, sawing, rasping, and continuous murmurs in the heart are louder, *cæteris paribus*, in proportion as the stream of blood through the contracted orifice is stronger. This, which is obvious, on theoretical grounds, I have found amply confirmed by observation. Thus, murmurs are increased by accelerating the heart's action, and diminished by calming it, especially if the pulse be much lowered by digitalis. Again, I have collected six or seven cases of valvular disease, in which there was one strong contraction of the ventricles, producing a pulse, followed by two or three feeble contractions, attended by a barely perceptible pulse. The strong contractions occasioned a murmur—the weak, none. Again, the currents by regurgitation through the sigmoid valves, and those flowing out of the auricles into the ventricles, through the contracted auricular valves, are more feeble than the currents setting in the opposite direction, that is, out of the ventricles; and the corresponding murmurs I have invariably found to be weaker. The strength of the current, however, is not the only circumstance which occasions loudness of murmurs; for such a configuration of the stricture as most breaks the stream produces not only a rougher murmur, as already shown, but one of greater intensity. Accordingly, we find rough murmurs, cæteris paribus, louder than others, with the exception of musical tones, these being, from their acute nature, more calculated for transmission to a distance.

"The pitch or key of the bellows, filing, sawing, or rasping murmur, (as distinguished from their roughness), depend mainly on the depth or distance from the surface at which murmurs are generated, the pitch being higher in proportion as they are nearer, and *vice versá*; but it is also slightly elevated by a stronger current and depressed by a weaker. A very narrow aperture also raises the key, provided the current be strong. These circumstances were not pointed out by Laennec or the other French writers; whence has resulted the prevailing confusion respecting the meaning of the above epithets, filing, sawing, &c. After much attention devoted to this point, I think that the following characters will be found at once tolerably accurate and easy of comprehension.

"Murmurs seated in the pulmonary orifice or artery, from being the most superficial, are on a higher key than any others. Though they are seldom as high as the whispered letter *s*, yet they range between this and the whispered letter *r*. Murmurs originating in the ascending aorta, where it approaches near to the sternum, are, for the same reason, on almost as high a key.

"Murmurs in the aortic orifice, being rather more deeply seated, seldom rise higher than a whispered *r*, which is their average key, and it is perhaps the most ordinary type of the sawing sound. M. Bouillaud, however, (to whom I am indebted for the ingenious idea of representing sounds by letters, and who has used *s* and *r* for this purpose,) thinks that *s* more truly represents the sawing sound.

"Murmurs from aortic and pulmonic regurgitation, in consequence of the currents being weaker, are generally two tones lower, like whispering *awe* by inspiration; and the click of the valves, when audible, may be represented by prefixing the letter *p*, as *paw*.

"Murmurs in the mitral valve, from being still more deeply seated, are, on the average, four tones lower, like a whispered *who;* the tone is somewhat elevated by a very strong current, as that of violent mitral regurgitation, and depressed by a feeble current, as that producing diastolic mitral murmurs.

"Tricuspid murmurs are rather higher toned than mitral, because less deeply seated." (pp. 84-6.)

Dr. Hope thinks that *musical murmurs* indicate nothing more than common murmurs. Of *continuous murmurs* Dr. Hope says:

> "They will probably be found to indicate sometimes organic disease, attended with regurgitation out of the aorta into the right ventricle or pulmonary artery: sometimes churning of a little serum between layers of rough lymph on the pericardium; and sometimes probably dilatation of the pulmonary artery and compression of the vena innominata." (p. 89.)

The *pulse* is a further assistance in the detection of valvular disease, when its characters are fully understood; although, without a thorough comprehension of its varieties and their sources, it must be rather a means of confusing than of aiding the practitioner. The "jerking" pulse of aortic regurgitation, we are told by Dr. Hope, is in particular so characteristic that from its presence alone a correct diagnosis may often be formed of the disease. The original genius of Corvisart endeavoured to connect peculiarities of the pulse with particular diseases of the heart, but he was by no means successful in the attempt. Laennec does not even mention the pulse in his diagnosis of valvular disease; he devotes a considerable portion of one of his earlier chapters to the exposition of his reasons for agreeing with Celsus that the pulse is "res fallacissima." Andral says that the pulse presents so many varieties that "only a secondary importance can be attached to it." M. Bouillaud says, that in valvular disease of the left side the examination of the pulse is a source of assistance which should not be neglected in differential diagnosis; and he straightway proceeds to describe as peculiar to aortic obstruction a pulse which, Dr. Hope assures us, that lesion very rarely occasions, and which is characteristic of mitral disease. Dr. Elliotson gives a much more correct description of the pulse in his Lumleyan lectures, a work which has by no means been estimated at its real value.

We think that Dr. Hope has deprived this subject of much of its obscurity and confusion, and has gone far to prove, by pathological facts, what Bichat inferred from physiological reasoning, that "to every species of action of the heart there corresponds a particular kind of pulse." The following account of the varieties of the pulse in valvular disease we have abstracted from Dr. Hope's work; we think a knowledge of them will prove a very valuable addition to our usual means of diagnosis.

The varieties of the pulse in valvular disease depend on the particular valve in which the disease is seated, and on the nature of the disease itself. 1. *Mitral valve.* Both contraction of this orifice, and regurgitation through it, confer on the pulse various degrees of smallness, weakness, irregularity, inequality, and intermission, proportionate to the amount of disease in a given case. 2. *Aortic valve.—a. Contraction.*

This form of disease must be very great to render the pulse small, weak, irregular, unequal, or intermittent. Dr. Hope has never found it to possess these characters in any remarkable degree unless the valves were either soldered together by cartilaginous degeneration, or more or less fixed by ossification in the closed position, so that the aperture was only a limited chink. An induration of the size of an ordinary pea has little effect on the fullness, firmness, and regularity of the pulse, and slighter degrees of contraction appear to have no effect on it whatever. These facts are curious and singular, but are proved by many of Dr. Hope's cases.—*b. Regurgitation.* Under this head Dr. Hope also includes regurgitation out of the aorta into the right ventricle, or into the pulmonary artery. The pulse is preeminently *jerking*, a high degree of the pulse of unfilled arteries, as seen in anæmia from any cause. The diastole or beat of the artery is short and quick, as if the blood were smartly jerked or shot under the finger, the vessel, during the intervals, feeling unusually empty. It differs from the jerking pulse of anæmia, in being more marked and in not necessarily being frequent, as the anæmic pulse is when its jerk is distinct. 3. *Valvular disease on the right side.* This form of disease produces little effect on the pulse, not being immediately connected with the arterial system, and having little influence over the general action of the organ. (pp. 375-80.)

It only remains to quote the diagnosis of valvular murmurs from those produced by anæmia, pericarditis, and aortic disease; and we shall have completed our analysis of Dr. Hope's present views on this extensive subject. The diagnosis of organic valvular murmurs from anæmic murmurs depends on the following characters of the latter. *a.* They are confined to the aortic orifice. *b.* They occur only with the systolic motion of the ventricles. *c.* They are always weak, and of the soft or bellows kind. *d.* They are generally attended with venous and arterial murmurs and thrill. *e.* They exist only when the heart's action is excited. *f.* They are accompanied with the *jerking* pulse. *g.* They are attended with general anæmic and hysterical or nervous symptoms. *h.* They disappear when the anæmia is cured by iron and animal food. These circumstances should be contrasted with the following characters of organic valvular murmur. *a.* Only the murmur of aortic contraction can be confounded with that from anæmia, by any one who has studied the rules for differential valvular diagnosis. *b.* This disease does nor produce the jerking pulse, venous or arterial murmurs. *c.* Valvular murmurs are often rough. *d.* They persist without intermission, even though the circulation be slow. *e.* They are not necessarily attended with general anæmic or nervous symptoms.

We formerly expressed our concurrence with the statement of M. Bouillaud, that valvular murmurs cannot always be distinguished with certainty from the attrition murmurs of pericarditis, especially when the two classes of sounds coexist in the complicated disease of endo-pericarditis. We are happy to see that Dr. Hope does not share this difficulty with us, and that he has supplied a diagnosis on which he can generally rely with confidence. The management of the complicated cases above mentioned renders the diagnosis sometimes exceedingly desirable, especially with reference to prognosis and the treatment by mercury.

> "Some writers," says Dr. Hope, "especially M. Bouillaud, (tom. ii. p. 211,) have experienced great difficulty in descriminating these two classes of sounds. I cannot say that, since I became acquainted with attrition murmurs, I have participated in this difficulty; even when the two classes of sounds existed simultaneously, and each was double. This is mainly from attending to the rules which I have so often inculcated; namely, of listening to murmurs of the sigmoid valves, two inches or more up the aorta or pulmonary artery, where attrition murmurs are almost inaudible, and of listening to murmurs of the auricular valves a little above the apex of the heart, where they are sure to be the loudest, whereas attrition murmurs may be louder at other parts of the heart, where they happen to be generated. Further, attrition murmurs present the following distinctive peculiarities. 1. They are usually of a much rougher quality of sound than the valvular, so that when the two coexist the one may be heard *through* the other. 2. When a murmur with the second sound is rough, as rasping, creaking, croaking, &c., it is certainly from attrition; as I have never known a valvular murmur with the second sound to be rough, the diastolic currents being too feeble to produce roughness. 3. Attrition murmurs are almost always attended with vibratory tremor; whereas valvular murmurs rarely present this phenomenon, and generally in a slighter degree. 4. Attrition murmurs are apt to undergo frequent and sudden changes of character and situation (Stokes), which are very pathognomonic, because valvular murmurs change little in character, and not at all in situation." (p. 174.)

In addition to these distinctions Dr. Stokes (*Dublin Journ.*, vol. x. p. 46,) attaches considerable weight to the slight extent over which attrition sounds are audible. He says they are frequently lost on removing the stethoscope an inch from the situation where they are generated. M. Bouillaud (vol. i. p. 197,) justly remarks that the attrition murmur is "superficial, diffuse, or *peripheric*" in its nature. Another distinction will be found in the accompanying pulse.

INORGANIC MURMURS. The section which treats of these has undergone no material alteration. The experiments on dogs continue to form the basis of the arguments, and the murmurs are almost entirely ascribed to anæmia. There are two circumstances which Dr. Hope believes to characterize inorganic murmurs in the heart, which are by no means generally known. The first is that they invariably accompany the systolic movement; the second that they occur only at the aortic orifice. And the explanation of these facts is far from difficult. It is obvious that murmurs cannot be produced in the aortic or pulmonic orifice, during the *diastole*, without the existence of regurgitation, which presupposes *organic* disease; and in regard to the auricular orifices Dr. Hope's cases show that murmurs are very rarely heard, even during the presence of the greatest organic disease. They can only be generated in the arterial orifices, because they are *systolic*, and a systolic mitral or auricular murmur is regurgitant, and must result from organic disease. Laennec stated that inorganic murmur most frequently accompanies the diastole of the heart, but he was, doubtlessly, describing cases of aortic regurgitation; his theory of the diastole, of course, prevented him from looking to the aorta for diastolic murmurs, nor would he necessarily have found very obvious marks of disease if he did, since regurgitation may result from a condition of the valves which does not always present striking indications of its morbid nature, and can only be detected by a person whose previous knowledge leads him to suspect it. Dr. Elliotson was, we believe, the first person who limited inorganic murmurs to the ventricular systole. He says, "the temporary bellows' sound has always, in my experience, been at the contraction of the ventricles." (*Lum. Lec.* p. 18.) In this, as in many other respects, he was much in advance of the profession in general.

The physical circumstances which produce inorganic murmurs in the heart, the immediate causes on which they depend, the condition of the aortic orifice in an anæmic individual whereby it takes on sonorous vibrations, the real essential difference between it and the ordinary state of the orifice, do not appear to us capable of explanation with our present amount of facts. The only circumstance which is constant in its existence is an excited action of the heart. Dr. Hope would add to this a diminished quantity or attenuated quality of the blood, conditions which certainly occur in the majority of cases, but which are by no means constant or essential. We recently attended the amputation of a leg, in the case of a healthy, robust, agricultural lad of fourteen: during the violent palpitations occasioned by the operation, a loud blowing murmur accompanied the ventricular contraction, which ceased entirely when the operation was over. The accident was a compound fracture, and had certainly induced considerable hemorrhage, but not to such an extent as to render the patient sensibly affected by the loss of blood; nor did he present any of the symptoms of anæmia. Both Andral and M. Meriadec Laennec assign *plethora*, a real bonâ fide increase in the quantity of blood, as an occasional cause of inorganic murmur, and state that the morbid sound may be removed by bloodletting, a sufficient proof that it could not have depended on anæmia. These facts prove, in our opinion, that an excited action of the organ alone is the occasional cause of murmurs, and we do not think sufficient evidence has been collected as to the mediate or immediate nature of the influence exerted by the anæmia when that condition is known to exist. One fact is certain, that before anæmia can occasion a murmur it must bring on excited action.

When an inorganic murmur occurs in an artery, in an anæmic patient, another circumstance which may have some influence in its production, is the flaccid condition of the coats of the vessel. This subject is sufficiently familiar, from the ingenious illustrations it has received in the experiments of Dr. Corrigan. The whole argument is ably treated by Dr. Hope, but we regard it still as incomplete, and beg to point the attention of practitioners to it, as a subject on which the publication of the symptoms and *treatment* of cases would be an exceedingly valuable addition to our knowledge.

Sufficient attention has not been given, in the course of this investigation, to the experiments of John Hunter, by which he endeavoured to determine the relative proportion of muscular and simply elastic contractility possessed by the coats of arteries. He proved, in those experiments, that, when the quantity of blood in a vessel is diminished, the vascular coats do not remain relaxed or "*flabby*," as Dr. Corrigan terms their condition, but that they contract closely around the quantity which remains; that their caliber is sensibly diminished, and that they thus acquire a quality of real *tension*, approximating that of a cord. (*On the Blood*, p. 124, *et seq.*) It is, in our opinion, a question invitingly open to enquiry, whether a vessel in this condition would not be necessarily excited into sonorous vibrations by the jerking action of the heart, on the common principles of acoustics, according to which tension is an almost essential element in the production of sound, and whether this is not precisely the state of things in a case of anæmic palpitations with murmur. The observations of Dr. Hope, Dr. Williams, and the London Committee, on the first sound of the heart, appear to prove that tension is equally necessary to the generation of sound in a hollow body as in a solid string. We offer this as a suggestion to the numerous enquirers who are so actively engaged in the investigation of this subject.

Venous Murmurs. Under this head the author has added a section containing an elaborate description with many interesting original observations, if the "continuous murmur" of Laennec, or the "*bruit de diable*" of Bouillaud, which was first shown by Dr. Ogier Ward, of Shrewsbury, to be seated in the veins. This murmur almost, if not quite invariably results from anæmia, of which it is a valuable sign. We are inclined to place much confidence in the constancy of its presence as an attendant on this obscure and deceptive disease. In those cases of anæmia, and they are very numerous, which so closely similate hyperæmia as to create perplexing doubts of their real nature, we anticipate that the venous murmur will be found an efficient aid in diagnosis. Dr. Hope states that he has never found the murmurs to exist, in any marked degree, in any instance in which anæmia was absent. Further, he has invariably found that they gradually disappeared in proportion as the anæmic state was removed by iron, aloetic aperients, animal food, and fresh air, (p. 122-4.) These results are in exact accordance with those of M. Bouillaud, and we have much pleasure in adducing the further testimony of our own observation to their truth.

We transcribe some of the more interesting of Dr. Hope's observations, which our readers will find no difficulty in repeating, on the greater number of their anæmic or chlorotic patients.

> "The venous murmur is on a much lower key than the arterial murmur, for, while the latter is often as high as the note produced by whispering the letter *r*, and seldom lower than *au*, the venous murmur is usually as low as *who*. This sound, indeed, offers the most complete and ready imitation of the phenomenon with which I am acquainted. The hollow sound of a large incessant forge bellows also imitates it very closely. When the arterial throb is considerable, the murmur experiences augmentations corresponding with each arterial diastole or pulse." (p. 110.)
>
> "When the vein under examination is very superficial, merely subcutaneous, as the external jugular, very light pressure with the stethoscope will increase the murmur by partially contracting the caliber of the vessel; but if the vein be obliterated by laying the point of the finger lightly upon it above the stethoscope, or by depressing the upper edge of the stethoscope, the murmur instantly ceases." (p. 111.)
>
> "Strong pressure with the stethoscope, sufficient to obliterate a subcutaneous vein, as the external jugular, if appliėd on the internal jugular, instead of suspending the murmur, swells it *gradually* to a surprising degree of intensity and diffuseness, like blowing the word *who* with great force, mixed up with which sound I have frequently heard humming, cooing, and whistling notes, appearing to proceed from several veins at once, which I shall hereafter show to be the case." (p. 112.)
>
> "If the point of a finger be nicely dropped on the internal jugular vein in any part of its course, so as to obliterate the vessel, yet without obliterating the carotid, the loud murmur instantly ceases, and nothing is heard but a dull, obscure rumbling seated in smaller veins. It is occasionally mixed up with puny, humming, and whistling notes. If the finger be now raised again from the internal jugular, the torrent rushes down and restores the original loud murmur almost as promptly as when the finger is raised from the hole of a wind instrument. By alternately raising and depressing the finger, the most sceptical may soon convince himself that the seat

of the murmur is really in the vein. One or two precautions are required. If the neck be displaced from the perpendicular, the sterno-mastoid muscle is apt to be put so much on the stretch as to obliterate the internal jugular and suspend the murmur. Again, if the stethoscope, placed behind the sterno-mastoid, press that muscle too much forward, it will obliterate the internal jugular. Again, if the skin be stretched across the neck, under the stethoscope, the tension will increase the murmur in most; but in a few, whose internal jugular is very superficial, it will obliterate the vessel and suspend the murmur." (pp. 112-13.)

"The loud murmur of the internal jugular becomes louder during inspiration, especially about its end, and weaker during expiration. I have noticed that when an anæmic patient becomes faint from long standing under examination, the murmur, previously loud and constant, becomes extinct except during the inspirations. This evidently proceeds from a deficient afflux of blood to the head, whence there is not a sonorous current down the veins, except when it is favoured by the suction of inspiration. For the latter reason, the murmur exists during inspiration only, in those who barely exhibit the phenomenon at all, for instance, the convalescent from anæmia." (p. 114.)

"In order to produce a murmur of the internal jugular in perfection, it is necessary to avert the face, while the neck is kept perpendicular and the chin well raised. When the head is restored to its natural position or is depressed, the murmur ceases or greatly diminishes." (p. 115.)

"As the respiratory murmur simulates the venous murmur, learners should request the patient to hold his breath." (p. 116.)

The venous murmur occasionally assumes a musical character, which Dr. Hope proposes to denominate the "venous hum;" it frequently resembles very closely a smart wind blowing through a keyhole. Its indications are of no specific nature.

Of MUSICAL MURMURS we shall add nothing to what we have already said. They are merely modifications of common murmurs, and cannot be referred to any distinct or peculiar morbid action.

ABDOMINAL MURMURS. A valuable chapter is introduced on auscultation applied to pregnancy, divided into two parts. In the first the rules for exploring the pulsations of the fœtal heart are concisely and practically explained, in a manner which will be found useful to the obstetric student. In the second, the author enters into an examination of the doctrine of utero-placental murmur, in the course of which he develops some new views on this imperfectly understood phenomenon, and endeavours to determine its real nature and value. He submits for further investigation the following propositions:

"1. That the murmur is arterial when it is a whiff. 2. That it is venous when it is continuous without augmentations synchronous with the pulse. 3. That it is arterial and venous conjoined when it is continuous with augmentations. 4. That its seat is sometimes in the vessels of the abdominal parietes, as the epigastric, circumflex ilii, internal mammary, and their branches and concomitant veins; sometimes in the great arteries and veins within the cavity of the abdomen, as the common and external iliacs, the renal, the three branches of the cæliac, the colica dextra, media, sinistra, and ileo-colica, and the portal veins; sometimes, possibly, in the uterine walls, and sometimes, possibly, in the vessels of various tumours. 5. That the murmur is generally created by pressure, whether that of the uterine or other tumour or of the stethoscope; and that it does not exist independent of pressure, except, possibly, in anæmic cases. 6. That the stretched condition of the arteries, and especially the veins of the abdomen, is favorable to the operation of pressure in producing the murmur." (p. 133.)

He remarks that these propositions cannot be adequately comprehended except by one who is thoroughly imbued, both theoretically and practically, with the doctrines of venous murmur. He then proceeds to demonstrate the analogy between abdominal murmurs and those heard in the neck of an anæmic patient, by quoting the descriptions of the former given by Laennec, Kennedy, and Montgomery. He shows that both are sometimes a mere arterial whiff; sometimes continuous with augmentations; sometimes continuous with little or no augmentations; sometimes ceasing with no assignable reason; and most marked in anæmic subjects. In illustration of these principles, he cites nine cases, which prove that the whole of these murmurs may exist independent of pregnancy. Finally, he shows the inconsistencies and imperfections in the arguments which have been employed to localize these sounds in the placental arteries, and concludes with the three following practical rules. 1. That a near-sounding high-toned murmur may result from any tumour, may exist wholly independent of tumour, and occurs almost exclusively in the thin blooded or anæmic, with a quick pulse. 2. That a distant low-toned murmur, heard on a tumour in the hypogastric region, affords presumptions that the tumour compresses the hypogastric artery. 3. That these murmurs afford presumptions of pregnancy only when they coincide with other signs. We request the reader to compare these observations with the third article in our present Number, p. 365.

Purring Tremor. A concise synopsis is given of the circumstances under which this phenomenon is developed in the heart and arteries. Dr. Hope states that he has observed regurgitation through the mitral valve to be beyond comparison the most frequent cause of tremor in the heart. He has never known it to exist in the heart independently of organic disease. In the arteries it may be produced by disease of their internal coats. Tremor, is, also, occasionally propagated as far as the carotid and subclavian arteries, by considerable contraction of the aortic orifice, but rarely, if ever, propagated so far as the radials. Aortic regurgitation may propagate a tremor as far as the radials, or even to still more remote arteries. Anæmia may, during nervous excitement, produce arterial thrill, without organic disease. In conclusion, as tremor has the same origin as bellows', musical, and other murmurs, it is always accompanied by them; but, as it requires a greater degree of vibration for its sensible development, they may exist without being accompanied by it.

Endo-pericarditis. The general history of this complex disease has undergone valuable revision in the present edition, and has received many useful additions in the form of notes. The morbid appearances of redness, effusion, and adhesion are elaborately described, and the relation of their varieties with the modifications observed in the general symptoms is pointed out. The general signs of the disease, their obscurity, their complexities, their diversities, their inconstancy, the causes of these apparent anomalies, and their relation to diagnosis, prognosis, and treatment are fully discussed. Dr. Hope concurs with Dr. Stokes and Dr. Latham in assigning the greatest importance in the production of these differences to the quantity of effusion in different cases. He combats, and with complete success, the opinion of M. Bouillaud, that these modifications are always attributable to pleuritic or peripneumonic complications. In speaking of the worst train of symptoms induced by the compression of fluid occasioning considerable obstruction to the circulation through the heart, Dr. Hope observes that the same class of symptoms is produced under whatever circumstances the circulation through the heart is greatly impeded; and adds that he has seen them result from poisoning by arsenic and by intense gastro-enteritis; that they result also from poisoning by the mineral acids, tobacco, &c., all of which agents have a paralyzing effect on the heart. He has likewise seen them occasioned by polypi forming in the heart before death and by extreme softening of the organ.

Dr. Hope describes the physical signs of endo-pericarditis under the heads *percussion*, *impulse*, and *sounds*. In respect of the two former, nothing new is added. He arranges the sounds into two classes: the sounds of the first class, being *direct* signs of pericarditis, arise from the condition of the pericardium itself; the second class are valvular murmurs resulting from endocarditis, which constitute *indirect* signs of pericarditis. The account of the attrition sounds coincides with that of Stokes and Bouillaud; but, in addition to the generally described phenomena, which it is unnecessary to notice here, the author notices a "continuous rumble," which he has occasionally heard, and which he attributes to the agitation of a small quantity of fluid in the cavity of the pericardium.

> "In one case, in which the fluid originally caused dulness as high as the second rib, the rumble came on with tremor, when the quantity of fluid became moderate; it passed into a double attrition sound with tremor, when the fluid underwent further absorption, and both phenomena ceased, when complete cessation of dulness and other signs indicated complete adhesion of the pericardium, which I ascertained to have taken place by post-mortem examination a year and a half afterwards." (p. 167.)

Murmurs of the second class, or those indicative of endocarditis, are identical with the valvular murmurs, of which we have given so ample an account. The three signs which, according to Dr. Hope's experience, are the least number that suffice to indicate inflammation of the heart, are increased action of the heart, fever, and a murmur which did not previously exist.

Connexion of Diseases of the Heart with Apoplexy, Palsy, &c. Dr. Hope's experience goes strongly to confirm the old Italian doctrine on this point. In illustration of it he has given an analysis of thirty-nine cases, treated in the Marylebone Infirmary, to which we beg to direct the attention of our readers.

Partial Dilatation of the Heart. The chapter on this subject is principally devoted to an abstract of Mr. Thurnam's valuable paper from the Medico-Chirurgical Transactions for 1838. Some interesting and instructive facts are adduced relative to this obscure affection, but they do not throw much light on either its diagnosis or its treatment, as distinguished from other organic diseases of the heart. The obscurity of this disease should render it an especial object of attention to the profession; it offers to the enquiring practitioner an inviting field for original research. Dr. Hope suggests that every novelty or anomaly, either in character or situation, of a murmur, and of the accompanying pulse and impulse, should be accurately noted and compared with post-mortem appearances, in order more clearly to determine the phenomena, if any there be, peculiar to real aneurism of the heart.

Diagnosis of Softening. Much useful matter is added to this chapter, and we are induced to hope that softening will henceforth take its place among the diseases which a stethoscopic exploration can distinguish with tolerable facility and general accuracy. Dr. Hope's contributions to our means of detecting this affection will be highly appreciated, when it is remembered that the latest writer on the subject, M. Bouillaud, has scarcely given any diagnosis at all. In addition to the general symptoms of organic disease, softening is indicated, according to our author, by a very weak and sometimes imperceptible impulse, as was stated by Laennec, by both sounds being weaker than natural, and the first sound becoming short and flapping like the second, and by irregular, unequal, feeble, and intermittent pulse, without valvular murmur. Some cases, illustrative of these points, are inserted at the end of the chapter.

Signs of Adipose Disease of the Heart. This disease is also illustrated with cases which lead to the inference that fat impedes the action of the heart and obstructs the circulation; and that its signs, so far as Dr. Hope can judge, are, 1, diminution of the sounds, especially the first, without the flap of softening; 2, irregular pulse without valvular disease; 3, "oppression" or even pain in the precordial region, with general signs of a retarded circulation, producing cerebral, hepatic, and other congestions.

Thoracic Aneurism. In an appendix to the chapter on aneurism of the aorta are related two cases. The first is a case of aneurism of the aorta bursting into the right ventricle, and leads to the presumption that the signs of this disease are: *a*, a remarkably loud, harsh, superficial sawing murmur, with both the systole and diastole, together with a continuous *incessant* rumble; both most audible above the level of the fourth rib, on or near the sternum, and thence, along the tract of the pulmonary artery, up the interspace between the second and third rib; *b*, a purring tremor in the same situations; *c*, weakness or extinction of the second sound, in consequence of the reaction of the aortic blood on the valves being enfeebled by the regurgitation; *d*, pulse preeminently jerking; *e*, great and rapid dropsy; *f*, livid complexion; *g*, the evidence is stronger if the symptoms followed a lift or effort producing faintness and paleness. The second case is one of rupture of a dilated aorta into the pulmonary artery, of which the following were the signs observed: *a*, a very loud, superficial, sawing murmur, prolonged continuously over the first and second sounds (and probably weaker during the interval of repose), loudest along the tract of the pulmonary artery; *b*, a purring tremor in the pulmonary artery, in the interspace between the second and third ribs; *c*, the second sound weakened at the clavicles; *d*, the jerking pulse; *e*, great, rapid, and universal dropsy; *f*, a livid, venous tint of surface; *g*, the symptoms following an effort.

Abdominal Aneurism. On the physical diagnosis of this disease much new and valuable matter is introduced. After describing the physical signs of the aneurism, the tumour, pulsation, dulness on percussion, and bellows' murmur, Dr. Hope proceeds to point out the fallacies which may arise from the existence of various abdominal tumours, and the means by which they may be detected and obviated. An abdominal tumour, he states, may generally be distinguished from aneurism of the abdominal aorta by the following characters: 1. The impulse is more feeble, especially if the tumour be large or superficial. 2. The impulse is rendered still more feeble by applying the stethoscope laterally. 3. The tumour is often moveable with the viscus in which it is seated. 4. The tumour and impulse may often be removed by a brisk purgative. 5. Tumours are generally less compressible than aneurisms. 6. In enlargement of the liver, dulness extends from the right hypochondrium, *without any interval*, to the seat of the pulsation. 7. The murmur of a tumour is generally less than that of an aneurism. 8. Collateral and important evidence may be derived from the general history of the case. The author subjoins a case in which a fatal aneurism produced very few of its characteristic phenomena, and was indicated solely by epigastric pulsation. Another source of fallacy consists in the pressure of the stethoscope, which may generate a murmur in a superficial artery situated over the tumour. Such a murmur will be distinguished by its *nearness*, its hissing tone, its being restricted to one spot, and by its ceasing when the vessel is obliterated by firm pressure.

NERVOUS AFFECTIONS. The chapters on angina pectoris and syncope remain as in the original edition. That on nervous palpitation is very considerably enlarged and modified. Inorganic palpitation is divided into the following four varieties, according to its cause: 1. Palpitation excited by various derangements of the nervous system only. 2. Palpitation from anæmia. 3. Palpitation from too stimulant diet. 4. Palpitation from plethora. As the first, third, and fourth varieties do not occasion murmurs, they are distinguishable from organic disease without much difficulty by a practised auscultator. Nor will much difficulty be encountered in establishing the diagnosis of anæmic palpitations, by an auscultator who is conversant with the rules laid down in this work for differential valvular diagnosis.

DISPLACEMENTS. The following cases of murmurs from displacement are interesting:

> "I at present attend a young lady, Miss M., in whom the heart was forced entirely over to the right of the sternum by pleuritic effusion in the left pleura. The aorta was felt to pulsate between the second and third right ribs, an inch from the sternum, and here a murmur was heard with the first sound, which has ceased since the heart has been restored to its natural situation by the absorption of the fluid. Is it therefore possible that a twist given to the aorta, or pressure of the vessel against the ribs, may be the cause of a murmur under such circumstances?
>
> "I have at present two cases of still greater displacement of the heart to the right, in consequence of universal consolidation and contraction of the right lung and hypertrophy of the left. The ascending aorta beats between the second and third right ribs, two and a half inches from the sternum in one case, and one and a half to two in the other. There is a murmur with the second sound, from aortic regurgitation, in the former case. It remains to be seen whether regurgitation proceeds from a twist in the aorta, disabling the valves, or from disease of the valves themselves. The pulsation of the aorta so far on the right, might be, and actually was, mistaken by non-auscultators for an aneurism.
>
> "When the heart is displaced to the right just so far as to be impacted between the sternum and the spine, I have found its impulse to be considerably increased, so as to convey the idea of hypertrophy. This occurred in the case of Miss M. above described, and, until I pointed out the circumstance, the disease was mistaken for hypertrophy, the pleuritic effusion being overlooked." (pp. 536-7.)

PULSES OF DISEASE OF THE HEART. We have noticed all that is new in the text, on this subject, in the remarks we have made on valvular diagnosis, softening, and adipose disease. A table is, however, appended to the work, containing a synopsis of the characteristic pulse of every disease of the organ, which, although still very imperfect, will be found useful for reference: we therefore transcribe it.

Table of Pulses of Disease of the Heart.

"Simple Hypertrophy of Left Ventricle. Pulse *strong* and *tensely prolonged*, because the ventricle contracts powerfully but slowly.

"Hypertrophy with Dilatation. *Strong, tensely prolonged*, and *large*, because the ventricle contracts powerfully, slowly, and expels an increased quantity of blood.

"n.b. If the above pulses be moderately accelerated, they become '*hard*.' They may be rendered temporarily or permanently *small* and *weak* by any debilitating causes impairing the contractile power of the heart. Also by extreme palpitation and dyspnœa causing engorgement of the organ.

"Hypertrophy with Contraction. *Tense but small*, and if the contraction be considerable, it becomes weak as well as small, from the insufficient quantity of blood propelled into the arteries.

"Dilatation with Hypertrophy, i. e. the dilatation being predominant. *Large* and rather *prolonged* but *soft*, from the larger capacity, but weakness of the ventricle.

"n.b. This pulse if accelerated becomes 'bounding.'

"Dilatation with Attenuation. *Large* and *weak*, becoming *small* in the last stage, when the ventricle is too weak to expel its contents.

"Softening. *Small, weak*, and more or less *irregular*, *unequal*, and *intermittent*, sometimes extremely so, in the late stages, from the debility of the ventricle.

"Free Regurgitation through the Aortic Valves. Eminently *jerking*; from the arteries being unfilled.

Contraction of the Aortic Valves. *Strength* little impaired unless the contraction be very considerable. The *regularity is seldom affected*, except by extreme contraction.

"Great Contraction of or free Regurgitation through the Mitral Valve. *Small*, *weak*, *irregular*, *intermittent*, and *unequal*, because contraction occasions an insufficient and irregular supply of blood to the ventricle; and because regurgitation weakens the pulse, in consequence of the resistance of the mitral valve being removed, and disturbs its regularity in consequence of rendering the supply of blood less uniform.

"A large Polypus formed before death. Suddenly causes a small, weak, irregular, and intermittent pulse, because the polypus chokes up the ventricle.

"Endocarditis with Polypus. Ditto.

"Pericarditis with much serous Effusion compressing the Heart. Ditto." (pp. 623-4.)

The sixth part of the work contains a valuable collection of cases. A few of the less complete cases in the former edition have been removed, and others of an interesting nature, illustrating particular points for the most part new, have been added.

The plates will be found a valuable improvement in the present edition. They illustrate most of the diseases of the heart which are difficult of comprehension. The frontispiece, which is a sketch of the heart in situ, exhibiting its relation to the ribs and surrounding parts, will greatly facilitate the student's labour in the practical prosecution of his studies. The plates reflect great credit on Dr. Hope as an artist, and they are neatly engraved.

In concluding our remarks on this work, we wish to observe that we hope none of our readers will imagine, because our attention has been chiefly directed to the portion of it relating to physical diagnosis, that we are either disregardful of the advantage and necessity of combining an intimate knowledge of the general symptoms of diseases with that of auscultation, or that the work contains an imperfect account of the anatomy, pathology, causes, general symptoms, treatment, &c. of the different diseases. On the contrary, it is our opinion that a principal advantage conferred on practical medicine by the discovery of auscultation consists in the closer attention to the general history of diseases, and the more correct appreciation of their respective general symptoms, which its study has necessarily induced. Without having thoroughly studied this part of Laennec's discovery, an auscultator is little superior, in the facility and precision of his diagnosis, to an experienced non-auscultator. Our reason for having so slightly referred to this subject is the obvious one, that we are reviewing, not the whole work, but the additions to the former edition of it. Ere long we may probably take up the whole subject of diseases of the heart; when we shall have occasion to compare Dr. Hope's treatise with some others recently published on the continent. We cannot, however, close the present notice without observing of the work, taken as a whole, that it is, in our opinion, by far the best publication on diseases of the heart and great vessels that exists in this or in any other language; an estimate grounded as much on the history which it contains of the general nature, course, and treatment, as of the physical diagnosis of the several diseases. No author evinces a more philosophical discrimination in his valuation of the relative importance of general and physical signs in establishing a diagnosis than Dr. Hope; and no writer has more fully and judiciously availed himself of the assistance of the former class of signs. On this point, indeed, we cannot better state our own opinion than by quoting that of the author, as expressed in the concluding paragraph of his introduction.

> "With respect to the comparative value of the general and physical signs of disease of the heart, it may be said that Laennec rather undervalued the former and overrated the latter. This was owing principally to the general signs being less perfectly understood when he studied than they have subsequently become *in consequence of being investigated with the aid of auscultation.* The ardour of his early disciples, who imagined that the physical rendered the general signs superfluous, brought auscultation into some disrepute by the inaccuracy of their diagnosis. But since the stethoscope has taken its proper place *as an auxiliary only,* and the diagnosis has been founded on the two classes of signs conjointly, auscultation has ranked as a discovery which will immortalize its author and form an epoch in the history of medicine."

ART. XI.

The Accoucheur; a Treatise on Protracted Natural Labours, Suspended Animation in new-born Infants, and Uterine Hemorrhage after birth of the Child; with illustrative Cases. By JOHN CRAIG, Surgeon, Paisley.—*Glasgow,* 1839. 12mo, pp. 252.

THE contents of this work are enumerated in the title-page. In his preface, the author offers advice to, and passes some observations upon, his medical brethren which cannot be called flattering: "However much," he says, "our professional friends may be opposed to innovation in the practice of their art, without the sanction of some great name, we urgently ask them simply, in the first place, to read and study our small volume; then we doubt not they would enjoy, perhaps for the first time in their lives, what ought always to stand with medical men in submitting their observations and reflections to practice, the conscientious approval of their own minds. But from the few testimonies we have quoted as well as from our own personal knowledge, we have misgivings regarding our medical brethren." (pp. viii, ix.) Such then is the lamentable state of midwifery that as yet we have seldom or never felt conscientiously satisfied with the results of our practice: how comfortable must the opposite condition be which is enjoyed in so eminent degree by the author! The work is indeed characterized throughout by unbounded assurance and self-esteem, and it is with unfeigned regret that we are compelled to notice in it also something of that invidious and disingenuous spirit which has marked some other obstretrical writings that have recently proceeded from the northern press. At the beginning of the very page from which we have made the above quotation, an insinuation is thrown out that Dr. Churchill of Dublin is one of "those who scarcely know what improvement means." Against such a charge we must enter our protest. Although in our review of Dr. Churchill's work on the Diseases of Females (*B. and F. M. R.* July, 1838,) we ventured to point out a few omissions or defects, yet in common fairness to its learned author we cannot let such an assertion pass without exposing its injustice. In the work in question Dr. Churchill has proved himself to be a man of very extensive reading and also ardently devoted to the practice of his profession.

Mr. Craig begins his first chapter on "protracted natural labours" with the following definition: "Under the designation of protracted natural labour are comprehended those forms in which the pelvis is of such dimension and capacity as to allow a child of the usual size to pass, and when the presentation is of such a kind as to permit the passage by the ordinary efforts of the expulsive powers, but in which from some other obstructing cause the period of delivery is delayed." (p. 1.) Where the protraction of labour neither depends upon the pelvis nor the child, it must arise from one of two other sets of causes, viz., a faulty condition of the soft passages or of the expelling powers. Mr. Craig's observations refer only to derangements of the expelling powers, but he must be aware that the above definition, as he has stated it, evidently refers to the other species of dystocia also. We object to the term "protracted *natural* labour." The cases quoted by him are *very* far from being normal, or consonant with a natural course of parturition; they are cases of extreme suffering, difficulty, and considerable danger; and we are surprised at the present day to see that, in conjunction with several of his countrymen, he still continues to use the old and faulty classifications of labours.

Mr. Craig sets out with the conviction that midwifery practitioners have hitherto been in the habit of using the perforator unnecessarily, and believes that if they would only read his "Novum Lumen Obstetricantribus," they will see the error of their ways. We might excuse this display of self-importance, although it pervades the work rather extensively; but we cannot pass over the sneering remarks which are made upon authors who have published the results of their own large experience; the more so as the real state of facts is sometimes considerably warped and perverted, we trust unintentionally.

> "In the Maternité at Paris, the perforator was used only sixteen times in upwards of 20,000 cases, and the celebrated Dr. Dewees of Philadelphia in more than 3,000 deliveries did not use it once. . . . . Dr. Merriman, Dr. Collins, and many others found it necessary to employ the perforator more frequently than the above authorities. Dr. Merriman in 2947 cases used the perforator nine times, and Dr. Collins in 16,654 cases employed it 118 times. So frequent an application of so fatal an instrument by so distinguished an accoucheur as Dr. Collins startles us not a little, particularly when we are told that it was employed more frequently than the forceps. Although Dr. Collins has put his mode of practice on record, and although it is favorably noticed by the medical periodicals, yet we trust that neither he nor any one else will long continue the same course." (p. 6.)

Now this statement implies a severe censure upon the professional conduct of two gentlemen who have attained that high distinction which is so eminently due to them. If the author had read our review of Dr. Collins's work, (*Br. and For. M. R.*, Vol. II., p. 77,) he would have seen (if he had never known it before) to what circumstances is the practice at the Maternité indebted for this unusually rare use of the perforator. If a statement of this sort is to be made, the whole of it should be honestly stated and not a portion of it kept back. There is no doubt but that in many cases where the forceps has been used in Paris, the perforator ought to have been employed. The forceps used there is of great length and power, and we have no hesitation in saying that the child may by it be dragged through a narrow pelvis with a degree of force which nothing can justify. Mr. Craig has taken the statement of Dr. Merriman's practice from the work on difficult parturition by this distinguished author, and as the statistical table of the Maternité precedes Dr. M.'s report of his own practice, Mr. Craig must necessarily have read it, if he did not actually quote it from that work: candour therefore should have suggested the propriety of adding the observations which Dr. Merriman has appended at the end of this table. "No mention," says Dr. Merriman, "is made of the number of deaths among the children born without artificial assistance, but among the 334 where artificial aid was required, 91 were dead born, of which 68 appear to have lost their lives during labour and 23 were dead before the labour began: of the deaths of the mothers we learn nothing from Madame Boivin."* It would have been much more fair and candid to state the nature of the nine cases in which Dr. Merriman "found it necessary to employ the perforator" than to insinuate the charge of mal-practice by trusting that neither Dr. Collins "nor any one else will long continue the same course." Every impartial mind on referring to Dr. Merriman's table of his own practice will at the first glance justify him in his use of the perforator. Of the nine cases (or one in 328) it was used seven times from distortion of the pelvis, in four of which cases premature labour was afterwards induced, and two of the children saved; in the remaining two cases it was used "in very lingering labour, *when the want of pulsation in the presenting funis had fully proved the death of the children.*"† And yet with all this staring him in the face, the author ventures to make the following observation: "To what degree the distortions extended is not so much as hinted at, but from what we have stated above and from our own experience we can scarcely admit that they could be all of the description to prevent a child of the usual size to pass with strong efficient pains. Be this as it may, two of the children's heads were perforated on account merely of the labours being very lingering!" (p. 7.) We would in the first place ask what right has the author or anybody else to doubt the word of such a man as Dr. Merriman? Or on what grounds does he suppose that the perforator was *not* fully justified in these cases? In the two others, where the funis presented and the child was already dead, the author is guilty of misrepresentation (we still hope unintentionally) in saying that the perforator was used "on account merely of the labours being very lingering." The attack on Dr. Collins is still more personal, and what is worse, the numbers are not even stated correctly. Of the 16,654 births recorded by that gentleman, *seventy-nine* (not 118) children were delivered by the perforator, and yet with this diminished proportion, viz., one in 210 cases, it is unfair to state even these numbers without making the reader in some degree aware of the circumstances under which they occurred. "In this report," says Dr. Collins, "of the number of children delivered by the crotchet, it is necessary to bear in mind that the *proportion* of such deliveries is greatly increased in consequence of the *same patient* returning to the hospital *two*, *three*, or even *more* times, in whom from deformity or other circumstances such mode of delivery was rendered unavoidable. . . . . Other circumstances occurred where delivery was effected by lessening the head, viz., in rupture of the uterus, prolapsus of the funis where the child was dead, and convulsions; these cases are recorded under the several heads stated. In my opinion the operation under the latter circumstances is altogether different, as in such the head is lessened in order to effect delivery with the least possible risk to the mother, without trusting longer to nature's efforts owing to *immediate* and *extreme* danger to the patient, or to prevent her suffering *unnecessarily* when the child is dead."* Mr. Craig should also have mentioned a fact connected with Dr. Dewees's practice which in great measure accounts for his not having used the perforator. "The occurrence of deformity of the pelvis in this country is so very rare as not to have been even encountered by some practitioners of pretty extensive experience; as far as regards our own, we must declare that we have not met with extreme deformity in American women three times in our lives; and when it has occurred to such extent as to render labour impracticable by the natural powers, it has uniformly been with European women."† We must apologise for devoting so much time and space to points which some of our readers might suppose of comparatively slight importance; but in our capacity of reviewers we cannot pass over observations like the above without deprecating the spirit in which they are made, whatever may have been the author's precedent for doing so.

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* Synopsis of Difficult Parturition. Table ii, p. 327. 1838. † Op. cit. Table vii, p. 336.

The author enumerates four causes of protracted labour: the first "is inflammation, the second is congestion, the third an excess of sensibility, and the fourth is spasmodic." (p. 20.) We do not object to them, for there is nothing novel in the fact that such conditions frequently interfere with the regular action of the expelling powers. Why the author should have been so many years, as he informs us, in "ultimately" coming to this conclusion, we know not; for it was one of the earliest practical facts that was impressed on our mind as a student. In his sixth section, on the "Symptoms that indicate the causes of protraction," those of the inflammatory state are fairly enough, although briefly, given; they are well known; those, however, of congestion, at least as given by the author, are, perhaps, somewhat novel.

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* Practical Treatise on Midwifery, by Robert Collins, M.D. pp. 487, 8.

† Compendious System of Midwifery, § 37.

> "In patients labouring under this form of obstructing cause, the skin is below the natural standard of heat; pulse nearly natural, both in strength and in the number of beats; little thirst; some appetite, at least at the commencement of labour. Throughout this form of labour the patient manifests considerable reluctance to motion; and as the labour advances, she becomes a torpid, unwieldy mass, scarcely able to assist herself to turn in bed or come out of it. After the labour has continued a few hours, the lower part of the belly becomes hard and tender to the touch; and during the pains the woman utters a dull but plaintive cry. Little urine forms, and the patient has to be urged to pass it. Vagina and os uteri generally cool to the touch and free from pain. Bowels generally constipated. Pains not frequent, at least at the beginning of labour, but they are very distressing to the patient, and advance the child but very little. It deserves notice that in labours of this description, and sometimes in the former, there are, during the intervals of regular pains, severe grinding pains in the lower part of the belly." (p. 23.)

The first part of this description conveys no information, and the latter part merely shows the presence of what are commonly called spurious pains. His description of the other causes of protraction is equally vague and unsatisfactory, and betrays a very confused view of the subject, although the whole is laid down with a degree of self-importance which is amusing.

The author's treatment is simple: it consists in giving the patient a drachm of laudanum at a dose, and repeating it twice more if necessary. If there is inflammatory action, he bleeds; if the bowels are constipated, he gives an enema of common salt and butter in warm water; and if all this fails, he gives the ergot of rye. The effect of this treatment in the majority of cases is to excite very severe but inefficacious pains, putting the patient to intense suffering, and, in some cases, driving her to a state of excitement verging on delirium. We have perused the author's cases carefully and impartially, and can come to no other conclusion than that the uterine action had been most improperly interfered with by the unjustifiable use of over doses of opium; and that in several instances the bleeding and enema were indicated solely to calm the excitement which had been produced by the opium. We can speak from pretty large experience on these matters, and can safely affirm that we seldom or never meet with cases of such violent, irregular, and ineffective uterine action where there had not been gross mismanagement and interference; wherever this state has been induced it has been capable of being restrained by the proper use of the lancet, enemata, and the warm-bath. Opiates here we have found decidedly useful, but not in the form or dose recommended by Mr. Craig. Under no circumstances do we venture to give opium during labour, without combining it with a diaphoretic, either in the form of Dover's powder, or in a draught with antimonial wine. When a gentle equable perspiration is induced by these remedies, it brings great relief to the patient and effects a complete change in the whole character of the labour. Thus combined, and in moderate doses, we find opium an admirable remedy: it allays irritation and induces sleep, from which the patient awakes astonishingly refreshed. It is now that, with the moisture upon the skin, we feel the pulse has become slow and soft; the vaginal secretion copious, the os uteri soft, cushiony, and dilatable, not thin and dilatable, as the author has expressed himself, for such a state can scarcely exist. The author himself admits, and his cases prove it, that the laudanum does produce the injurious effects which we have attributed to such over doses.

> "For in general those cases which really require the employment of laudanum are too obstinate to be effectually relieved by any single means; and we find that, after the first dose of the opiate has been administered, the pains continue unchanged in regard to their efficiency, though in severity they are often increased. Under such circumstances, about an hour after the first dose of laudanum was administered, a second and similar dose is given to the patient; and, should this second quantity fail, in about half an hour after it has been swallowed, to produce a sensible improvement on the labour, we may rest assured that the inflammatory and other symptoms will require bloodletting, in order to remove their obstructing influence over the expulsive efforts of the mother, and to bring the labour to a speedy termination." (p. 39.)

Mr. Craig's remarks on the action of the enema are very sensible and just, but they contain nothing which is not familiar to every practitioner in midwifery. No one has pointed out the use of enemata in labour more fully than Mauriceau.

Mr. Craig states that, "in the treatment of protracted natural labours, we are uncompromising advocates for shortening their duration," (p. 37;) and he limits this duration, "in almost every instance," to "within or in about twelve hours." (p. 50.) "We have always," says he, "practised the manual assistance mentioned by Dr. Hamilton, and have never hesitated to introduce the point of the finger within the os uteri, moving it gently round within its orifice." (p. 57.) All this we object to strongly; it shows that the author must have a very indistinct knowledge of the real process and mechanism of natural labour. It is to those who entertain such views that the celebrated Naegelé refers when he says, "instead of studying the laws of nature by long and laborious observation, they presumed to dictate to her."* We are the more astonished at these views, as we know how admirably the subject has been handled by Boer, Wigand, &c. in Germany; by Denman, Merriman, &c. in England; and so lately and successfully by Dr. Collins, E. Murphy, Churchill, &c. in Dublin.

The author's views respecting the use of the forceps are none of the clearest. He does not profess to know anything about the stethoscope, as applied to midwifery, and therefore considers it useless.

In stating the duration of some of his labour cases, the author does not appear to be very accurate as to the precise time; thus, in case xii., the patient, considering herself in labour, requested him to call early in the forenoon, when she stated that she had been suffering pains for some hours previously; the os uteri was dilated the size of a shilling, and the presentation so high up as to be reached with difficulty. Having made her swallow two drachms of laudanum, and the pains being extremely violent, although ineffective, bleeding became necessary; the third drachm was also given, and, after a dreadful night of pain and vomiting, the child was born at five in the morning, as the author coolly assures us, "ten hours after the labour had actually commenced!" (p. 121.) Some of the cases seem to us striking examples of the mischievous effects of Mr. Craig's peculiar treatment. Case viii. was of a patient in delicate health and in her seventh pregnancy; on being called to her the author found that she was suffering under spurious pains, and gave her a drachm of laudanum. She "vomited repeatedly during the night," but was easy the next morning. On the following morning "the pains appeared of a genuine description"—"the os uteri was but little dilated, yet the pains had the effect of rendering its edges tense and its orifice more capacious." "During the subsequent two hours after our arrival at the patient's house, the pains continued frequent and severe, yet at the end of this period no perceptible change had taken place on the os uteri. As she had now been about three hours in labour, sixty drops were administered with a view to modify the unavailing uterine actions; and, as no alteration whatever took place at the expiration of an hour after the laudanum was given, a second and similar dose was exhibited. These two full doses of laudanum had no effect in abating either the frequency or violence of the pains, and, although we waited for an hour after the second dose was given, in expectation of a favorable change, yet the pains continued as inefficient as at the commencement of labour. The patient by this time was suffering under most agonizing pains, producing slight delirium; she could scarcely move in bed; belly hard and painful, considerable heat of skin, pulse accelerated." (p. 109.) Bleeding had of course become necessary, by which means the pains became efficient, and the child was born just in time to save her from (what the author calls) a *third* dose of laudanum. If in this case the bowels had been thoroughly evacuated, instead of a drachm of laudanum being given, at the beginning of the labour, and if this unjustifiable interference had not been repeated, we have little doubt but that the course of labour would have been very different; a mild diaphoretic after the action of the laxative would have assisted still further in rendering the pains effective, and in preventing any disposition to inflammatory action.

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* Mechanism of Parturition, Transl. p. 3.

Case xiv. is a dreadful detail of agony and, we think, malpractice; nor can it be palliated by the circumstance of the patient being an opium-eater; it indeed merits the epithet *instructive*, which, in one respect, applies to the whole book; it should be a warning to all practitioners against being carried away by false theories and blind prejudices. The os uteri had dilated to the size of half-a-crown, between four and five o'clock in the afternoon. "By eleven o'clock the pains had become very severe, but did not advance the child. This being the usual time that she was in the habit of taking a second opium pill, one of two grains was administered, but it had no effect in alleviating her sufferings or improving the labour. She continued in this state until half past twelve o'clock, when her sufferings became so intolerable that she exclaimed it was impossible she could longer support such agony. The purgative clyster was now administered, which operated well; and after its operation, sixty drops of laudanum were given to the patient. In about half an hour after these means were employed, the pains became less severe, and more effective; and by two o'clock, A.M., the child's head had advanced so far as nearly to dilate the external parts of the mother. From this period until three o'clock no change took place, except the pains being more aggravated. The patient now lay on her bed like an inflexible mass of matter, being incapable of motion without assistance; her belly, particularly that portion of it situated over the fundus of the uterus, was so hard and tender that she could scarcely bear it to be touched." (p. 126.) Bleeding, of course, became necessary; and as a last goad to exhausted nature, the ergot of rye was given, and the child at length expelled; the author does not think fit to inform us *when* this event really took place, but merely states that the patient "was little more than twelve hours in actual labour." Almost every case is described as being attended with the most violent and agonizing uterine action, which, if not caused, must, we think, have been at least severely aggravated, by these doses of laudanum.

In his fifteenth case we have the following remark: "The expelling efforts, however, soon ceased, or at least were completely counterbalanced by the resisting powers. The practice now to be pursued was very evident: that of improving the expulsive efforts by the ergot of rye." (p. 131.) We, of course, do not object to this reasoning, but it comes rather oddly to the reader after the following passage, a few pages back. "The nature of the case, the amount of remedial means used, and the stage of the labour, showing that the expulsive powers predominated over those of resistance, clearly evinced that the only safe course to pursue was to increase the expulsive powers of the uterus by means of the ergot of rye." (p. 118.)

The third chapter is "on suspended animation in new-born infants," and is marked with the author's characteristic faults. He appears to be ignorant of the two forms under which *asphyxia neonatorum* is usually described—the one a state of congestion, the other of atony and exhaustion. He remarks that, "from the relaxed and shrunk appearance which many, if not all stillborn children exhibit, one is apt to imagine that the child has continued to be emptied of its blood by the umbilical arteries, after it had ceased to receive any by the umbilical vein." (p. 137.) On this observation we remark that, in the first place, a large proportion of stillborn children present a very opposite appearance to that just mentioned, viz. one of general congestion; and, as to the child being "emptied of its blood by the umbilical arteries," we should like to know where the blood goes to? The author neither uses frictions with spirit nor the warm bath, which are looked upon by most practitioners as such valuable remedies in these cases; he places his chief confidence in inflation of the lungs, and continues this operation even for some time after the child has begun to breathe. In speaking of this operation he makes an excellent remark, which, although not new, is not the less valuable, and is not sufficiently attended to.

> "It deserves to be particularly noticed, too, that when the child begins to show symptoms of returning animation, its tongue will sometimes be so forcibly retracted, as to fill up the passage of the throat so completely as to render the inflation of the lungs very difficult; indeed, its accomplishment, in some cases, is quite impossible, until the tongue is drawn forward or pressed down by the finger, so as to allow the air to pass. The same thing occasionally takes place even after the child has respired naturally. We have felt considerable embarrassment in the management of such cases, for, after respiration had been fully restored, all at once the breathing ceased; and, on endeavouring to inflate the lungs, not a particle of air passed down the trachea

> On examining the throat, by means of the finger, the tongue was found drawn backward and rigidly fixed, so as completely to shut the rima glottidis. To remedy this evil, we introduced our fore-finger back to the root of the child's tongue, and pressed it gently forwards and downwards, when the opening became so far relieved as to allow the air again to pass, and the child soon began to respire." (p. 144.)

Case v. illustrates, we think, the impropriety of not using the warm-bath and external stimulants. The child was stillborn; there was slight pulsation in the chord; the face and body were livid and shrunk, but, on inflating the lungs, they became of a florid colour, and the heart began to pulsate strongly. The inflation was continued for at least nine hours, during which time, the author remarks, "we never observed the child to make the least effort to breathe, neither was any external motion observed in any part of the body; yet all the time the heart continued to beat, and, for the most part, the lips and face were of a florid, healthy-like colour." (p. 147.) At the expiration of this period the colour again became dark, and the heart ceased to beat. If this child had been placed in a warm bath, and stimulants applied to the epigastrium, and if a little cold water had been dashed in its face, we are of opinion that a sufficient shock would have been applied to rouse it from its insensibility. The inflating the lungs was perfectly right and proper, but other means ought also to have been used in conjunction with it.

The fourth and last chapter is on "Uterine hemorrhage after the birth of the child;" and here we think that the author even outdoes himself. We are, he says, neither to wait until we see "a small stream of blood trickling over the bedside," nor until we are informed "that there is a large discharge." (p. 164.) But we will now give a short view of the author's treatment, and, for obvious reasons, we will do it as much as possible in his own words:

> "The first step in the treatment of such cases which the accoucheur should take, is to administer with his own hand, so as to be certain, not less than ʒjss. of laudanum to the patient. The laudanum may be given in a little cold water. In cases of this description we never give less than this quantity, and we have repeatedly given ʒij. as the greatest quantity, and without any disagreeable symptom being produced, farther than occasionally, after the flooding has ceased, the patient has been very sick and vomited, which, however, is rather a favorable result. As soon as the laudanum has been swallowed, a wine-glassful of two ounces of undiluted ardent spirits, whisky, rum, or brandy, should be given to the patient, and the same quantity should be repeated in as rapid succession as the woman can swallow it, until four wine-glassfuls are exhibited. In a few cases we have given the fifth wine-glassful of spirits, but we have never exceeded this quantity, nor do we believe it to be necessary."!! (p. 166.)

Whilst this is doing, the author also directs the external application of cold, according to the usual mode. In his first case the patient took ten ounces of undiluted whisky, in about half the number of minutes; the hand was now introduced, to bring away the placenta, after which the bandage was applied to the abdomen, "when she expressed herself greatly relieved, and *very comfortable.*" (p. 126.) The author remarks, "Although we have not noticed the other usual means, they were all duly put in operation, and should never be neglected." (p. 179.) It is therefore difficult to decide how far the success must be attributed to the cold or to the spirit; we are inclined to think that, beyond making the patient "*very comfortable,*" the spirit had much less to do with making the uterus contract than the cold had.

The author lays great stress on the patient retaining her power of swallowing in these cases, and seems to think it rather extraordinary that this should be the case in the midst of great prostration. "This remarkable state of the organs of deglutition should never be forgotten by the accoucheur, and we cannot too deeply impress it upon his mind; for, as far as we know, it is either not noticed at all, or but imperfectly, by any writer on the subject." (p. 164.) We can only say, in answer to this observation, that if a patient's powers be so far depressed as to render her incapable of swallowing, she can be little else than in articulo mortis. He continues—"We have never seen a case of uterine hemorrhage after the birth of the child in which the woman had not the power of swallowing; but were such a case to occur to us, we should not hesitate to inject a pound of spirits and ʒiij. of laudanum into the bowels, and to retain it by pressure on the fundament."! (p. 189.)

Mr. Craig devotes the last section of his fourth chapter to considering "the opinions and practice of others." With this we shall not take up any more of the reader's time; for, putting aside his peculiar and, we are compelled to add, frequently erroneous views, the whole is written in so disagreeable a spirit, and many of his remarks are so personally rude, that we shall do no more than record our protest against such unwarrantable attacks.

As to his treatment of hemorrhage, after the birth of the child, we need scarcely say that we dissent strongly from it, and cannot help thinking that the number of relapses which he has described would not have taken place if the usual means for stopping hemorrhage had alone been resorted to. He recommends the "frequent introduction of the cold hand," (p. 248,) for the purpose of cooling and emptying the vagina, and considers this "the easiest way to apply cold internally." (p. 249.) Surely the author must be well aware that practitioners have long been in the habit of applying cold to the vagina, for the purpose of stopping a hemorrhage, and by a much simpler means.

Art. XII.

1. *Lehrbuch der Physiologie des Menschen.* Von Dr. Friedrich Arnold, Professor an der Hochschule in Zürich. Erster Thiel.—*Zurich*, 1836, pp. 388. Zweiten Theiles, erste Abtheilung.—*Zurich*, 1837. pp. 460.

System of Human Physiology. By Dr. Frederic Arnold, Professor in the University of Zurich. Vol. I.—*Zurich*, 1836. Vol. II. Part I.—*Zurich*, 1837.

2. *Lehrbuch der pathologischen Physiologie.* Von Dr. Wilhelm Arnold, Professor an der Hochschule in Zürich. Erster Thiel.—*Zurich*, 1836. Zweiten Theiles, erste Abtheilung.—*Zurich*, 1837, pp. 542.

System of Pathological Physiology. By Dr. W. Arnold, Professor in the University of Zurich. Vol. I.—*Zurich*, 1836. Vol. II. Part I. —*Zurich*, 1837.

In an article on the German school of physiology in our Ninth Number, we had already occasion to allude to the works of which the titles are prefixed, but the extensive nature of the subject prevented us at that time from examining them so minutely as their merits perhaps required. We have been since waiting for the completion of the works to renew our notice of them; but as so much time has already passed, we now propose to give a brief account of their contents, in their present unfinished state.

The reputation of Frederic Arnold, although of no ancient date, may already be termed European; and the name that he has acquired by his publications on the Eye and Nervous System will be amply sustained by the work now before us. The first volume is dedicated solely to the consideration of the general doctrines of the science. If this is a department which is too much neglected in British elementary works, Professor Arnold has erred by passing into the opposite extreme. There would seem to be this difference in general between the German and British mind, that whilst the former deems it necessary first to lay the broad foundation of general laws before proceeding to raise the detached structures, the latter at once commences with facts, and leaves the student to draw his general conclusions from the details which have been placed before him. The reader, therefore, who is at all conversant with general science, will be apt, on perusing Dr. Arnold's first volume, to think that much superfluous matter, relating to general physics and natural history, has been introduced, most of which, he will be inclined to say, cannot fail to be known to every well-educated general student.

In the first section of his first volume Dr. Arnold considers, 1st, the general physical and intellectual organization of man; 2dly, the differences between man and the brutes; and 3dly, the distinctive features by which the various races of mankind are physically and mentally characterized. In discussing the last topic, he adopts an arrangement of his own, somewhat resembling that of Bory de St. Vincent, and divides mankind into five great races, comprehending numerous subdivisions: I. The Caucasian race, embracing the Europeans and their descendants, the Western Asiatics and North Africans. II. The Altaic race, so called from the Altai mountains, embracing the northern, eastern, and southern Asiatics, with the exception of the Malays. III. The Indian-Oceanic race. IV. The Ethiopian race. V. The Aborigines of America. The minute anatomy of the tissues, which is fully and carefully given, concludes the first section.

The second section comprehends the relations of man to the external world, and treats firstly of the general physics of the earth and planetary system; noticing, for example, the effects produced upon man by various climates, by a pure or impure atmosphere, by proximity to the sea, by high or low-placed habitations, sudden changes of temperature, electricity, &c. It then treats of the organization on the surface of the earth, in so far as the wants of man are concerned, and under this head embraces a chapter on general dietetics. The third and concluding section refers to the general phenomena and laws of the living body, and comprehends definitions of contractility, irritability, elasticity, temperament, animal heat, animal electricity, sleep, &c.

The second volume bears the stamp of industry and intelligence impressed upon every page, and forms an excellent compendium of that part of modern physiological science of which it treats. It comprehends the processes of digestion, respiration, and circulation, and is written in a concise and distinct style. The opinions of others when differing from the author's are fairly stated, and the facts which appear to support the one or other side faithfully laid before the reader. In treating of digestion Dr. Arnold dwells largely on the interesting and important experiments of Beaumont and Eberle, but he does not seem to have been acquainted with the later experiments of Müller and Schwann, noticed in our Seventh Number. We shall extract some interesting observations from this part of the volume. After quoting the numerous and contradictory experiments as to the influence of the tenth pair of nerves (the eighth of other authors), on the process of digestion, he brings forward the following experiments, as calculated to throw some additional light on the subject.

"The experiments were performed on fowls and pigeons. In some instances the tenth pair was simply divided, but in others a piece of the nerve was cut out, and again in others the pharyngeal branches of the ninth and twelfth pair were cut in addition. The animals were kept fasting for two or three days before the operation, after which they were fed, and then allowed to live till they died in consequence of the injuries they had received. The crop, on examination, was found filled with grains of wheat; they were much swollen, and easily crushed by pressure, and were surrounded by a considerable quantity of chyme-like fluid, with a strong acid reaction. Wheaten bread which had been taken was found almost entirely chymified in the crop of a fowl. The proventriculus contained a slightly acid mucous fluid, and the gizzard a like fluid, with a stronger acid reaction, and also some grains of corn very much swollen, besides some husks and a considerable number of small pebbles. In the duodenum the fluid was more or less acid. Deglutition was not impeded by the operation, but the contractile powers of the proventriculus were considerably weakened, for in a fowl which lived for two days after the operation 329 grains out of 400 which had been swallowed, were found after death in the crop; and in a pigeon which lived for 52 hours, of 290 grains only 20 were missing. In two pigeons, in which, besides the pneumogastric nerves, the pharyngeal branches of the ninth and twelfth pairs had been divided, 29 grains were missing in the first, which lived 80 hours, out of 300 grains which had been swallowed, and in the second, which lived 66 hours, 45 grains were missing out of 350. In these cases no grains or husks could be detected in the gizzard, although in the other cases in which the pneumogastric nerves only had been cut, both grains and husks were found in that organ.

"The secretions of the crop are not suppressed by the division of the above-mentioned nerves; for the grains, although they had diminished in number, had acquired considerable addition in weight; in the crop of a pigeon, with a loss of 20 grains in number, there was a gain of 50 grains in weight, the animal having lived 52 hours; in a fowl with a loss of 71 grains in number, there was a gain in 48 hours of 164 grains in weight; and in another where there was a loss of 45 grains of wheat, there was a gain of 261 grains in weight in 66 hours; and in a third a gain of 335 grains in weight in 80 hours, with a loss of 29 grains in number. The animals did not appear to die from starvation, but from the effects of impeded respiration. It results from these experiments, that the secretion of an acid fluid in the crop, proventriculus, and gizzard, is not suppressed by the division of the pneumogastric nerves, nor, judging from the quantity of fluid, would it even appear that it is lessened in quantity. The chymifying properties of the fluid remain the same as where there is full integrity of the pneumogastric nerves; and digestion, in so far as it is dependent upon the gastric juice, is not destroyed by the operation. The contractile powers of the proventriculus, however, are weakened, although neither they nor the tributary powers of the gizzard are completely destroyed. It appears, likewise, that simple division of the nerve is followed by the same consequences as excision of a part of its substance. The nerves of the stomach, therefore, appear to exert an influence upon chymification, in so far as this process depends upon the various motions of the organ." (p. 80.)

The following observations on the gradual conversion of the chyle into blood are equally interesting:

"The mucous membrane of the alimentary canal imbibes the fluid contained in the intestine precisely on the same principle as a sponge becomes saturated with water. The lacteal vessels do not, as has frequently been asserted, present open mouths upon the surface of the membrane, but they ramify through its substance along with the capillary blood-vessels. The structure of the walls of these capillary blood-vessels would appear to differ from that of the walls of the lacteals, and this difference affords the reason why the chyle passes into the lacteals only, and is not taken up, like the other fluid contents of the intestine, by the capillary vessels. The transformation of the chyle into blood probably commences in the tissue of the mucous membrane; a reciprocal action takes place in the substance of the membrane between the blood and chyle, and certain principles of the former are imparted to the latter, such as the red colouring matter, fibrine, and perhaps alkali. This constitutes the first change in the chyle, and disposes it to form globules. The chyle assumes more and more the characters of blood with each mesenteric gland through which it passes. The same reciprocal action takes place here, the red colouring matter and fibrine pass from the blood into the chyle; and it is probable, too, that the blood parts with some of its oxygen, by which the albumen of the chyle becomes changed into fibrine." (p. 149.)

"It is probable, from what has already been said, that the formation of the globules of the chyle commences in the lacteal vessels during the process of absorption. At least the examination of chyle taken from the lacteal vessels close to the intestinal canal corresponds, in the small quantity of globules which it contains, with the observation that it does not coagulate before its passage through the mesenteric glands, and that its coagulating power and the number of its globules become greater the nearer it reaches the extremity of the thoracic duct. Besides, there is no sufficient evidence to show that fibrine is formed in the intestinal canal and passes from it into the chyle; this opinion, on the contrary, is opposed by the facts already noticed, and it is rendered improbable by the consideration that the fibrine of the aliment ceases to exist, as such, in the chyme, but is changed into albumen or some other animal principle. The number of the globules of the chyle increases with its progress in the lacteal vessels, a fact which the examination of a drop of the fluid before and after its passage through the mesenteric glands, or in the lower or upper portion of the thoracic duct, readily proves. The chyle, when it has reached the extremity of the thoracic duct, is very rich in globules. These are not all alike, some of them being one third in diameter less than others. The smaller are perfectly globular, but the others, from their central depression and marginal ring, as likewise from their chemical relations with water, appear to be identical with the globules of the blood. Some of the latter, however, when examined singly beneath the field of the microscope, show no appearance of the red colouring matter, but this appearance of individual globules is not necessary to prove that the red colour of the chyle depends upon the globules of the blood. A careful examination of the chyle proves further that the red colouring matter does not occur in it in a dissolved state, but, as in the blood, united with the globules; a fact which I have had occasion to observe, not only in the coagulum, but also in the serum of the chyle of dogs, the red colour being quite sufficient to point out the presence of cruor even to the naked eye. The supposition, therefore, that the colouring matter occurs in the chyle in a dissolved state, is unfounded. The circumstance that the chyle, and also the lymph which has been in relation with the blood through the medium of the lymphatic glands, contain, besides the completely round and smaller lacteal globules, other larger globules, which correspond in size and shape with those of the blood; the observation that the number of the latter augments with the progression of the chyle in the lacteal vessels, whilst the number of the former undergoes a relative diminution; and lastly, the fact that the central nuclei of the larger globules correspond in size and shape with the smaller, leave no doubt that the globules of the blood are formed from the smaller globules of the lymph and chyle, which, from their affinity to the red colouring matter, attract this substance from the blood with which they come in indirect contact, through the walls of the capillary vessels." (p. 174.)

If these views be correct, the globules of the chyle must always stand in a definite relation to the globules of the blood. But this assumption is combated by many able physiologists. According to Müller, the lacteal globules are sometimes equal in size to those of the blood, as in the cat; in other instances, as in the dog and goat, they are rather smaller; and in others again, as in the rabbit, much smaller than those of the blood. Hewson found the lacteal globules in man resembling in size and shape the central nuclei of the globules of the blood, and Prevost and Dumas give their size as rather more than half the size of the globules of the blood.

In the chapter on Respiration, Professor Arnold strongly expresses his dissent from Sir Charles Bell's views as to the respiratory tract of the spinal cord.

> "It has been supposed," he says, "that the nerves subservient to respiration form a peculiar system, that of the irregular nerves, in opposition to the system of regular nerves on which motion and sensation are dependent. To this system have been reckoned to belong the third, fourth, and sixth pairs for the eye; the twelfth for the tongue; the ninth for the pharynx; the seventh for the face; the tenth for the larynx, heart, lungs, and stomach; the phrenic for the diaphragm; the accessory of Willis for the humeral muscles, and the external respiratory for the thorax. They are distinguished by their origin by one root only, and by having no ganglion. According to this hypothesis, the seventh, ninth, tenth, and eleventh pairs of cerebral nerves, the phrenic and external respiratory nerve bring the lungs and respiratory muscles into mutual relation, and arise in one line, forming a particular column of the spinal marrow, which lies between the olivary bodies and crura cerebelli, and which may be traced along the lateral aspect of the spinal marrow between the grooves, for the anterior and posterior roots. These nerves may be cut, and thus several muscles be prevented from taking part in the act of respiration, without impairment of their voluntary motion. Thus when the eleventh and seventh pairs are cut, the respiratory motions only of the humeral and cervical muscles are interrupted, but the power of voluntary motion which they derive from other means is not destroyed. To this hypothesis, which easily deceives by its simplicity, but which on nearer examination presents many errors and discrepancies, it may be objected that the respiratory and voluntary motions are allied, and by no means opposed to each other; and that many of the so called regular nerves take part in the function of respiration, as for example, all the thoracic nerves; secondly, that the origin of the irregular nerves from a lateral tract is not only not proved, but that it is, in the case of several of them, unsusceptible of proof; thirdly, that some of these nerves, as the ninth and tenth pair, have ganglions; and lastly, that it does not depend upon any specific property of the nerve whether such or such a motion is produced, but whether the nerves supply this or that muscle." (p. 220.)

Innumerable theories have been advanced to explain satisfactorily the nature of the changes which the blood and air undergo during the act of respiration. Another has been propounded by Professor Arnold, which, in many of its leading features, corresponds with that advocated by Tiedemann, Gmelin, and Mitscherlich.

> "As it is more than probable that the carbonic acid occurs in the venous blood, united with some substance from which it is separated with greater or less rapidity by the contact of the atmospheric air, and as, further, the carbonate of the protoxide of iron greedily withdraws oxygen from the atmosphere, at the same time parting with its carbonic acid and becoming changed into a peroxide, it may reasonably be supposed that the carbonic acid of the venous blood is united with the iron of the red colouring matter, and that it is set free during the act of respiration, by the reciprocal action of the blood and air. The protoxide at the same time, by absorption of oxygen becomes a peroxide, which, during the circulation of the blood through the

capillaries, again parts with its oxygen. Carbon is at the same time eliminated from the blood, and unites with the liberated oxygen to form carbonic acid, which is thrown out by the lungs, whilst oxygen is again absorbed." (p. 252.)

This theory has likewise been advocated by Maack, in his treatise "De ratione, quæ colorem sanguinis inter et respirationis functionem intercedit," where various chemical experiments are adduced in support of it.

We have given the above extracts chiefly as specimens of the work. Throughout the whole volume we find a careful selection of facts, with cautions and in general just reasoning. It contains, besides, the results of various of Dr. Arnold's own experiments, which are generally important. In criticising the opinions of others, Professor A. shows, with one exception, the strictest impartiality; but when speaking of Müller, of Berlin, he occasionally displays a spirit of ill-disguised jealousy.

We may here remark, that the works of the two brothers are written upon the same plan, and embrace the same number of sections and chapters; but Frederic treats only of healthy, and William of pathological physiology. The volume on general doctrines, prefixed to the pathological physiology, by the latter, is burdened with all the faults already remarked in the corresponding volume of his brother's work, whilst it possesses few of its redeeming qualities. The statements are so lax and general, that the reader rises from their perusal with no definite or fixed impression. In the first section, on the organization of man in the diseased state, with reference to general deviations from the healthy standard, we find some remarks on the moral and intellectual faculties; on the modifications they undergo by sex, age, climate, and disease; on the distinctive characters of man and the brutes; and on the characteristics of giants and dwarfs. The remarks on the diseases of the different tissues may be useful, by rendering the beginner familiar with their names, but otherwise they can be of little value. The second section comprehends observations on the influence of the sun, moon, and planets; of light, heat, cold, magnetism, and electricity; and of the various winds in producing and modifying disease. Some of the remarks in this chapter are sufficiently curious, but, as usual, deficient in precision. The following passage will startle some of our cautious readers.

"The relation of the moon to the earth, and its influence on man, and on the production of human disease, have in all ages furnished copious matter for controversy. Whilst such influence has been totally denied by some, by far too much has been referred to it by others, whose assertions, however, are not always borne out by facts. But it results from incontrovertible facts, that the cause and aggravation of many diseases depend upon the lunar influence. At change of moon, chronic swellings, particularly those of glands, as of the thyroid, vermicular affections, and many other diseases, decrease or become aggravated. On it depends the origin and crisis of many diseases, particularly of fevers; and these relations of cause and effect are said to be very manifest within the tropics. At new moon nervous diseases, such as epilepsy, chorea, somnambulism, often become aggravated, and their attacks are then more frequent and severe. In the wane, many tumours and vermicular affections are said to yield to remedies much more readily than at other times, an opinion which is in general repute with the vulgar." (p. 96.)

He thus speaks of the effects of magnetism:

"On the application of the north pole of the magnet, the patient becomes aware of a prickly, vibratory, pulsating sensation in the point of contact, of a determination of blood to the part, and of a feeling of heat or cold. If of a melancholy disposition, he often complains of great uneasiness, sleepiness, or general soreness. Febrile symptoms occasionally ensue, with heat, sometimes of the whole body, sometimes only of a single part, as of the head, whilst the extremities may be cold. Pain in different parts of the body also frequently follow, such as toothach or headach, accompanied in some cases by giddiness, impaired memory, or even loss of consciousness; by irritation, or even deception of the organs of the senses." (p. 106.)

It would not be difficult, on reading these quotations, to suppose that they were extracts from some author of the middle ages, instead of being the production of an existing professor of the university of Zurich. In another part, we are cautioned against the immoderate use of snails and oysters, as, by furnishing too large a supply of material for the manufacture of semen, they may exert a deleterious influence on the genital organs. The whole volume abounds with crude notions of a similar description, which detract greatly from its value, as it is impossible for the beginner to separate the chaff from the wheat.

The third section, which treats of the more prominent general symptoms of disease, such as increased or diminished irritability, increased or diminished animal caloric, of the days of crisis, &c. contains nothing which calls for notice.

We have very little to say of Professor William's second volume. He goes over the same ground with his brother, and notices the pathological changes in the structure of organs, the consequent disorder of their functions, and other abnormal phenomena. The work is more characterized by plodding industry than original talent; it is almost entirely a compilation, and is open to one grievous objection, that as much time, space, and care are devoted to the most trivial subjects as to the most important doctrines of pathological science.

Art. XIII.

Medical Notes and Reflections. By Henry Holland, m.d. f.r.s., &c. Physician Extraordinary to the Queen.—*London*, 1839. 8vo, pp. 628.

This volume consists of a number of detached essays on various subjects relating to the philosophy and practice of medicine. Taken collectively, they belong to a class of writings which are not numerous, because they require for their production a character of mind and a range of acquirement rarely to be met with. Borrowing an illustration from the fine arts, we may compare them to simple designs, which, if drawn by an ordinary hand, would be meager and unimpressive, but which, under the hand of a master, are made to embody just and vivid conceptions, and evince not only more power, but more *learning* than many an elaborate picture on which all the minor resources of art have been expended. The very absence of detail serves to show the truth and vigour of the design: inferior works owe their whole effect to the details. An essay of the kind alluded to results from the mature reflection of a mind naturally capacious, and well disciplined by liberal studies. Such works, in medicine, are nearly peculiar to the literature of our own country. They were more frequent in the last age than they are in the present, and accorded well with a turn of mind conspicuous in the old

English physician—a character whose evanescence from the drama of real life, though perhaps an inevitable consequence of the more general diffusion of knowledge, we, for our own part, shall never cease to regret. In the spirit which pervades them, Dr. Holland's *Notes and Reflections* are redolent of the good old school; they have the same philosophic air, the same tincture of good letters, and the same exemption from the vice of book-making, which constitute leading excellencies in the writings of Mead, and those of Ferriar, more recent in date, but of similar character. While, however, they remind us, in these respects, of some of the productions of the bygone time, they are, nevertheless, perfectly adapted to the present state of science; indeed, it is but justice to our author to say that we have seldom met with a volume treating of so great a variety of subjects, the writer of which has shown himself so completely *au niveau du siècle* in them all.

But we must proceed to give the reader some idea of the contents of Dr. Holland's book. The first chapter, or essay rather, is "on medical evidence."

> "There can be few better tests of a sound understanding," says our author, "than the right estimation of medical evidence; so various are the complexities it presents, so numerous the sources of error. The subjects of observation are those in which Matter and Mind are concurrently concerned—matter under the complex and subtle organization, whence vitality and all its functions are derived; mind, in its equally mysterious relations to the organs thus formed—both subject to numerous agencies from without—both undergoing great changes from disease within. Individualities of each have their influence in creating difficulties, and these amongst the most arduous which beset the path of the physician. Few cases occur strictly alike, even when the source of disorder is manifestly the same. Primary causes of disease are often wholly obscured by those of secondary kind. Organs remote from each other by place and function are simultaneously disturbed. Translations of morbid action take place from one part to another. Nervous affections and sympathies often assume every character of real disease. While remedial agents are rendered uncertain in effect by the various forms of each disorder, by the idiosyncracies of the patient, by the difficulty of securing their equal application or transmission into the system, and finally by the unequal quality of the remedies themselves." (p. 1.)

Such being the complexity of the data on which rational belief in medicine is founded, it is not wonderful that the mass of mankind has ever been prone to the most abject credulity, and a prey to every species of imposture; but it does appear somewhat unaccountable that physicians themselves, in their appreciation of the merits of doctrines and the efficacy of remedial means, have shown themselves scarcely less credulous than the multitude.

> "Else whence the so frequent description of effects and cures by agents put only once or twice upon trial; and the ready or eager belief given by those who, on other subjects, and even on the closely-related questions of physiology, would instantly feel the insufficient nature of the proof. Conclusions requiring for their authority a long average of cases, carefully selected, and freed from the many chances of error or ambiguity, are often promulgated and received upon grounds barely sufficient to warrant a repetition of the trials which first suggested them." (p. 2.)

Our author anticipates a very improved era of medical enquiry from he introduction of numerical methods, averages, and statistical statenents. These, doubtless, are of inestimable value in hands capable of onstructing and using them aright; but we apprehend that such capacity can exist only in those whose minds are already imbued with the principles of just reasoning, and informed with the spirit of universal science; while, therefore, we hail the announcement of more correct methods of investigation, we have yet to lament the too limited extension of that frame of mind which alone can render them available, and which is only to be attained by directing the path of the medical student through the fields of general philosophy. *Διο σχεδον των τε περι φυσεως οἱ πλειστοι, και των ιατρων οἱ φιλοσοφωτερως την τεχνην μετιοντες, οἱ μεν τελευτωσιν εις τα περι ιατρικης οἱ δε εκ των περι φυσεως αρχονται περι ιατρικης.**

Dr. Holland concludes his chapter on medical evidence with a few remarks on undue scepticism. This is a real evil, but of minor extent, as well as less frequent occurrence, than credulity. The same remedy applies to both of these vicious conformations of mind. Habits of cautious induction and disciplined exercise of the reasoning powers will teach us alike to embrace what is true, to reject what is false, and to suspend our judgment on what is doubtful—in which third category a large proportion of what we call "medical science" is still unhappily included.

Chapter ii. is "on hereditary disease." Our author here admits, as the basis of his reasoning, the general law developed by Dr. Prichard, "that all original or connate bodily peculiarities tend to become hereditary; while changes in the organic structure of the individual, from external causes during life, end with him, and have no obvious influence on his progeny." This law, however, is recognized only as a general one, subject to many exceptions, as in those instances where necessities of situation alter certain parts of the organization of animals, and where the continuance of such altered structure is needful for their preservation under new circumstances; a case of which the domestication of animals affords a familiar example. The hereditary tendency to disease shows itself either in the anormal conformation of particular organs and textures, or in the presence of various morbid products; which products may be conceived to owe their origin to the variations of organic texture, or to transmissible peculiarities in the blood and fluids. With respect to hereditary peculiarities of texture, the scope of Dr. Holland's argument is to show their dependence on laws similar to those which give rise to the more marked anomalies of organization or monstrosities. In pursuing this theme he displays an intimate knowledge of the doctrines of transcendental anatomy. This new and extraordinary study has grown gigantic, even in its infancy. It has flung down the barrier between physical and abstract science, by indicating points where they blend, without being lost in each other; and in presenting to the mind a rational type of material forms subjected to our senses, has in some sort realized the subtleties of Plato, and revealed to us the *idea* stamping its impress upon matter. It has already extended its influence into every department of physiology and zoology; and it is difficult to conceive the possible limits of its application. Few attempts, however, have hitherto been made to bring it to bear on pathology, although there can be little doubt that it has, in reality, the most important relations to this branch of science. M. Serres appears to have entertained the notion of such relation more distinctly than any former writer; but Dr. Holland, in the paper before us, has actually encountered the subject, and much credit, we think, is due to him for the bold yet judicious manner in which he has handled it. The following observations on hereditary tendencies to disease, based on the principles of philosophical anatomy, evince much condensation of thought:

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* Aristot. Lib. de Sensu et Sensibili, cap. i. Ed. Duval.

"Examples of the anormal conformation of particular organs, transmitted by descent, are alike numerous and familiar. If peculiarities of external form and feature, whencesoever originally derived, tend so speedily to become hereditary,—affecting, as we see on every side, not families alone, but, by intermixture and descent, whole races of mankind,—we can have no doubt that deviations of internal structure, (whether they be of deficiency or excess, or of any other nature,) are similarly transmitted; and with them propensities to, or conditions of, morbid action in the parts thus organized. Though the direct proof is not equal for the two cases, and though the effects resulting are of such different importance, yet is it certain that the peculiarities, so carried on from one generation to another, have reference for both to one common law. And it is to the same principle that we must look for explanation of the difference in the average duration of life in different families;—a fact well attested in itself, notwithstanding the many exceptions which the laws or social usages of mankind are ever inducing upon it. Those deviations from the primitive or common type of the species, which occur chiefly in the bony structure, integuments, or muscular fabric; producing varieties in the outward form and feature, in the texture or colour of the skin, hair, &c., may exist to great extent, without affecting in any important way the health or natural functions of the individual. On the other hand, much smaller deviations from this type, in the internal organs of circulation, respiration, digestion, absorption, or secretion,—or in the brain and nervous system,—may produce morbid actions, painful in progress and fatal in result: each class of deviations alike transmissible to progeny, under the same general law. This distinction is obviously one of summary importance in the history of disease, and capable of very wide application. It throws light upon the connexion of various morbid states, by giving the relation to a common physical principle in their cause; and this principle one which is associated with other of the more general laws of life. There is no reason to doubt that hereditary peculiarities of structure are as frequent and varied, perhaps as extensive, in the internal organs, as in the external parts of the body. Analogy would suggest this as probable, and it is confirmed in great measure by observation; more extensively as the examination is rendered more minute. On the same grounds it may be presumed that there is a general ratio between the resemblance of external features and that of internal parts of structure. The child most like its parent in traits of countenance and figure has probably closest kindred with him in other and more minute points of conformation. The evidence here is chiefly that of similarity of morbid affections in such cases—a fact almost indisputably ascertained, and which, if the views contained in this chapter be correct, affords the best proof we can reasonably seek for. In the instance of gout it has been observed, that the children most resembling the gouty parent have greatest liability to the disorder. This can only be explained by supposing a corresponding likeness in those parts of internal structure which are chiefly concerned as causes or seats of the disease. It must be admitted, indeed, that this enquiry is still incomplete in many of its parts; and, on first view, it might appear that such internal deviations were much less extensive than those of outward conformation. But we can scarcely name any organ of importance which does not afford evidence of diseased actions, derived from structure and transmissible by descent. And looking to the textures more widely diffused through the body,—as the different vascular systems, the nerves, &c.,—we have every reason to suppose, though the proof be less direct, that they are subject to hereditary variations of structure, not merely in detached parts of each system, but throughout those minute branches and terminations where the most important functions of the body, both animal and vital, may be presumed to take place. On a subject of this nature, however, it is not sufficient to refer merely to the vague division of external and internal

> parts. The distinction between the animal and vital organs is at the foundation of every such enquiry, and each particular question must be brought into connexion with it. The fact may be considered as ascertained, that the vital organs are subject to more frequent and extensive deviations from the natural type than those of animal life; the principle of symmetry (essential it may perhaps be deemed) in the latter, requiring, for the integrity of the functions, that all such deviations should be limited in extent. It is a remarkable attestation of this fact, that where anormal varieties of the muscles occur (a much rarer event than in the vascular system), there seems a strong tendency in these varieties to become symmetrical for the two sides. A presumption might hence arise, that the tendency to transmission by descent would follow the same law, and the parts belonging to organic life offer more frequent cases of hereditary malconformation than the animal organs. I know not that this question of relative frequency has ever been explicitly answered. There is, however, enough of proof to make it probable that no disproportion exists of the kind just indicated; and that anormal structure in the parts of animal life is quite as liable to be transmitted as in the organic. Taking the obvious instances, indeed, these may appear more numerous; but as the difficulty of observation is much greater in the latter case, it is enough to rest in the fact that both the great divisions of life are liable to this general law, and probably not in any different degree." (pp. 16-19.)

The author adds some curious examples of hereditary defect, which have fallen within his own observation, and suggests the probability of a similar origin of some endemic diseases, as goitre and plica polonica, the prevalence of which, in certain regions, has received no satisfactory explanation from local circumstances or modes of life. He is inclined to refer the great frequency of stone in the bladder, in certain districts, to a like cause, and considers the probability of this view as increased by the well-known connexion of the calculous with the gouty diathesis.

The following is a curious observation, made by Dr. Holland, with respect to *trismus nascentium.*

> "When in Iceland, in 1810, I had the opportunity of collecting some facts as to the singular frequency of this disease in the Vestmann Isles, on the southern coast of this island. On these desolate rocks, the population of which does not exceed 160 souls, I found that, in a period of 25 years, 186 infants perished of this disorder, under the age of 21 days; of which 161 died between the fourth and tenth days after birth; 75 on the eighth day. Though the condition of life of these poor people is singularly destitute, fish and the eggs of sea-fowl being their sole aliment, yet is it not so different from that of the Icelanders of the mainland as to explain the frequency of this fatal disorder among them; and it would seem as if some constitutional and hereditary causes were concerned." (p. 22.)

In this instance we think that some local cause, operating on the nervous functions of the individual, is likely to be much more influential than hereditary vice of conformation, especially as such causes may readily be conceived to abound in islands of volcanic origin. In general, however, admitting the premises with which we set out—that all congenital peculiarities of structure tend to become hereditary, and that many morbid dispositions are connected with peculiarities of intimate structure—it becomes evident that hereditary predisposition must have great influence in the perpetuation of endemic diseases, since the extrinsic causes which first impress a peculiarity of structure must operate on each successive generation with the increased force derived from a transmitted susceptibility.

As a general expression of facts, Dr. Holland considers it established "that no organ or texture of the body is exempt from the chance of being the subject of hereditary disease,—or, in other words, every part is susceptible of deviations from the normal type or natural structure, capable of being conveyed to offspring, and of producing morbid actions, which are thus, under the name of disease, propagated through successive generations." (pp. 22-3.)

A singular variety of this law is that which has been called *atavism;* where an anomaly or disease, existing in a family, is lost in one generation, and reappears in the following. Dr. Holland remarks that this, in some cases, may depend on change of sex, and cites an instance known to himself of hydrocele occurring in three out of four successive generations in one family, the omission being in the person of a female, in whose son the disease reappeared. (p. 20.) He well observes that there may be analogous cases, in which the structure affected by hereditary disease is such as to be disguised or superseded by other casualties in the bodily conformation of the individual. There are numerous instances, however, where sex and all other obvious circumstances being the same, the anomaly or disease is missing in one or more individuals of a family series and recurs in their children. It is with the external lineaments as with the peculiarities of internal structure, and we often find some strongly-marked feature of face or form lost in one individual of a family and restored in his offspring. Physiology is entirely at fault in the solution of such phenomena; but, says Dr. Holland, they all concur to establish an unity of plan and a general relation among them, "which makes the simple resemblance of an external feature the exponent of other cases, in which the most severe diseases are conveyed from parent to offspring." (p. 23.)

Another inexplicable circumstance in the transmission of disease is that where several children of a family are affected in common with some malady of which no indication has appeared in either of the parents.

> "An example," says our author, "has lately occurred to me in one family, of three sons and a daughter, every one of whom underwent an attack of hemiplegia, before the age of forty-five, though neither father nor mother had been similarly affected. I find another instance in my notes, where three brothers severally suffered hemiplegia, and about the same period of life, without any record of the like event in the family. I have recently seen a fatal case of cerebral disease, with epileptic fits, in a young lady of twenty-four, two sisters of whom had died about the same age, with similar symptoms, though neither parent had been subject to such disorder. I am acquainted with a family in which four children have died during infancy from affections of the brain, without any like instances in the family on either side. In another family, without any similar disease in the parents, three or four children had epileptic fits. I have notes of several similar instances: chiefly, as I think, but not exclusively, disorders of the brain and nervous system. I have known three cases of diabetes mellitus in brothers, under ten years of age, in the same family; one of them fatal in result. In another instance, four cases of ascertained disease of the heart, all fatal about the same period of life, occurred to my notice in the brothers and sisters of one family, without any suspicion, as far as I could learn, of the parents having been the subjects of this disease. By a singular coincidence, another instance is known to me, where four brothers died, between sixty and sixty-five, of ossification and other disease of the heart: but here there appear to have been prior cases of the same kind in the family. In the instance of the deaf and dumb, already referred to, the examples are frequent and curious of several children being thus affected (five out of a family of eight, four from a family of seven), without similar defect existing in the parents. At the school for the deaf and dumb, in Manchester, in 1837, there were forty-eight

children taken from seventeen families, the total number of children in these families being 106; and giving, therefore, an average of nearly three such cases in each family. Out of these instances there appears but *one* in which the defect was known to exist in either parent; and we may rightly, therefore, consider this as one of the most striking examples of the fact under consideration." (pp. 24-5.)

The remainder of this essay is occupied with points of great interest, but very difficult solution. Such is the enquiry into those diseases which appear to depend mainly on the formation of morbid products, and the pathology of which involves the question how far the tendency to the formation of such products resides in a peculiar organization of the solid parts, and how far in a taint of the blood. In reference to this question allusion is made to the subject of gout—which is more largely commented on in a subsequent essay—that of scrofula; the pellagra of Lombardy; and other affections. Among the hereditary disorders of the brain and nervous system, insanity holds the most conspicuous place. Our author does not dilate on the subject, but suggests, in a note which we transcribe, a notion highly worthy of attention.

"Here again, as so often before, we are called upon to note the close relation of particular morbid phenomena to more general laws; and, in the present case, to those laws which determine the varieties of character in nations and communities of men. Hereditary deviations in excess, such as come under the character of insanity, are corrected or limited by the usages of society. Those variations from the common type (if we may apply this term to mind as well as body), which are not so controlled, may, in the infancy of any community, and in combination with other causes, become the basis of those more permanent traits which we designate as the character of a people. Such diversities existing, as they actually do, and being in many cases perpetuated within human record, must be derived in some part from this source. Or if we could suppose it otherwise as to origin, it must at least be admitted that this cause is concerned in their perpetuation among races of men." (p. 32.)

Dr. Holland concludes by enquiring to what extent the changes of organization, or other material causes of hereditary disease may continue their progress through successive generations, and what are the limitations to such changes? In attempting the solution of these questions he again happily avails himself of the analogies afforded by philosophical anatomy; and admitting, as most probable, that throughout the varieties and anomalies of organization there are fixed laws, by which the permanence of each species is secured, so likewise, in the less obvious changes of structure or condition on which the hereditary tendencies to disease depend, there is a certain point, up to which the repetition of the modifying circumstances tends to augment and confirm the variation; but beyond which they are restrained by the more general laws of organization, and the original type of the species, which define their extent "differently, perhaps, in different parts of the structure, but still with an eventual and certain limit to all."

We have dwelt at considerable length on this essay, on account both of the importance and novelty of the subject, and the very able manner in which its outline has been traced by Dr. Holland, who remarks, that a work embracing it in its whole extent is yet a desideratum. Such a work would open a very rich and profitable field of research; but he who would explore it must be a man of no ordinary endowments; for he must be at once a physiologist of the first magnitude, and an experienced, observing, and learned physician.

Chap. iii. is on "Bleeding in Affections of the Brain." It appears to us that a very general error has obtained in the reasoning of pathologists with respect to the symptoms of disease of the brain. This, like other organs, has its own individual properties of texture and modes of nutrition, which are liable to various perversions; but, unlike any other organ except the heart, its influence is so diffused over every part of the frame, that it may be said to exercise an universal function. Now, the error we refer to is that of confounding the effects of deranged function of the brain on the entire organism, with the symptoms of some particular lesion of the brain itself; whereas, in reality, such derangement of cerebral function, with all its extensive consequences throughout the system, may arise from the most opposite pathological conditions of the brain,—conditions which agree in nothing except in disturbing the functions of the great centre of animal life, but hence producing analogous effects on various remote and dependent functions. For example, we find a patient insensible and deprived of voluntary motion, with dilated pupils, stertorous breathing, and convulsions; and we say these are the symptoms of compression of the brain. But they are not so; they merely indicate the lesion or suspension of certain functions of the brain, which may arise from various causes, of which compression is one; and the circumstance that compression is, perhaps, the most frequent of these causes, does by no means connect the symptoms alluded to in a more necessary relation to this than to any of the other causes capable of producing similar derangement of the cerebral functions. Viewing the matter in this light, we doubt much if it would be possible to state the characteristic symptoms of any one pathological condition of the brain, except acute inflammation, in which, as in inflammation of other organs, the nature of the morbid action is indicated by the febrile excitement of the system, and its seat by the local pain.

If the foregoing remarks be just, it is not without reason that Dr. Holland asks, "is not depletion by bleeding a practice still too general and indiscriminate in affections of the brain, and especially in the different forms of paralysis?" He adduces various instances which show that this question must be answered in the affirmative, and that various states of diminished cerebral power produce symptoms which might lead to the destructive use of the lancet. Such are the delirium of typhoid fever; the acute and throbbing pain of the head, and general excitement of the system, consequent on profuse hemorrhage; the vertigo and delirium arising from starvation—the phenomena of delirium tremens; the cases of infantile disease in which symptoms of cerebral excitement follow excessive depletion;* and the experiments of Sir A. Cooper, in which it was found that the tying of the two vertebral arteries brought on various species of paralytic as well as spasmodic affection.

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* The pathology of the brain in children seems yet more obscure and intricate than in the adult; for not only do the symptoms of cerebral exhaustion simulate those of excitement, but the state of real and active inflammation in very young children is often strangely masked. According to our own observation, profound coma, dilated pupils, and convulsions, sometimes coexist, from the commencement of the attack, with high febrile excitement of the system; and this in cases where, on dissection, the appearances of acute inflammation alone are visible, without any cause of compression.—Rev.

Some judicious remarks follow on several of the symptoms common to different and opposite states of the brain; as coma, vertigo, lightheadedness, headach, the degree of contractility of the pupil, and the susceptibility of the retina.

The following observations on the treatment of palsy are replete with practical wisdom; and the cautions they suggest, though, as the author admits, familiar to many, are not, in general, sufficiently kept in view.

> "Even were the tendency to paralytic seizures as generally lessened by bleeding as common practice could imply, it does not thence follow that abstraction of blood from the brain should be needful or desirable in immediate sequel to such attack. In many cases it is undoubtedly otherwise. The paralysis, when depending on apoplexy, with extravasation of blood or serum, or on other cause of continued pressure, may come on by degrees, and admit of relief in its progress by emptying the vessels of the head. But often it occurs as an instant shock to a portion of the brain or spinal marrow, without any proof of extravasation or obvious cause of pressure;—the shock itself being of momentary duration, though it leaves lasting effects on parts of the nervous system thereon depending. In these cases, and they are frequent, the physical causes of the change are little known to us. There are reasons for supposing that the nervous substance itself is often primarily affected. We have certainly no sufficient proof of mere pressure from fulness of vessels being concerned, to warrant large bleeding, especially after the stroke of palsy has actually occurred. The degree of coma attending and following these seizures is not alone sufficient cause for the practice; and will usually subside without it, where the original attack is not such as to endanger life.
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> "Looking indeed to the magnitude of the event between, common reason would suggest a doubt whether the same treatment can be desirable immediately before, and after a stroke of palsy. I do not mean to give this the weight of an argument. From the nature of the circumstances, it is extremely difficult to bring unequivocal proof on the subject; but there is much cause to believe that the practice of bleeding in the latter case is often injuriously pursued. The risk, I believe, will generally be less from waiting a certain time,—to observe the effect of what has occurred upon the circulation, the breathing, and the sensibility,—than from hastily taking away blood, at the moment of a great shock to the brain, and before we can rightly appreciate its consequences. This effect upon the greater functions of life gives us in fact the best information we can have in guidance of further practice. But this we forfeit in great part, by the disturbance any large depletion makes in the system, and particularly in the organs upon which these functions depend. The practical importance of this consideration may readily be understood.
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> "Even where evidence is obtained of the fitness of bleeding soon after one paralytic attack, for the prevention of another, the question still remains as to the manner of this,—whether by copious depletion at once, or by smaller bleedings, repeated as observation may suggest. And this question the practitioner, while prepared for boldness in all fit and urgent cases, is bound always to keep before him; seeing especially that any great excess in the remedy may hurry on the very mischief it is sought to prevent. I believe that in most cases the latter method is to be preferred. It accords better with the state of our knowledge of these disorders; involves no irretrievable step; and in its progress affords the information most requisite to decide how far it should be carried into effect. Paralytic cases there presumably are of such nature, that a few ounces of blood taken away at regular intervals will ward off a recurrence of the attack, which any large and sudden depletion would probably hurry on. The proof here can seldom be explicit; but the presumption is one I have often been led to entertain." (pp. 44-6.)

Chapter iv. is on "Sudorific Medicines." There is reason to believe that what is called perspiration involves two distinct processes, one consisting in simple exhalation from the surface, and therefore purely physical in its nature, the other in a vital process of secretion, which is therefore influenced by the various states of the circulation and nervous functions. The latter only is the point of view in which it can be regarded with reference to the operation of medicines. The general tendency of our author's observations is to show that the relief which accompanies natural diaphoresis in many diseases is not necessarily dependent on such evacuation, and that the sweat may, with as much probability, be regarded as merely the sign of an altered state of the system on which the improvement depends. He instances the supposed critical perspirations which occur in fever, and adverts to the familiar fact, that remission or intermission frequently takes place, with accompanying alterations of the state of the skin, but with little or no actual perspiration succeeding the hot stage. Even in the simple paroxysm of ague, which seems to afford the instance most favorable to the idea of a critical diaphoresis, the sweating stage is sometimes entirely absent, and the febrile paroxysm goes off without it. We are convinced from our own experience in this disease, which has not been small, that there is no necessary relation of cause and effect between the three stages which constitute the ordinary paroxysm of ague. We have occasionally seen the cold stage without the hot or the sweating, and have much more frequently observed the gradual subsidence of the hot stage without any marked perspiration. We are persuaded that the notion entertained by Cullen, and others, of a fixed relation between the different stages of ague is entirely erroneous; and we may, indeed, ask, how is it possible that any such relation should exist, unless the cutaneous circulation bore, in every individual, the same relation to the other functions? Of two men in perfect health, one will run a mile without sweating, while another will be drenched with perspiration before he has run a third part of that distance. If, then, the natural movements of the circulation affect the functions of the skin so differently in different individuals, can it be expected that the effects of its morbid excitement will be more uniform?

Our author alludes also to the frequent occurrence of spontaneous perspiration without any relief, and sometimes with aggravation of disorder:

"Proofs of this may be drawn from common experience in continued fevers, pneumonia, acute rheumatism, and various other diseases attended with fever, where profuse or long-continued sweating often occurs, with doubtful benefit or even manifest disadvantage to the patient. The perspirations in hectic fever, though marking the several periods of remission, yet augment the distress of the patient, and increase his weakness. In the fever attending the epidemic influenzas, the more general state of the skin is that of a clammy perspiration, manifestly not producing any remission of the symptoms. Even in the exanthematous fevers, where it might be inferred from the eruptive symptoms that diaphoresis would be more uniformly beneficial, and a legitimate object of treatment, cases constantly happen where natural sweating is attended with no obvious good; and where the attempt to force it out by medicines or other means is distinctly injurious to the patient.

"Many other instances will occur, more or less directly in illustration of the same point. In those complaints usually termed bilious, and where there is accumulation, or disordered change of secretion, in the digestive organs, perspirations break out often and copiously, without any relief to the system. The same happens in various disorders of the alimentary canal; an effect of the intimate sympathy between this great internal membrane and the surface of the body. A tendency to perspiration is the consequence of purging in any excess; as indeed of all causes which, even without fever, tend to debilitate the body. These instances, though less determinate than those of simple and determinate fever, yet illustrate the same general view. If perspiration be regarded as an index of the change of symptoms, rather than as the cause of such change, it may be expected to occur in many cases, where it does not indicate any favorable alteration, and where in fact none such takes place." (pp. 54-5.)

Dr. Holland remarks that there must be at least two, and possibly more conditions, under which perspiration takes place: the one consisting in simple relaxation of the cutaneous vessels, the other in augmented activity of the same vessels, whereby the fluid is forcibly thrown out.* The former, being the reverse of that which obtains during the hot stage of fever, indicates an entire remission of the febrile state, while the latter is attended with little diminution of it.

He admits, also, that consideration is due both to the quantity and quality of the perspired matter, as ridding the blood of superfluous or noxious ingredients, a view which derives increased importance from those researches which show that a function analogous to respiration is carried on by the skin, and that the quantity of carbon eliminated from the blood may be considerably increased through this channel. The presumed influence of perspiration in reducing animal temperature must also be taken into account; but, as our author observes, the frequent continuance of morbid heat during excessive perspiration proves that there is no relation between the two symptoms which can be of any great value in practice. On the whole, Dr. Holland regards the use of diaphoretics as less determined by method or reasonable experience than that of other evacuant remedies, and lays down the following positions as established by sufficient evidence:

> "First, that it is more reasonable, as well as beneficial in practice, to have regard to the changes in the circulation producing diaphoresis than to the action of sweating itself. And, secondly, that the amount of perspiration is rarely a just measure of the good obtained; and that to make this a primary object is likely to give a wrong and injurious bias to the treatment of disease." (p. 62.)

Chapter v. is on the "Effects of Mental Attention on Bodily Organs." This is an ingenious essay on a very curious subject. Dr. Holland commences by observing, that while the influence of the will on the voluntary muscles, and of the passions of the mind on other parts of the economy, have long been studied, little notice has been taken of the effects of consciousness directed by a voluntary effort to particular parts of the body. This concentration of the attention on any one part may, he says, be exercised as a mental act, without the suggestion of previous sensation.

> "Though it may be termed a function of the will directed towards the body, it produces no effect on the muscular structure as such. Where indeed the attention is excited by external impressions, it is perhaps but another name for sensation itself; but we need a different term for that voluntary act by which the consciousness receives, as it were, a local direction, and is by effort retained for a time in this state,

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* Presuming that the greater part of diaphoretic medicines act by relaxing the cutaneous vessels, we have a good instance of the distinct nature of the two modes of perspiration in the horse. In this animal perspiration is easily excited by exercise: that is, by increasing the force of the circulation; but no medicine will produce a diaphoretic effect. We have frequently met with human subjects who perspired very readily by exercise or heat, but on whom sudorific medicines had little effect.—Rev.

> It is this enquiry which I do not find to have been explicitly made; though the familiarity of the effects, and of the language applied to them,—as well as the speculations regarding a vital principle, common to physiologists of every age,—may be said to have implied in reality all the points in question." (p. 64.)

The subject has not, indeed, been sufficiently investigated, but the general truth was distinctly present to the mind of John Hunter, who made also an application of it to animal magnetism similar to that which we shall presently find noticed by Dr. Holland.

In Mr. Parkinson's Hunterian Reminiscences we find the following words of this great physiologist, who had consented to subject himself to animal magnetism:

> "I feared lest my anxiety for the event should bring on my spasm, and that should be imputed to animal magnetism; but considering that if any person was affected by it, it must be by the imagination being worked up by attention to the part expected to be affected, and thinking I could counteract this, I went: and, accordingly, when I arrived at the place, I was convinced, by the apparatus, that everything was calculated to affect the imagination. When the magnetiser began his operations and informed me that I should feel it first at the roots of the nails of that hand nearest the apparatus, I fixed my attention on my great toe, where I was wishing to have a fit of the gout; and I am confident that *I can fix my attention to any part, until I feel a sensation in that part.* Whenever I found myself attending to his tricks, I fell to work with my great toe, working it about, &c., by which means I prevented its having any effect on me." (*Hunter.*)

Although the materials for observation on such points are very abundant, inasmuch as every man carries about a store of them in his own person, there are two causes which render it extremely difficult to arrive at any certain conclusions with respect to them: the one is the impossibility of fixing on any definite nomenclature of sensations; the other is the different relations which subsist in different individuals between the nervous system at large and the functions of particular organs. Still we believe that the principle which Dr. Holland seeks to establish has a firm foundation in truth; namely, that continued attention, or in more precise language, the continued direction of consciousness, to particular parts, has a power not only of altering their sensations, but of affecting their functions in a greater or less degree. He adduces a variety of examples in which sensations or actions may be excited by the simple direction of attention to the organs. Thus yawning, coughing, and sneezing, are occasionally excited in this manner. The organs of articulation and deglutition are variously subject to the same influence: a person who stammers will stammer more than usual if he wishes particularly to speak plain, and the act of swallowing is always rendered difficult by fixing the attention on it. A similar direction of consciousness to the region of the stomach causes a feeling of weight, oppression, or other uneasiness; and, when the stomach is full, seems greatly to interfere with digestion. The action of the lower bowels is often excited by attention to them, and a desire to empty the bladder is readily occasioned in the same manner. The last-mentioned phenomenon is very common in highly nervous persons, who, if they see the prospect of being confined for some length of time without an opportunity of retiring, will be seized all of a sudden with a strong inclination to empty the bladder; and we may notice, as a curious fact, that the kidneys often sympathize with the irritability of the bladder thus artificially induced, and secrete a large quantity of urine in a very short time.

"The power which this stimulated attention to particular parts of the body has in altering, not only the sensations derived from them, but also more or less their functional state, may be instanced in many other ways. The salivary glands, for example, are manifestly thus altered in their secretions; organs, it may be remarked, singularly susceptible of being affected instantly by all mental emotions. In the gums, the sensation created by attention given to them may arise almost into pain. The feelings produced in the tongue in like way are peculiar and well marked. Or a single limb, or portion even of a limb, may be taken for experiment; and a peculiar sense of weight and restlessness, approaching even to cramp, be produced by urging the attention expressly upon it. Here the muscular texture may be presumed to be chiefly affected; and with feelings much akin to those generally arising from fatigue, stagnant circulation, or other causes. Sensations of heat or cold, or other more vague feelings, on the surface of the body, may readily be created in similar way. In that state of skin, however produced, of which general itching is the symptom, the attention directed upon any particular part, will very often bring this sensation immediately to it. Such cases as these, where it is difficult to prove more than a change or increase of sensation from the parts under this influence, may appear ambiguous in proof. But that some real alteration is made in them, as respects either their nervous state, or circulation, or both, is probable from the more distinct evidence of this in other instances, from the same cause of excitement." (pp. 67-8.)

"All parts of this subject are curious, and deserving especial notice in their relation to the symptoms of disease. The case of the dyspeptic I have already mentioned. Closely akin to this is the disorder of the hypochondriac; some of the most singular perversions of which admit of the same explanation. Here the patient, in fixing his consciousness with morbid intentness on certain organs, creates not merely disordered sensations, but often also disordered actions in them. There may be palpitation of the heart, hurried or choked respiration, flatulence and other distress of stomach, irritation of the bladder; all arising from this morbid direction of attention to the organs in question. It is certain that many of the secretions are immediately affected by emotions of mind; and the case appears to be the same from anxious and sustained attention to the parts concerned in these functions.

"In chorea, hysteria, and other diseases where disordered nervous actions occur, the same principle is more or less concerned; but without equally distinct intervention of consciousness and the will, as in most of the instances already cited. Yet in hysteria, the instances are frequent of attacks brought on by the mere expectation of them; or by imitation; or *occasionally even by a sort of morbid solicitation of the organs to these singular actions.* Of the latter fact medical experience furnishes extraordinary examples. In diseases of this class, such results are connected with those more commonly recognized as produced by disorder of the sensorium upon the animal and vital organs." (pp. 69-70.)

The author does not hesitate to ascribe some of the results of animal magnetism to the effects induced, in different parts, by concentration of the attention upon them; and he remarks, that when the head or præcordia are the parts so influenced, especially in hysterical subjects, very singular, and apparently mysterious phenomena may result. The fact expressed by the words which we have placed in italics in the foregoing extract, is regarded by Dr. Holland as affording an explanation of some of the more singular incidents of the alleged magnetic state. To this we entirely assent. In a large proportion of the cases we have witnessed, the patients were manifestly impostors; in others, however, the phenomena appeared to be those of real hysteria, determined as to their particular character and locality by an eager expectation and desire of their occurrence. It may be added that, when they have once taken place, habit must exert a powerful influence in their reproduction.

How it comes to pass that the direction of consciousness to a part can thus create sensations and modify organic functions, is one of the most difficult problems in physiology. As Dr. Holland observes, there are several reasons which render it nearly certain that some nervous function is concerned in the production of these results. This being conceded, the question arises, through what class of nerves are such actions carried on? Those of voluntary motion (says our author,) can scarcely be admitted, because motion is no part of the effect, and the influence extends to parts over which we have little or no voluntary power: on the other hand, if we resort to the nerves of sensation, we must recognize two modes of action in opposite directions along the course of the same nerve, but of this we have no proof.

We agree with our author, that we possess no means of solving the difficulty; but, if we might venture on an hypothesis, we should suggest as possible that a *sensation* of the kind alluded to, though referred to a particular part, does not originate in that part, but is, in reality, an *internal sensation* arising exclusively in the sensorium from a repetition of that action whereby former sensations produced by external causes have been perceived. Of the reality of such a case we have ample proof in the sensations which are referred to the extremity of a limb months after it has been amputated, and frequently with the most minute reference to certain parts of it, as a finger or a toe. With respect to the *changes of function* under consideration, we should be inclined, on this hypothesis, to refer them to the agency of the organic nerves influenced by the long continuance of the cerebral actions above mentioned; nor do we see anything unlikely in this supposition, because there is little doubt that the ganglionic system, though its functions be distinct and peculiar, is very much under the influence of the brain. We have noticed this chapter at considerable length, because the subject of it has relation to many obscure points in the pathology of nervous diseases, and has moreover a direct bearing upon some remarkable phenomena which have lately attracted much attention, and given rise to no small exhibition of folly and credulity. These phenomena, now heterogeneously associated under the name of animal magnetism, will probably lead, in the hands of sober enquirers, to the discovery of a new and important tract of physiology. Pseudo-sciences, though they have no truth *in* them, have generally some truth *under* them.

Chapter vi. is on "Points where a Patient may judge for himself." It contains some sound precepts as to the licence to be conceded to patients with respect to their own choice of temperature, diet, posture, and confinement to bed or otherwise, the fitness of particular remedies, and the propriety of protracting or discontinuing the use of medicines. On all these points Dr. Holland believes that the practitioner may often derive knowledge from the expression of the patient's natural feelings and instincts, and concludes that "a discreet forbearance is as needful to the physician as firmness, and the best rule is not to be implicitly subservient to rule, a maxim essential in all cases to the right practice of medicine, seeing the many contingencies which are ever arising in contradiction to its soundest methods and precepts." We strongly recommend this chapter to the attention of routine practitioners, convinced as we are that the adoption of the advice it affords would prevent what very frequently happens, namely, that the patient, in addition to the perils of the disease, runs some risk of being bored to death by the doctor.

In chapter viii. on the "Abuse of Purgative Medicines," will be found some just animadversions on the too prevalent practice of giving constant purgatives, and insisting on daily evacuations, without reference to the natural constitution of the individual. The author remarks that a certain degree of distension of the large intestine is essential to its healthy state, and that the continual use of cathartics, by preventing this, and substituting a partial and unequal distension by air, produces irritation, and impedes the natural peristaltic actions.

He particularly reprobates the habitual employment of this class of medicines in cases of torpor of the bowels arising from debility.

> "The colon, perchance, cannot readily or quickly propel its contents, though the earlier stages of digestion are well and easily performed. To remedy this defect, it is goaded by the constant use of cathartics, which injuriously fret the stomach and long tract of bowels through which they have to pass before reaching this part. The habitual irritation of the mucous membrane alters and depraves its secretions throughout the whole course of the alimentary canal, becoming thereby a further source of mischief and suffering to the patient. These disordered secretions are too often urged in proof of the need of further evacuation, (an error sometimes arising from inexperience, sometimes from a graver source;) and thus the practice proceeds, in a vicious circle of habit, from which the patient is rarely extricated without more or less of injury to his future health." (pp. 99-100.)

Dr. Holland observes, that if we seek in such cases to obviate the evil through means which act only by irritation, we shall increase it: the proper plan is to combine tonics with laxatives. There are cases, he says, in which, if there be no irritation (inflammation?) of the mucous membrane, bark will often actas a laxative. The observation is perfectly correct. The author has several good remarks on the use as well as abuse of purgatives. Where this class of remedies is indicated, he advocates the exhibition of full doses, with proper intervals between, in preference to the more frequent repetition of smaller doses, excepting those cases where the vital power is too feeble to encounter any sudden change without risk. We believe the precept to be practically good. The following observation on the treatment of obstruction of the bowels is most just and well worthy of attention:

> "I do not mean that purgatives should not be given in cases of obstructed bowels; but I wish to convey the caution, required, as I think, by the too uniform direction of practice in these instances; viz. that if there be distinct local pain, threatening inflammation in any part of the canal, or much active irritation, with nausea and vomiting, and if the first cathartic medicines, freely given, fail of success,—it behoves the physician well to consider whether he shall urge this treatment further. There are doubtless cases where it is expedient to do so; but many others where the irritation of drastic purgatives hurries on the patient to danger or fatal result; and this is not unfrequently, where quiet; abstinence equally from food and medicine; leeches and fomentations over tender parts of the abdomen; or still better, in some cases, leeches to the hæmorrhoidal vessels, would have removed obstructions, and relieved the complaint." (pp. 104-5.)

We are quite convinced that many cases of fatal enteritis originate entirely in a practice opposite to that here recommended.

Dr. Holland observes, with truth, that the abstraction of blood from the hemorrhoidal veins is too much neglected, especially in this country. It is, in fact, the only form of general bloodletting in which a sufficient reason can be given for drawing blood from one vessel rather than another, inasmuch as the portal system is in some degree insulated from the general circulation, and is more immediately concerned in the functions of the chylopoietic viscera. The immense relief which every practitioner must have witnessed from a free hemorrhoidal discharge in various diseases of the abdomen, clearly indicates one of those instances, unhappily too few, in which a therapeutical intention may be directly founded on a natural remedial process. We think, however, that the study of natural therapeutics, as it may be called, is not sufficiently cultivated, and that careful observation and experiment might multiply such instances to a greater extent than is generally believed.

Chapter ix. is on "Methods of Prescription." It contains some judicious remarks of a general nature: these, however, we pass over, and proceed to notice the important subjects of "Gout and the use of Colchicum," with which chapter x. is occupied.

The author assumes, in limine, the following postulates as either ascertained or strongly to be presumed:

> "1. That there is some part of bodily organization disposing to gout, because it is an hereditary disorder. 2. That there is a *materies morbi*, whatever its nature, capable of accumulation in the system,—of change of place within the body,—and of removal from it. 3. That though identity be not hitherto proved, there is a presumable relation between the lithic acid, or its compounds, and the matter of gout; and a connexion through this with other forms of the calculous diathesis. 4. That the accumulation of this matter of the disease may be presumed to be in the blood; and its retrocession or change of place, when occurring, to be effected through the same medium. 5. That an attack of gout, so called, consists in, or tends to produce, the removal of this matter from the circulation; either by deposits in the parts affected; by the excretions; or in some other less obvious way, through the train of actions forming the paroxysm of the disorder. 6. That there is intimate relation between the condition of gouty habit, and the functions of the kidneys and liver, both in health and disease. 7. And that the same state of habit, or predisposition, which in some persons produces the outward attack of gout, does in others, and particularly in females, testify itself solely by disorders of internal parts, and especially of the digestive organs." (pp. 116-7.)

The second of these principles, regarding a material cause of gout circulating in the system, and eliminated from it by the gouty fit or in other ways, has been disputed by high authorities; but, as it is acquiesced in by many, is supported by all the facts and analogies furnished by recent enquiry, and affords a readier solution of the difficulties of the subject than any other that has been proposed, Dr. Holland makes it the basis of his reasoning.

Under this view, less importance is attached than formerly to the actual paroxysm of gout, since it comes to be regarded "as one only of a series of changes taking place within the system; though, perhaps, the most characteristic and interesting in its obvious effect of relieving the constitution for the time from the causes of the malady." Admitting a morbific matter as the efficient cause of gout, the question arises whether the hereditary tendency to this disease consist in a disposition to form or accumulate such matter by secretion or retention within the system, or in some peculiarity of texture in the solid parts, rendering them liable to inflammation of a specific kind, and occasioning the deposition of the peculiar matter when abounding in the system from other causes? The former of these opinions is espoused by Dr. Holland, who affirms that we have no reason to regard hereditary gout as more than "a disposition to generate a certain morbid matter within the body, in effect of certain circumstances of structure either favouring its formation or preventing that excretion of it from the system which is essential to a healthy state; or, in other words, gout as an hereditary disease may depend upon some transmitted peculiarities either in the organs of assimilation, or in those by which certain parts are separated from the mass of the blood."

We cannot say that we are inclined to so exclusively humoral a pathology of gout as our author, because, admitting as we fully do the presence of a morbific matter in the blood, the relations between this fluid and the textures which it supplies with the materials of life are so intimate, that we cannot conceive a morbid peculiarity of the blood to be transmitted from generation to generation, without inducing, as a necessary consequence, peculiarities of minute organization in all the textures. Yet, on the other hand, it is perfectly conceivable that other circumstances may so influence the organization of the solids as to render them more or less susceptible of influence from the morbific cause resident in the blood. We think, therefore, that a combination of the suppositions stated by Dr. Holland affords a better solution of the phenomena of gout than either of them taken separately, and especially of those relating to the exemption of certain individuals in gouty families.

Dr. Holland believes that the peculiarities which constitute the gouty diathesis may be generated in the individual independently of any hereditary taint. We would extend this admission to all diseases communicable from one individual to another, whether by consanguinity, contagion, or any other means. Those who deny this seem to forget that every disease must have originated in the person of some one or other, or it could not now exist; and if so, we must either admit that the disease may have a new origin in any other individual similarly circumstanced with the first, or that the first was placed in physical circumstances in which no human being could ever be placed again, a highly improbable, not to say absurd supposition. With respect to the actual nature of the morbid matter of gout, Dr. Holland thinks

> "It is probable that the discovery, if made, will show it to be,—not a matter alien to the system, and wholly morbid in kind,—but rather the excess, either from superabundant formation or undue retention in the blood, of some material, a certain amount of which is incompatible with, or even necessary to, the health of the body. Or this view may be modified in part, by supposing that, though generated in the body, it is so, only as an excretion needful to be removed, and hurtful in its retention or accumulation there. I have already alluded to what at present is the most plausible conjecture on the subject. Without venturing to antedate our future knowledge, by expressly defining the matter of gout to be either lithic acid, or urea, or one of the lithic or purpuric salts, or any other highly azotised principle, it is impossible not to suppose that there is produced in the blood some animal principle having close kindred with these, and morbid either in kind or by excess;—a matter

> in the separation of which the kidneys are largely concerned, and the retention of which in the system is the cause of various disorders, according to the age, sex, temperament, or other peculiarities of the persons affected." (pp. 128-9.)

He is of opinion that researches pursued in this direction are likely to lead to a more intimate knowledge of the disease in its active form, and also of its connexion with other local or constitutional disorders.

> "Modern observation has led us to recognize some of these relations under the names of gouty headach, gouty ophthalmia, and gouty bronchitis. My own experience would lead me to add certain forms of asthma to the number. But many more undoubtedly yet remain to be determined; and not the least important, those which subsist between gout and the system of the brain and nerves. Reference has already been made to hypochondriasis; and it is highly probable that other disorders of the same class, still less generally viewed under this connexion, will hereafter be submitted to it.
>
> "The relation of gout to the functions and disorders of the liver, is another point of much interest in pathology,—clearly attested both in the active symptoms of the disease, and by those which are common under other forms of the gouty temperament. Its connexion with cutaneous diseases is an additional topic, yet almost unexamined; though I cannot doubt, from my own observation, that certain of these disorders occur as effects of the habit in question. I have so often seen psoriasis, for example, prevailing in gouty families,—sometimes alternating with acute attacks of the disease, sometimes suspended by them, sometimes seeming to prevent them in individuals thus disposed,—that it is difficult not to assign the same morbid cause to these results, however unintelligible its mode of action under such different forms.
>
> "But the kidneys, as already stated, are evidently the organs of the body, upon the disordered or deficient action of which depend those changes in the circulating fluids, which have closest relation to all the phenomena of gout. These functions, it is important to observe, undergo variation at successive periods of life, independently of actual disease. By such variation they serve in part to the destined changes of the body at these respective periods; this influence being attested by an altered state of the secreted fluid, both in the nature and proportion of several of its ingredients. That period which begins the decline from perfect manhood is marked generally by an excess, if it may be so termed, of the lithic acid, which continues more or less through after life;—testifying itself with the greatest safety, and often remedially, by large habitual discharges of this substance from the kidneys; becoming a source of grave and various disease where this separation is insufficient or suddenly interrupted. Much certain discovery for the future (perhaps even as respects the causes and phenomena of fever) may be affirmed to lie in this particular path of physiology. And much more of practical caution might be drawn, even from our present knowledge, as to interference with these important functions, whether in health or in the treatment of disease." (pp. 129-130.)

Some other points of considerable interest are adverted to by Dr. Holland, which lack of space prevents our noticing. He subjoins to this essay some remarks on the use of colchicum. And first he enquires, does its operation consist in destroying the matter of gout by some specific change, or in withdrawing it from the part affected into the general circulation, or in removing it from the system through some of the excretory organs? These, he observes, seem to include all the modes in which it can be supposed to act, except the improbable one of its exclusive influence on the nervous system. Experience affords ample proof of the efficacy of colchicum in every modification of the disease, in whatsoever texture it may be situated; and we are hence naturally led to infer its action on the morbific matter diffused throughout the system: but this inference by no means implies any chemical or other change immediately effected on the matter itself; it is more probable that the medicine exerts its specific power on some organ the function of which is expressly connected with the morbid conditions of gout. He thinks that colchicum acts on the kidneys more decidedly than on any other part, and this independently of the presence of any gouty disease or disposition. On the nature of the changes induced by it in the urinary secretion he has not been able to satisfy himself, though he is convinced that its operation is not confined to a mere increase of quantity, but involves a change in the nature or proportion of the animal compounds excreted. This idea accords well with the pathological views already stated, with the presumed nature of the morbific matter, and with the acknowledged affinity of the gouty and calculous diatheses.

Such a view, if admitted, affords some important practical suggestions as to the administration of colchicum, and especially as to its use as a preventive. Dr. Holland has no belief in the opinion that this medicine renders the recurrence of the paroxysm more frequent. This opinion he believes to have arisen from mistaking the effects of the disease for those of the remedy, and from the faulty or insufficient application of the latter; and he attributes such misapplication to the too exclusive attention given to the external development of the disease. The medicine has been used empirically to remove the local inflammation, and discontinued when this was effected; whereas it admits of continued use on the definite principle of eliminating the morbific matter from the system: to attain which end it should be conjoined with means adapted to sustain all the excretions in a sufficient degree; this being the best guarantee that the matter is fairly carried out of the circulation.

We regret that we cannot follow out our author's observations on this subject; but we have stated above the principle which pervades them: we think that it well deserves to be kept in view, and made an object of enquiry both by the animal chemist and the physician.

Chap. xi. is on "some supposed Diseases of the Spine." Some good remarks will here be found on a class of cases in which disordered function of the spinal nerves is liable to be mistaken for organic disease of the spine. These cases are most frequent in females, and especially those of an hysterical habit. There is no doubt that a misunderstanding of their true nature formerly led, and does frequently still lead to much injurious practice in the way of confinement to the recumbent posture, local depletion, and severe counter-irritation, all of which tend, in the most direct manner, to aggravate the disorder. The author does not lay claim to any originality in his view of the subject, and we shall not enter into it further than to express our satisfaction at one of the curative measures he enjoins, namely, "exercise of the limbs sedulously persevered in and extended." We believe that a frequent effect of that bodily indolence which is one of the greatest evils of civilization, and which obtains particularly in the female sex, is to exalt the functions of the sensatory nerves at the expense of the motor tracts of the spinal cord, the functions of the latter being enfeebled by not being called sufficiently into action. At all events we can speak from manifold experience to the practical truth that properly regulated muscular exercise is the most powerful remedy in some cases of spinal irritation.

There are several chapters in this volume which treat respectively "of the Brain as a Double Organ," "on Dreaming, Insanity, Intoxication," on "Sleep," on "Time as an Element in Mental Functions," on "Phrenology," and on the "Present State of Enquiry into the Nervous System." These essays are so connected in their subjects that it would be difficult to insulate one or more of them for particular examination. We omit all detailed notice of them at present: first, because the purely physiological part of them will come under review on an early occasion along with the works of some other authors; secondly, because many of the topics they involve belong as much to psychology as to medicine; and the practical character of this Journal obliges us to exclude such subjects, except on particular occasions. We would not, however, for a moment be thought to depreciate abstract science as a study for the physician: on the contrary, we sincerely regret that the greater number of medical writers evince so little familiarity with it, and there is nothing which we admire more, in these essays of Dr. Holland's, than the evidences they afford of an assiduous addiction to mental philosophy.

Chap. xiii. is on "some Points in the Pathology of the Colon." Our author here offers the following suggestions: First, that the large intestine is more an organ of secretion than of absorption, and that its secretions are not merely subservient to changes in the matter passing through it, but useful or necessary in removing other excrementitious matters from the system: whence some important considerations as to the treatment of certain forms of diarrhœa, such as whether the diarrhœa should be checked or allowed to proceed, and whether practice should be directed to the gradual alteration of the secretions or the immediate removal of noxious matters. Secondly, that many morbid states of the alimentary canal which are referred to the stomach or liver have their real seat in the colon—a fact little noticed in books, but of which we conceive no practitioner endowed with any observation can have the smallest doubt. Thirdly, that, owing to the attachments of this portion of intestine and its immediate proximity to several important organs, its unequal distension and frequent changes of position give rise to various sympathetic affections, the nature of which is frequently misunderstood. The most important remarks are those on the morbid secretions of the large intestine. This subject, however, has been too little investigated to afford data for any critical examination of Dr. Holland's views, which, indeed, are advanced only in a very general manner. On the whole, we regard this chapter as well worthy of attention, and indicative of several points on which there is need of enquiry.

Chapters vii. and xiv. treat on the "Connexion of certain Diseases," and on the "Epidemic Influenzas of late years." We notice these chapters together because they have a common tendency.

Dr. Holland's observation has led him to trace a singular concurrence of measles, scarlatina, hooping-cough, and infantile remittent fever, in particular districts about the same period of time; and he has noted also, as a variation of this fact, that one of these disorders is often exceedingly prevalent in certain localities, while another is equally so at the same time in contiguous places. He alludes to the familiar fact that scarlatina and measles have an affinity which sometimes renders them difficult of discrimination, and further remarks that these diseases have been very frequent during the course of influenzas, at which times a class of cases of a singular and ambiguous character present themselves, having many appearances analogous to each of the exanthematous disorders above mentioned, but chiefly, as he thinks, to scarlatina. He notices the tendency of erysipelas and puerperal fever to assume an epidemic form in the same seasons, and the increased frequency of both these disorders during the prevalence of influenza. He observes that hooping-cough and infantile remittent fever are both remarkably coincident with influenza; that hooping-cough rarely prevails as an epidemic without a simultaneous prevalence of those bowel affections attended with fever, which, in their more distinct forms obtain the name of infantile fever; and that ulcerations or eruptions of the mouth, fauces, nose, lips, and face, are very frequent, especially among children at those periods when disorders of the bowels, whatever be their cause, are particularly prevalent and severe. The dysentery of adults, he continues, has various relations to infantile fever in the course of the symptoms, in the textures affected, and in the lesions consequent upon it; and perhaps there may be no differences between the diseases beyond what are compatible with a common cause acting under different conditions. He extends this remark to the suggestion that the disorders of childhood already mentioned may have a virtual identity with certain fevers or other diseases of adult age, the operation of the causes varying under altered circumstances. Is this view quite in unison with an opinion expressed in chapter xxiv. on "Diseases commonly occurring but once in Life?" It is there maintained as probable that the series of actions or changes constituting such diseases have their seat in the blood, and are carried on throughout by the circulation, and that the future insusceptibility arises from a change in the state of the blood. The first position—that the essence of these diseases consists in a virus acting on the blood—can hardly be called in question; but if the diseases reappear with a new face in the adult, it follows that the blood must, in some sort, retain its susceptibility of the action of the virus; and we should be rather led to ascribe the difference of its effects to an altered constitution of the solids. It is scarcely fair, however, to argue on what is thrown out only in the form of suggestion, or to insist on too exact a congruity between ideas which, in the present state of our knowledge, can at best be but dimly defined. Dr. Holland observes that dysenteric symptoms are generally prevalent along with the diseases of childhood just alluded to. He has also found that dysentery and other bowel affections become much more frequent on the decline of catarrhal epidemies. It is doubtful whether this should be regarded as a translation of morbid action from the same original cause, or merely as an incidental effect to which the system becomes liable from the previous disorder: Dr. Holland inclines to the former supposition. He finally comments on the analogy which influenza bears to the milder forms of typhus and to intermittent fever. The intermittent character of the epidemic catarrhal fever is strongly insisted on, and is supported by the testimony of Sir George Baker, who describes the same type of fever accompanying the influenza of 1762. It will easily be perceived that Dr. Holland's aim is to establish some real relation, however indefinite, among a number of different diseases, and to bring them all into apposition with influenza, in the hope of deriving from the contemplation of the last-mentioned epidemy some principles of general application and utility.

"In referring thus frequently to these epidemic influenzas, which have so frequently and widely prevailed of late years, I may add, that no class of diseases lays open to us a larger field of practical enquiry, as relates not only to their own nature, but also to the connexion of other diseases, with which they are closely associated. The simultaneous or rapidly successive influence of a common morbid cause over large communities and countries discloses relations which in no other way are equally accessible to research. In showing the various forms which a single disease is capable of assuming, it illustrates the nature and action of the circumstances which thus modify it, and especially the effect of particular textures in altering the aspect of the symptoms. We have not sufficiently drawn from this source of knowledge. It is probable that we may hereafter learn from it the virtual identity of many diseases; hitherto placed asunder by distinctions which have foundation only in subordinate symptoms, thereby disguising from us what is most important both in pathology and practice. Or, if no such identity be proved, we may find evidence, scarcely less curious, of an endemic state of constitution (be it called adynamic, or by any other name,) which, originating with the same causes that produce the symptoms of influenza, renders the body for a period more prone than usual to certain other disorders, the material causes of which are ever more or less present. Each mode of viewing the subject is probably correct in part, and they are perfectly compatible with one another." (pp. 92-3.)

The idea is good, and far be it from us to damp the ardour of enquiry; but the perusal of the chapter on influenza is sufficient to show that the benefits to special pathology from the study of this widely-spread disease are yet in reversion; for our author's remarks as to its origin and nature consist chiefly in a statement of difficulties, conjoined, however, with a judicious exposition of the course of enquiry most likely to lead to their solution. His precepts as to the treatment are, we think, in general sanctioned by the experience of the best practitioners. One of them, relating to the use of the sulphate of quina, is founded on the prevalence of an intermittent type in this disease which appears to have presented itself more distinctly to Dr. Holland than to many other observers, and more so, we confess, than to ourselves.

"Any inference that might be drawn, as to its use, from the tendency in the disorder to these intermittent actions, is fully justified by the actual effects. It relieves them, when fully established, almost as speedily and certainly as the attacks of a common ague; and this whatever the part of the body so affected. This remarkable power over one of the conditions of the disease gives so far a specific character to the remedy, that it may rightly be adopted in prevention of a state which it is capable of curing. It is not easy to define an exact time at which its use should be begun. This must vary in different temperaments, and in different degrees of the disorders. But it is an inference from the reasons already stated, that quinine may be given safely and beneficially in many cases where there is still hard cough, with pain, oppression, and restlessness;—and experience confirms this conclusion. A soft feeble pulse, and moist skin, often concur with these symptoms, and furnish additional authority for the practice. If the cough itself, as frequently happens, tends to intermittent character, the security of the remedy becomes greater, and its effects more speedy." (p. 211.)

Chapter xviii. contains some brief but just comments on the "Method of Enquiry as to Contagion." The author refers, for a full exposition of the laws of contagion, to a paper by the late Dr. Henry in the Fourth Report of the British Association. A review of it will be found in the Second Number of this Journal.

Chapter xix. is on the "Medical Treatment of Old Age." The subject is introduced with some appropriate physiological remarks, in which, however, the author leans more than we are disposed to do towards the belief that merely physical laws enter largely into the arcana of life. We think the phenomena of old age afford the strongest case in favour of an opposite opinion. As age advances physical laws assert a gradually increasing power in the organism: why? Because the vital energy by which they are controlled is becoming progressively more feeble. Death resigns all to the domain of physics.

Some of the best practical observations in this chapter have reference to those habitually increased excretions, common in old age, which our author regards as depurating rather than morbid processes; and hence inculcates caution in interfering with them. He instances particularly the augmented discharge of mucus in the bronchial affections of old age, and the increased excretion of lithic acid with the urine. He notices, also, as of less frequent occurrence, but still worthy to be kept in mind, under the same point of view, the passive hemorrhages incidental to old age: epistaxis, hematuria, hemorrhages from the bowels, uterus, &c.

The consideration of these subjects carries us back into the Greek era of medicine, which we are convinced is destined to diffuse much light over the future annals of the science.

There is a remark concerning the pulse, which we notice because the fact to which it refers, though very obvious, is too often overlooked in practice. We allude to the jarring stroke of the pulse caused by organic affections of the heart, so common in old age, and which is liable to be mistaken for an indication of strength. This applies particularly to the treatment of apoplexy—a disorder of advanced life, and one which is very frequently indeed associated with disease of the heart. It has occurred to us more than once to see a practitioner fully prepared to take away thirty or forty ounces of blood, in a case of apoplexy, but astonished to find the pulse sink to a thread before ten ounces had been withdrawn. Dr. Holland expresses his belief, in a note, that there is no important structural disease of the heart which is not represented in some manner by the pulse, as felt in a vessel of the size of the radial artery; an opinion strikingly corroborated by the views and doctrines of Dr. Hope, as detailed in a preceding article. Whatever doubts may be entertained of the possibility of such minute discrimination, the general principle, that the pulse should always be considered in two points of view—as an index to the state of the heart namely, as well as that of the system—is, we conceive, of great practical importance.

This chapter contains good general precepts, both for the hygeienic and therapeutical treatment of old age, which might suggest materials for a complete treatise on a subject too little attended to.

There are several chapters on the application of particular classes of medicines—as emetics, opiates, digitalis, and antimonial medicines. We have already given a specimen of our author's manner of treating such subjects in our notice of the chapters on diaphoretics and purgatives. Those above enumerated we are obliged to pass by; but there is yet one chapter on the "Uses of Diluents," on which we must pause for a moment, if it were only to solicit attention to a class of remedies as much neglected by British practitioners as they are rendered unduly prominent by our continental brethren. Dr. Holland views their operation under three principal conditions: First, as mechanically diluting and washing away excrementitious or noxious matters from the alimentary canal; secondly, as modifying certain morbid conditions of the blood; and, thirdly, as affecting various functions of secretion and excretion, especially those of the kidneys and skin. We recommend his remarks on all these heads to the attention of the practitioner, and especially one included under the first, though not strictly referrible to it, namely, the internal use of cold water as a local means of refrigeration. He very truly observes, that the abstraction of heat from an inflamed or irritable internal membrane is often quite as salutary as the direct application of cold to a hot and dry skin, although the former use of the remedial agent is, for obvious reasons, more limited. He adds, "I have seen enough of the benefit from cold liquids freely given in the acute stage of gastric disorders, inflammatory and febrile, with express reference to this point of temperature, to justify the recommendation of more frequent recourse to it in practice." We are happy to find so good authority in support of a practice suggested by common sense, and of the utility of which our own experience has amply convinced us.

Chap. xxii. is on "Morbid Actions of intermittent kind."

As an article in our last Number was occupied with the consideration of intermittent fevers, we may be excused from entering into detail on the subject of this chapter. The observations of the author also are directed chiefly to the theoretical part of it, which, from the absence of sufficient data, is of too vague a character to demand our special attention. The general tendency of Dr. Holland's remarks—which extend to the intermittent disorders of sensation and voluntary motion, as well as to febrile affections of the same type—is to connect, and refer to some common law of periodicity, the various healthy and morbid actions of the system which have a disposition to recur at regular intervals. This is evidently the right track of enquiry; but it is perhaps the most obscure in the whole range of medical science. Not a ray of light crosses it; and the only encouragement to attempt its exploration is the probability that a glimpse of the truth, on any one part of the enquiry, might soon enable us to unveil the whole mystery.

Why is the term of existence so rigidly prescribed for all living beings, and yet so variously for different species? Why are infancy, youth, mature age, and senilitude, so definite in their periods? Why is the term of uterine gestation fixed for every species of animal? Why do the catamenia occur monthly in the human female? Why does an ague or a megrim return at certain intervals? When we can answer any one of these questions unequivocally, we shall most likely have a clue to them all.

It is matter of regret to us that we are unable to afford even the most cursory consideration to a long and very excellent chapter on "the Influence of Weather in Relation to Disease." The author directs his attention to the subject of *weather simply*, as determined by the temperature, hygrometric state, weight, and electrical conditions of the atmosphere, exclusively of chemical change in the air itself, or the admixture of other gases, or of animal or vegetable miasmata. Dr. Holland here evinces much physical knowledge and acute observation; but the subject is vast, and treated only in outline, so that any attempt to condense this chapter would be unprofitable.

Four other chapters on the "Exercise of Respiration," on "Diet and Disorders of the Digestive Organs," on the "present Questions regarding Vaccination," and on "Disturbed Balance of Circulation and Metastasis of Disease," we must also refrain from commenting on. There is another chapter on the "Hypothesis of Insect Life as a cause of Disease," which we must content ourselves with recommending to those of our readers who wish to know all that can be said upon the subject. Dr. Holland states the argument in favour of the insect origin and propagation of pestilential diseases, and especially cholera, as lucidly as could be wished by the strongest advocate of that hypothesis, to which, however, our author by no means professes himself a convert. We must acknowledge that we scarcely consider this hypothesis worthy of grave consideration. If cholera, or similar epidemies, be extended by the migration of insect swarms, these insects must be susceptible of life and activity in all localities, seasons, climates, and temperatures. Now this would be an anomaly so extraordinary, and so opposite to everything hitherto known of the external conditions of animal life, that to adduce such a supposition for the solution of the question as to the spread of epidemies appears to us simply to be attempting to solve what is difficult by what is impossible.

But we must now take leave of our author, which we do with many acknowledgments of the pleasure and instruction we have derived from his work. It is pregnant with information and with thought; and, as such, we heartily recommend it to our readers.

Art. XIV.

On the Physiological Inferences to be deduced from the Structure of the Nervous System in the Invertebrated Classes of Animals. By William B. Carpenter, m.d., m.r.c.s., &c.—*Edinburgh*, 1839. 8vo, pp. 83. (*With Two Plates.*)

We are induced to bestow an early notice upon this essay, because we have reason to know that it contains the results of enquiries upon which its author has been for some time engaged, and the publication of which has been delayed by his wish to embody them in the inaugural dissertation required at the time of graduation. To those who have looked with any degree of interest at the recent discussions upon the functions of the nervous system, we recommend the perusal of this pamphlet, as containing a summary of important facts, which have not before been thus compared and examined, and a series of inferences from them which have an immediate and, to our minds, decisive bearing upon the chief questions at issue. To our other readers it may be adduced as an example of the important aid which comparative anatomy is capable of affording to those who aim at penetrating the mysteries of physiological science; since an intricate question appears here to be easily decided by an appeal to the structure of the inferior classes of animals, which it would be difficult, and perhaps impossible to determine upon any evidence furnished by the study of man alone. We have only further to add, with regard to its general merits, that this essay has been honoured by one of the four prize-medals now annually given by the University of Edinburgh. This fact sufficiently stamps its value as a *thesis*, but is not enough to characterize it as it deserves. The readers of this Journal will understand the reserve with which we speak on this subject; and will not, we hope, misconstrue the delicacy which restrains us from giving more than a dry analysis of a production which, however prized by physiologists as an important addition to science, will, we think, be regarded by them yet more as a further earnest of what physiologists may expect from one who, almost in his pupilage, has rivalled, if not outstripped, the most learned of his contemporaries.

As many of our readers, however, may desire to know the general results of our author's enquiry, and the nature of the evidence upon which they are founded, without following him through the details of his argument, we shall present them with such a brief analysis of this essay as may serve the purpose. The opinion which it appears to be his object to establish is, that the centres of simple reflex action, and the nervous filaments which serve as its channels, are generally among the invertebrata structurally distinct from those which minister to sensation and voluntary motion. Some traces of such a view, in reference to the articulated classes, at least, may be found in our former pages (Vol. V. p. 500-1); but these could only be regarded as speculative, until confirmed or disproved by more extended enquiry. It has been by directing his attention to the anatomy of the nervous system in the mollusca, and by comparing it with the structure of the same apparatus in the articulata, that the author has succeeded in giving a high degree of probability to his doctrine, if not in fully establishing it. Those cases are among the most interesting in the history of science, in which a theory, erected upon the comparison of a limited number of instances, is found to be not only *applicable* to an entirely new group of phenomena, but to meet with *independent* support from them: and it is almost superfluous to observe upon the vastly-increased probability of its truth when such is the case.

The first two sections, which treat of the supposed nervous system of plants, and of the nervous system of the radiata, present no peculiar novelty, in fact or opinion, to the readers of this journal. We shall therefore pass on at once to the consideration of the leading characters of the nervous system of the mollusca. The chief of these is the smallness of the number of the ganglionic centres, and the distinctness in the function of each. Except in the highest group, the cephalopoda, in which there is a considerable tendency to approach the vertebrata, we rarely find more than four pairs of ganglia, and very frequently but three, or even two. Those which commonly form pairs often coalesce into a single one; and to such an extent is this reduction carried in some instances, that in one large group, the tunicata, we have but a single ganglion, and this is obviously connected chiefly with the respiratory apparatus. We must not suppose that there is here a *concentration* of function, such as we perceive in the central masses of vertebrata; the fact being, on the contrary, that the functions themselves are absent; so that, as the author has observed, nature has here anticipated the experiment of the physiologist, in showing that life may be continued with the integrity of that portion only of the nervous system which is concerned in the respiratory function. It is in the gasteropoda, the typical mollusca, that the peculiarities of the nervous system of the group may be best studied. Here we usually find one pair of ganglia situated above the œsophagus, and connected with the organs of special sensation. These, the *cephalic* ganglia, evidently present the nearest approach to the brain of vertebrata. The situation of the others is more variable. We usually find a single ganglion, or a pair, in connexion with the branchiæ; another with the foot; and sometimes another with the mantle. The distribution of their nerves to the different organs would alone indicate their respective functions; but these are placed beyond doubt by that very great variety in the disposition of these organs which is characteristic of the mollusca. The development of the sensory organs, the situation of the gills, the structure and position of the foot, and the conformation of the mantle, are well known to differ in the most obvious manner, in genera which are most closely allied to each other. Hence we are able, by the discovery of corresponding changes in the arrangement of the nervous system, to satisfy ourselves of the peculiar functions of its different centres.

These points being established, we are led to enquire into the influence of these centres upon one another, and we then perceive the important fact, that, while they have little or no communication with each other, they are all directly connected with the cephalic ganglia, which seem thus to harmonize and control their individual actions. Frequently, however, a communication seems to exist where there is really none,—a cord from one ganglion passing through another in its way to the head; but in such a case, we find a distinct communicating cord belonging to this second ganglion; and there are even instances in which three such cords are found, two of which have proceeded from ganglia of different functions *fused* into one mass, while the third has passed through this in its way from a distant centre. These facts have been previously noticed by anatomists; but no explanation of them has been given. Further, a careful examination of these ganglia and their connecting cords discloses this important fact, which is peculiarly evident in the case of the *pedal* ganglia,—that the cord does not lose itself in the gray matter of the ganglion, but divides itself into filaments which mix with those proceeding from it, to form the nervous trunks which it distributes. We can scarcely, then, fail to infer that the pedal ganglion, with the nervous fibrils proceeding from it, is the source of the reflex actions of this organ, whilst the filaments, which are continuous with those of the connecting trunk, and which thus proceed from the gray matter of the cephalic ganglia, are the channels of sensory impressions, and of the motor impulses of volition. Those who desire to become acquainted with the detailed evidence upon which this novel position is based must consult the essay; we have here only space for a curious example of its application. The arms of the cuttle-fish are provided, as is well known, with a series of suckers, which are important instruments of locomotion and prehension to this animal. It has been observed by Dr. Sharpey that the nerves which supply these arms are provided with ganglionic enlargements, of which one corresponds with each sucker; and that, like the ventral cord of the articulata, they are composed of two tracts, of which one passes continuously over the ganglia without entering them, but sends off nervous filaments, which help to form the branch going to each sucker. It has been supposed that the white tract is the motor portion; and the ganglionic has been thought to be the sensory; but Dr., Carpenter has shown that we may with much more probability regard the white tract as conveying the influence of the will to all the suckers alike, whilst the ganglion in connexion with each ministers to those reflex actions which it will exhibit even when separated from the rest of the arm.

The idea that there is any analogy between the ganglia of the sympathetic nerve, or those on the posterior roots of the spinal nerve (which the recent investigations of Remak have shown to be part of the sympathetic system,) and the ganglionic centres of the invertebrata, is, we think, very satisfactorily combated in this essay. It is plainly shown that the latter are analogous to certain portions of the cerebro-spinal system, both in their structure, connexions, and functions. Any inferences founded upon this fallacious analogy must therefore be invalid; and such we cannot but deem that which has attributed sensory functions to the ganlionic portion of the ventral cord of the articulata, and exclusively motor functions to its white or fibrous tract. This cord is evidently formed by *a repetition of the pedal ganglia* of the mollusca; and every pair of nerves given off from it is manifestly derived in part from its own ganglion, and in part from the cephalic ganglia, just as in that class. An easy explanation is afforded by this hypothesis, of the independence of the segments so remarkable in the articulata, and of the number and variety of the reflex movements which they will perform when isolated; and its claims to reception, as physiological truth, are evidently increased to no trifling amount by its conformity, not only with the analogy of the mollusca, as already stated, but with that supplied by vertebrata, which we shall give in the author's words:

> "In the vertebrata we find the ganglionic or mixed portion of the spinal cord, and the simply fibrous tracts, performing functions respectively analogous; for, when any segment is isolated from the rest, reflex actions may be excited through it, in the production of which the white columns can scarcely participate, being structurally distinct from each other and from the ganglionic portion of the cord, and continuous only with the fibrous portion of the brain; whilst pathology supplies us with instances of the converse occurrence, namely, the destruction of the ganglionic portion by disease, without the functions of the parts below being impaired—*their* ganglionic portion being segmentally independent, and their communication with the brain being maintained by a continuity of white or fibrous structure." (pp. 61-2.)

We have not here space to follow our author through another very interesting branch of his enquiry,—that which relates to the *stomatogastric* system of nerves in the invertebrata. Here also may be seen the advantage of not studying one class of animals to the exclusion of another; for here again the functions of certain problematical nerves in insects receive important elucidation by comparison of them with analogous parts in the mollusca. The general result of this comparison is a very interesting one,—that in the lower tribes, the system of nerves concerned in manducation and deglutition is as distinct from that which may be designated as the sensori-volitional system as is that of respiration; whilst in vertebrata, these different centres are united, and placed in more immediate relation to each other—the centre of the stomato-gastric system, as well as that of the respiratory, being in the medulla oblongata.

The following is the recapitulation furnished by our author of the chief results which he considers to have been established or rendered probable by the facts which he has adduced. We quote them without further remark at the present time, as meriting our general assent, although several points in them lie open to discussion.

"A general review of the ground over which we have passed will enable us, we think, to draw the following conclusions with a high degree of probability:

"1. That a nervous system, in the form of connected filaments with ganglia on certain parts of them, exists in all animals, (that is, in all beings endowed with any degree of sensibility and voluntary power,) although its presence may not be detected by our means of observation.

"2. That the actions most universally performed by a nervous system are those connected with the introduction of food into the digestive cavity.

"3. That we have reason to regard this class of actions as everywhere independent of volition, and perhaps also of sensation;—the propulsion of food along the œsophagus in man being of this character.

"4. That, for the performance of any action of this nature, a nervous circle is requisite, consisting of an *afferent* nerve on the peripheral extremities of which an impression is made; a ganglionic centre, where the white fibres of which that nerve consists terminate in gray matter, and those of the efferent nerve originate in like manner; and an *efferent* trunk conducting to the contractile structure the motor impulse, which originates in some change in the relation between the gray and white matter.

5. That such actions may be regarded as the simplest of those which the nervous system performs, and most resemble the examples of contraction produced by the irritation of distant organs in plants (where an *impression* is mechanically conveyed by the circulating system,) of any which the animal kingdom affords.

"6. That in the lowest animals such actions constitute nearly the entire function of the nervous system; the amount of those involving sensation and volition being very small.

"7. That, as we ascend the scale, the evidence of the participation of true sensation in the actions necessary for the acquirement of food, as shown by the development of special sensory organs, is much greater; but that the movements *immediately* concerned with the introduction of food into the stomach remain under the control of a separate system of nerves and ganglia, to the action of which the influence of the cephalic ganglia,—the *special*, if not the *only*, seat of sensibility and volition,—is not essential.

"8. That, in like manner, the active movements of respiration are controlled by a separate system of nerves and ganglia, and are not dependent upon that of sensation and volition, though capable of being influenced by it.

"9. That the centres of these systems are brought into closer structural relation with that of the sensori-volitional system as we ascend the scale of invertebrated animals; until they at last apparently become a part of it, as in Vertebrata, where, however, they still remain really separate, and may be artificially insulated.

"10. That, whilst the actions of these systems are in the lower tribes almost entirely of a simply reflex character, we find them, as we ascend, gradually becoming subordinated to the will; and that this is effected by the mixture of fibres proceeding directly from the cephalic ganglia with those arising from their own centres.

"11. That the locomotive organs, in like manner, have their own centres of reflex action, which are independent of the influence of volition, perhaps also of sensation.

"12. That the influence of the will is conveyed to them by separate nervous fibres, proceeding from the cephalic ganglia; and that similar fibres probably convey to the cephalic ganglia the impressions destined to produce sensations.

"13. That the stomato-gastric, respiratory, and locomotive centres are all united in the spinal cord of vertebrata, where they form one continuous ganglionic mass; and that the nerves connected with all these also receive fibres derived immediately from the cephalic ganglia.

"14. That whenever peculiar consentaneousness of action is required between different organs, their ganglionic centres are united more or less closely; and that the trunks themselves are generally connected by bands of communication.

"15. That the sympathetic system does not exist in the lowest classes in a distinct form; that the nervous system of the invertebrata, taken as a whole, bears no analogy with it; that, as the divisions of this become more specialized, some appearance of a separate sympathetic presents itself—but that this is never so distinct as in vertebrata.

"16. Hence it may be inferred that, as the sympathetic system is *not* developed in proportion to the predominant activity of the functions of organic life, but in proportion to the development of the higher division of the nervous system, its office is not to "preside over" the former, but to bring them into relation with the latter; so that the actions of the organs of vegetative life are not dependent upon it, but influenced by it in accordance with the operations of the system of animal life." (pp. 76-9.)

We must not conclude this notice without a few words in reference to Dr. Marshall Hall and his opinions. It will be recollected that, on a former occasion, we withheld our assent from that portion of his doctrines which concerned the *structural distinctness* of his *excito-motor* from the *sensori-volitional* system. We by no means asserted that it might not prove to be well founded, but simply that no sufficient evidence was adduced by him in its behalf. On the latter point we see no reason to change our opinion. A new field of enquiry has been explored by one who was originally adverse to this part of Dr. Hall's system; and he has obtained evidence of its truth, which ought to be gratifying to that gentleman, and which will not, we hope, pass unnoticed by him. "A wise man," it has been well said, "changes his mind as often as he sees occasion for it." We do not pretend to infallibility; and we see no discredit in giving our assent to opinions which we formerly hesitated in receiving, when new evidence is presented in their support. As to Dr. Hall's *entire originality* in this part of his doctrines, we never expressed the slightest doubt; and we are now quite willing to accord to him the credit of having made a most important advance in nervous physiology, not only (as we formerly stated) by "the harmonious combination" of previously-entertained doctrines "into a uniform system, and the application of it to the explanation of many phenomena which were not formerly regarded as explicable on such principles," but by the discovery of a previously-unsuspected fact, which, when placed entirely beyond doubt, will take the rank of those established by Sir C. Bell.

ART. XV.

1. *Selections in Pathology and Surgery; or, an Exposition of the Nature and Treatment of Local Disease, exhibiting new pathological views, and pointing out an important practical improvement; illustrated by Cases.* By JOHN DAVIES, Surgeon to the General Infirmary at Hertford.—*London*, 1839. 8vo, pp. 128.
2. *Practical Remarks on the use of Iodine locally applied in various Surgical Diseases and External Injuries; illustrated by Cases.* By JOHN DAVIES, &c. &c.—*London*, 1839. 8vo, pp. 62.

THE two small treatises which stand at the head of the present article constitute properly but one volume, the first mentioned containing the second. This last the author has published in a separate form, being desirous, he says, to make known the curative properties of iodine as extensively as possible amongst the members of the profession. We shall, in like manner, divide our remarks under two heads: first, considering the pathological views which are set forth; and secondly, the practical application of the remedy in question in various forms of disease.

Were we to be critical, we might fairly object to the title given to the first portion of this work, as conveying an idea of a variety of morbid states being therein discussed; whereas the whole is limited to an explanation of some of the phenomena of inflammation. In reviewing the more voluminous writings of Hunter and others, in our last two Numbers, we took occasion to enter so fully on this subject, in order to show the variety of opinions held respecting this most important of all diseases, that it will only be requisite for us to allude briefly to the views entertained by the author of the present pamphlet. Indeed, we do not hesitate to state our opinion, that no writings of the present description can enhance our knowledge of the pathology of inflammation; that we may go on reasoning and reasoning only, *ad infinitum*, without advancing one single step in the enquiry; and that, until some other physiologist steps into the field, prepared by his experimental researches to confirm what is known, or else to place the matter in an entirely new light, the subject, as far as words can determine it, may, for the present, be considered as exhausted. The microscope, we conceive, affords us the only means, at present, of investigating the true nature of inflammation. Nothing satisfactory can be elicited by reasoning unsupported by experiments; and next to nothing can be gained by considering the symptoms during life, or examining the alteration of structure left after death. Previous to carrying on, however, an investigation of this description, it is essential that the eye should be accustomed to watch the phenomena exhibited by the capillary vessels in a natural state, in order that we may commit as few mistakes as possible, in tracing and recording the changes presented to us by disease; and it is essential that our minds should not be tinctured with particular views respecting the structure and functions of the vessels concerned in the process; but that we follow up the plan, so successfully adopted by Kaltenbrunner, of simply detailing what is offered to the sense of sight, without reference to any peculiar theories.

Several reasons might be assigned for the difficulty we experience in arriving at an exact knowledge of the proximate cause of inflammation, besides the extreme minuteness of the parts concerned in the process: an important one is our ignorance concerning some of the leading functions of the vessels. On this ground have arisen the numerous controversies, carried on more particularly by those who have never made any experiments themselves, but who, picking and culling from the researches of others, determine, in their own minds and to their own satisfaction, what shall and what shall not be the actual condition of the vessels in an inflamed part. Aware of this defect, the author of the short treatise before us, prefaces his observations on the pathology of inflammation by some remarks on the mechanism, structure, and functions of the blood-vessels, in which he glances at a few of their leading properties.

> "Notwithstanding all the theories," he says, "that have been advanced respecting the nature or proximate cause of inflammation, all must agree that the visible and tangible signs of it depend upon the condition of the arteries of the inflamed part and upon the modification of the circulation of the blood within them. Taking these facts as the groundwork of the enquiry, we shall proceed to examine what this condition really consists in; we may then be able to assign some rational or probable cause for the *inflammatory* appearance put on by so many diseases, possessing, in all other respects, such various properties, and leading to consequences so different in their nature. First, however, as our views differ essentially respecting the *natural functions* of the vessels principally engaged in the representation of the more striking phenomena of inflammation from those of any author with whose works we are acquainted, it is necessary to say a few words upon that subject." (p. 2.)

We must confess that we cannot detect anything either new or original in the views here put forth; and we think this will be obvious to our readers, on our laying before them an abridgment of Mr. Davies's statements, without any critical examination or special detail on our part.

The apparatus which conduces to carry on the circulation consists of a forcing machine, to which is attached an elastic tube, which tube divides and subdivides into innumerable branches. The heart is a muscular organ, its fibres being remarkably dense and with little cellular membrane intervening between them. Its contraction is *active*, but the dilatation of the cavities is *passive*, depending principally upon the *elasticity* of its structure. An explanation of the power by which "the muscular fibres of the heart are enabled to shorten themselves, below that medium which characterizes them as an elastic substance," although not "generally considered as pertaining to the *practical* part of the subject," the author very singularly conceives is "essential towards acquiring a rational idea of the pathology of inflammation." In the remarks which he deems it necessary to make on this point, and by which we are led to expect that the mystery will be cleared up, we are informed that organs and tissues, besides being composed of material molecules, have a *something* which confers upon them vitality, "that the properties and functions and effects of animate matter are only attributable to life, which an organ is capable of manifesting in a living state, and which it is not capable of exhibiting in the state of death," that for this particular reason the liver, "although as perfect for some time after death as before," refuses to secrete more bile, the stomach to digest more food, or the kidney to furnish any more urine; but that if we attempt to search into the *why* and the *wherefore* of these things, "our curiosity will be very likely to meet with disappointment." The arteries are considered as made up of three elastic tunics, the outermost, contrary to every other opinion and even ocular demonstration, being stated to possess elasticity in the least degree. No muscular fibres, after the minutest examination, can be detected in the middle coat. The vessels undergo no sensible motion during the circulation, and with the exception, in some instances, of a small portion of the aorta, are therefore mere passive tubes. They are always full of blood, each contraction of the left ventricle urging the column a step forward. If an artery be laid bare, and the finger placed very lightly upon it, no pulsation will be felt, while to the sight the vessel presents an unmoving cord. Two supposed prevalent errors are next combated, namely, that the arteries are always in a state of "forced distension," and pulsate independently of the heart. Having established these preliminary facts, respecting the condition of the heart and arteries during the natural and undisturbed circulation, the author next introduces us to that part of the subject upon which, we are told, mainly rest the new pathological views concerning inflammation. These are placed before the reader under two heads.

> "1st. The only motion which the arteries undergo is that of *gradual* contraction and of *gradual* dilatation, so as to adapt themselves to the quantity of blood within them. 2d. The calibre of all the arterial branches during life and in a healthy state is below the medium of their elasticity. The overlooking or being ignorant of these two facts have led pathologists into endless absurdities respecting the pathology of inflammation; at any rate respecting the theory of the phenomena presented by it." (p. 10.)

We are then shown, in the ordinary way, that the arteries are always full of blood, whatever quantity may be in circulation; and that they have the faculty of accommodating themselves to their contents. While no mention is made in what part of the vessels this contractile power resides, and while every trace of muscularity is denied them, it is most infelicitously attempted to illustrate the operation of this power by reference to the action of parts, as the bladder or intestines, whose muscular structure is so very conspicuous:

> "We now come," continues the author, "to the most important part of this subject. A material fact, which has been overlooked by physiologists, is that the caliber of the arterial tubes is always below that point which would obtain if the vessels were allowed to submit to their innate elastic forces. Instead of obeying the laws of dead matter, and of remaining at that state at which their elastic medium would place them, they are forced to submit to the vital force of contractility, and thereby reduce their canals to some extent below that medium." (p. 13.).

Do our readers require to be informed—what every physiological work proclaims, and every member of the profession knows—that the diameter of the arteries is greater after death than during life?—and yet this is the sum and substance, as far as we can see, of the "most important part of the subject," and the "material fact" which has been overlooked! Respecting the origin, distribution, physical or physiological properties of the capillary system, the immediate seat of disease, not a word is uttered.

We come, in the next place, to the "pathology of inflammation;" and here we regret being compelled to state at the outset that we have no favorable opinion to record; not that we quarrel with the author respecting the views he may choose to entertain, but that the matter introduced is, in many respects, so little connected with the subject under discussion, and the whole account of the process of inflammation, as it is seen going forward in the transparent textures, so miserably deficient, that the term "pathology," as here employed, is one almost of mockery. We shall extract those passages which show the condition of the vessels in an inflamed part, and contain all that is said respecting the nature of the disease. Alluding to the manner in which determinations of blood take place, it is said,

> "Now, in the case of blushing, it is often that the face alone assumes increased redness. It may be asked, how do the arterial branches of one part of the body manage to acquire more blood than their due proportion? The heart pumps out the fluid equally for the benefit of all the branches. The power, therefore, of causing the disproportion must reside in the arteries themselves. Then comes the question—the most *important* question—by what process, or by what means, do the arteries of one seat succeed in obtaining more than their proportionate share of blood? The answer is short and clear—*simply and solely by enlarging their diameters or calibers in that seat.* . . . . Now, if the *cause* (meaning blushing) were different—if it were of such a nature as to weaken *permanently* the contractile powers of the vessels, more especially if it totally *destroyed* that power, *then* the vessels would be *incapable* of resuming their natural diameters; an undue proportion of blood would constantly exist in them; the seat of the disturbance would present the appearance of redness, and some degree of swelling, and furnish all the characters of incipient INFLAMMATION. In a word, the visible and tangible characters of inflammation depend entirely and solely upon an undue enlargement of the capillary extremities of the arteries. The enlargement may, and often does, extend some distance towards the larger branches, but its origin is invariably in the capillary tubes, and its extension takes place by continuity along the vessels. This enlargement enables them, as a matter of course, to hold more blood than the quantity naturally or proportionally belonging to them; which circumstance is the cause of the 'redness' of an inflamed part." (pp. 22-3.)

The variations in the degree of inflammation are shortly accounted for: "The cause may be of such a nature as merely to weaken the contractile force of the tubes in the smallest degree, or it may be such as totally to destroy that force. Between these two points there are various degrees, and, as a consequence, the inflammation may present various degrees of intensity."

From this it appears that Mr. Davies is to be classed with those who look upon inflammation as consisting only in relaxation and enlargement of the capillary vessels, and who thus manage to compress the whole philosophy of the disease into a nutshell. We do not intend to enter on this long-debated question; but we would remark, that such simple views, whether of health or disease, however ingenious, can seldom be just. They have their origin too frequently in the spirit of system, not in the careful study and enumeration of the complicated circumstances which concur in the production of all vital phenomena. How miserably do such views fail in explaining the series of morbid changes which constitute the entire process of inflammation. Let those who have never witnessed these various changes as they appear under the microscope to the eye, but peruse the writings of others who have made them the subject of their researches, and then they will become aware of the folly of attempting to reduce the intricate phenomena of the disease to a silly hypothesis concerning the "contraction or relaxation" of a few capillary tubes.

The abstract consideration of either theory certainly involves the doctrine of the vital contractility of the extreme branches—their independent power of altering the distribution of the blood, and of accelerating its motion. Unless we grant these, it will be impossible ever to illustrate the nature of inflammatory action. The author, however, with several others, has been led away in his attempt to resolve the local action of arteries into a relaxation of their tunics; and, did our space permit, we could adduce ample reason for rejecting this opinion. If we admit that these vessels are endowed with a vital contractility, by which their inner coat is always kept in close contract with the blood, then we must allow that, within certain healthy limits, they are also capable of distention; for the circulating fluid varies in quantity at different periods, and it is only when these limits are exceeded, either by loss of tonicity in the vessels, or by the pressure of a greater column of blood than they are able to sustain, that relaxation and debility ensue.

The next section is dedicated to an explanation of the "cause of difference in the character of inflammation." Our knowledge of the progress of this disease in different structures—since attention was first directed to it in this country by Dr. Carmichael Smyth, and afterwards in France by Pinel and Bichat—affords a beautiful illustration of the rich store of pathological treasures which may be accumulated by careful observation and research. Mr. Hunter was of opinion that the character of inflammation could not be determined by the tissue affected, otherwise we ought to expect various kinds of inflammation in an amputated limb. Even up to the present hour we have no evidence to show what is the difference, if there is any difference, in the primary phenomena which in one case leads to adhesion, in a second to suppuration, in a third to ulceration, and in a fourth to mortification. We say that the process may be the same, for all we know to the contrary, which leads to these various changes, requiring only, in some instances, to be increased or diminished, and, in others to meet with a structure which, from its peculiar organization or vital properties, is more ready to fall into one species of action than another. To say that "tissue modifies inflammation, or that inflammation is modified by tissue," is like reasoning in a circle; it is a substitution of words for knowledge—a cloak under which our ignorance may remain concealed. We are too well aware that no light will ever be shed on this point by mere conjecture; we are sick of the miserable theories which are invented; and, after all that has been done, we still think that a physiologist could not confer a greater boon on medical science, at this moment, than by giving a faithful relation of the phenomena of inflammation, as they arise in various tissues. How satisfactory would it be to be able to assign the just reason why the disease terminates in different ways under different circumstances; why it should be followed in one membrane by effusion of lymph, and in another by effusion of pus, instead of being compelled to receive these statements in the way they are now made. We do not despair of this task being yet accomplished; although, in order to trace out the steps which nature silently pursues, we should have many difficulties to contend with, many conflicting doubts to reconcile, and many anxious hopes to keep in abeyance.

The chapter in the work before us which has given rise to these reflections contains nothing on which the mind can rest. To us, indeed, many parts of it are incomprehensible, whilst the suggestions thrown out are such as we are neither prepared to receive nor reject. Could we bring ourselves to admit as unquestionable facts what the author adduces as such, and conscientiously believes to be so, we should be equally happy to rest in the conclusions he has drawn from them.

The *Selections in Pathology* conclude with a few remarks on the "condition of the blood" and the "influence of the nerves" in inflammation. While, in the former, there is little to commend, there is a good deal of what we conceive to be objectionable. In alluding to the morbid appearances observed in animals whose blood has been deprived of the greater part of its fibrine, it is attempted to show that the process of ulceration may be occasionally independent of inflammation. The illustrations adduced in proof are surely nothing less than absurd. They are—the *removal of structure upon a large scale, in cases of emaciation from want of food*—the fact, *that a stomach had, in one instance, consumed a portion of itself after death*—and the circumstance of *the ulceration which characterizes a chancre being often unattended by the least appearance of inflammation.* In speaking of the condition of the blood in inflammation the author seems inclined to the opinion that a morbid alteration in the circulating mass takes precedence of the local disorder. Instead, however, of addressing his observations to the question as it concerns inflammation, he enters upon the doctrine as it stands related to fever. With the latter we have nothing to do at present; but, as regards the former, we may be permitted to observe that, except where the blood contains an excess of fibrine, or is deficient in that material, the circulating mass seems to us to be *secondarily* affected.

The phenomena of the buffy coat, the causes of its formation, and the circumstances accompanying it, are scarcely touched upon, and what is advanced leaves the subject, to say the least, in no better condition than where it was found. That we may not be thought unjust, we shall give the following sentences without comment:

> "Where the disposition to form the buff coat exists, if any number of portions of the same blood be drawn at the same time into different vessels of the same shape and size, the consistence of the cake will, in general, be found to vary in them all. The quantity of serum expressed, and the density of the coagulum, will be *least* in the portion drawn first, and both will *increase* in the order in which the different portions were abstracted. . . . . On the contrary, inflammatory blood will sometimes cease to be buffy after the first or second bleeding, though by far more commonly this quality *increases* in proportion to the quantity abstracted. It may be noticed, as a general rule, that the *thinner* and more *emaciated* the patient is, the denser and, generally, the quicker the coagulation will be. In *such constitutions* the blood presents the appearance of buff in the most *trifling* cases of inflammation; whereas, in *strong, robust,* especially fat, individuals, the fluid often exhibits hardly any buffiness, even in *severe* cases. The buff, as by far the more general rule, bears an *inverse* ratio to the vital powers of the system." (pp. 62-3.)

In concluding our remarks on the first part of this work, we must confess that we cannot understand what could have induced the author to give its contents to the world as exhibiting "new pathological views." Surely he cannot be ignorant of the fact, that all which he has advanced respecting the "functions of the blood-vessels," or the "pathology of inflammation," has been the subject of discussion for many years back;

and we, for our parts, have not been able to discover any new light that has been thrown upon it.

We turn now with pleasure to the second part of the work, which consists of practical observations on the external use of iodine in various surgical cases. These observations are prefaced by some general remarks on local therapeutics, wherein the author takes the opportunity of stating his opinions respecting diseased action, the manner in which it takes place, and in which topical remedies succeed in removing it. Although we cannot coincide in the leading propositions laid down—and chiefly for a reason which may appear somewhat extraordinary to some, namely, that they are too simple to explain the intricate operations of nature—the chapter is, nevertheless, well worthy of a careful perusal. We will quote a passage which at once affords a key to the views brought forward, and will be found useful to remember as we proceed. After laying it down as the *grand principle* that disease is the same in its nature, whether situated externally or internally, and that during its progress secretion exceeds absorption, the author remarks:

> "We have stated that when a part is undergoing disease, the secreting function generally overcomes the absorbent, which causes a preternatural deposit of matter. This is usually the first step towards the disorganization of the structure. The question to be now considered is, how does this condition of the part happen? It may, and probably generally does, owe itself to two causes: first, in consequence of the vital derangement of the capillary or secreting vessels, the calibers of these vessels enlarge, so as to enable more than the due proportion of fluid to pass through them; and, in the second place, as the absorbent function is the reverse of that of secretion, and must be performed by a *different* class of vessels, a similar derangement and relaxation must diminish their force, and thereby render their function *less* active than natural. For instance, if the vital derangement of the capillary extremity will cause this extremity, in consequence of its preternatural enlargement, to deposit two atoms in the time it could only deposit *one* in its natural state, it does not follow that a similar enlargement of an absorbent vessel, to whatever class it may belong, can *take up*, and transmit, twice the number of atoms in the same time. On the contrary, any loss of contractile power in the absorbents, (or the extremities of the veins, or imbibing pores, or whatever the nature of the absorbing apparatus may be,) must render their function less active than it is in their natural state. If the preceding view be correct, it follows that the same morbific cause which is calculated to *accelerate* the secreting function, has also a tendency to *reduce* the activity of the function of absorption. As in all cases of local inflammation the dimension of the capillaries is considerably increased, they have a morbid necessity of depositing more than the natural and healthy quantity of materials in the seat of disease; and as, on the other hand, the force of the absorbent vessels is diminished by the same cause, the inevitable consequence is, first, a simple swelling; and, ultimately, an organic change of structure." (pp. 69-70.)

The morbid phenomena in all local diseases are further considered as analogous to those which characterize inflammation. As the properties of the arterial extremities vary in the different tissues, and as the tissues themselves are also innumerable in variety, it is no wonder if their derangement should lead to organic changes so different in character. Such being the state of a part when disease is advancing, the obvious therapeutical indication, after the removal of the morbific cause, is not only to restore the lost balance between the deranged functions of secretion and absorption, but, if necessary, for a time to give the latter a preponderance over the former. This is to be accomplished by local remedies which are capable of *recovering the lost contractility of the minute branches and augmenting the action of the absorbents.* In inflammation, local bleeding, evaporating lotions, poultices, fomentations, liniments, escharotics, and pressure, are all explained as operating in this manner.

Were any one so evil disposed as to attempt to prove that the nature of a disease does not necessarily depend on structure, for the same structure may give rise to several kinds of disease; or to deny that the secerning and absorbent vessels are always in a state of relaxation, the foundation and superstructure built upon it would, unfortunately, be swept away; but this ungracious task we are not disposed to undertake.

The utility of iodine, as an external remedy, is prefaced by some judicious remarks on its therapeutic properties, and on the best mode of applying it in a variety of morbid states. As an internal remedy this substance has now been long administered, but without any knowledge of its action, and often without any well-defined object in view; while its outward curative properties seem to have excited little attention. It is not the object of the present essay to take any notice of its inward administration: it being merely remarked, in passing, that after long trial, it has not been found to cause absorption of the mammæ or testes, as usually represented; that it has failed in removing scrofula, for which it has been said to be a specific; but has been found useful in continued dyspepsia, and some other affections of the mucous membrane of the alimentary canal. In bringing before his medical brethren the merits of a remedy which seems to have been too long neglected, the author, being well aware that, to propose any substance as a cure for numerous complaints, is apt both to occasion distrust in its efficacy, and lay the proposer open to a charge of empiricism, has determined to state nothing which repeated observations have not proved to be correct. He has, likewise, very candidly warned us against its indiscreet use—a caution that it would be well to observe on like occasions.

> "In urging a remedy on the attention of the profession, it is necessary, at the same time, to caution the members of that profession against the indiscriminate and indiscreet use of it. Many valuable medicines have fallen into disrepute, and have been altogether discarded from the list, owing to their having been mismanaged or abused. Iodine, though not a *new* remedy, has by no means yet had its effects on the human system fully tested. Properties have been attributed to it of which it is quite devoid, while, on the other hand, it is endued with many therapeutic virtues which it has not been generally known to possess." (p. 81.)

The remedy is to be employed in two forms, but principally in the shape of tincture, made by dissolving two scruples of iodine in an ounce of rectified alcohol, the strength of which may be reduced by the addition of more spirit: the other, an ioduretted solution, consists in dissolving thirty-two grains of iodide of potassium in an ounce of distilled water, and adding to it eight grains of iodine. The latter is not so useful as an external application, as, from its containing the hydriodate, it is rendered acrid, and causes considerable smarting when laid upon an abraded surface, diluted, however, with from seven to ten parts of water, with the addition of a little honey, it forms a good iodine gargle in ulcerated sore-throat. Nevertheless, the alcoholic tincture is preferable in all cases, provided the practitioner can find time to apply it himself—an indispensable condition, apparently, in its use. The strength of the preparation is to be varied according to the *quality* of the skin, and the nature or intensity of the disease. Where the former is thin and delicate, the tincture must be reduced one half by adding spirit; where a part, again, is acutely inflamed, or threatening to slough, it ought at first to be applied undiluted, and afterwards weakened as the necessity for it diminishes. The effect of its application to ***healthy*** skin is a sensation of heat and smarting; sometimes only a slight and agreeable warmth, followed by desquamation of the cuticle. When too long continued, the skin is liable to form watery vesicles, which give way in a few days to a lotion of spirit and water. When applied to an *inflamed part*, on the contrary, the pain soon becomes deadened, and a sensation of warmth succeeds, but which subsides in a few hours, leaving the patient free from any uneasiness in the seat of disorder. Brought into contact with a sloughing ulcer, a part on the verge of gangrene, or a foul, irritable sore, it is seldom felt by the patient. In a healthy ulcer, a recently-lacerated wound, or any healthy raw surface, it produces generally a very sharp smarting pain for a minute or two, but which ceases and leaves the part in a comfortable state.

The rationale of the operation of this remedy accords, of course, with the author's views respecting the formation and removal of disease; it excites contractility of the capillaries, thereby diminishing secretion, while, at the same time, it increases the activity of the absorbent system. The reader, however, is requested not to take the failure to give a satisfactory reason for its mode of acting, as a justification for rejecting the ascertained *facts* respecting its effects. To prevent repetition, we shall here give the mode of using the iodine, in perhaps one of the worst cases that could come under our notice.

> "Suppose we are called to a case of severe inflammation of the leg, in a stout, robust person: the limb is intensely red, hot, swollen, and glossy, all the way from the toes to above the knee; it is double the size of the corresponding one, and so painful as to disturb the general health, as to cause quickness of the pulse, white tongue, thirst, &c. We immediately *paint* the whole limb with the tincture of its full strength, extending its application from the toes to several inches above the upper margin of the inflammation; the remedy is applied with a camel's hair brush. This is all the local application requisite for the present. The limb is directed to be kept in a horizontal posture, and either to be very lightly covered over with a sheet, which must not come in contact with the skin, or else to be left exposed, according to the temperature of the apartment. In less than twenty-four hours, in less than twelve hours, the swelling will be found to have diminished. At the end of twenty-four hours the skin will be seen much corrugated, showing its contents to have become less in bulk, and the circumference of the limb will measure some inches less than the day before. The diminution will be found to have taken place more particularly towards the upper part of the swelling. We now repeat the application of the tincture, of the same strength. In another twenty-four hours the reduction of the swelling will have gone on rapidly, and only a remnant of the disease will be found to exist. The strength of the tincture must now be reduced to one half, and its application continued daily, or less often, according to circumstances, until the limb is well. After the second or third application of the tincture, we sometimes brush the limb over with spirits of wine alone, so as merely to dissolve the iodine which remains on the surface of the skin." (pp. 83-4.)

We will now briefly state the diseases in which iodine forms so useful an external application, premising that the local treatment, however instrumental in reducing the constitutional disturbance, by assisting in the removal of the disease, must not supersede general remedies where these are required. First, then, in *erysipelas*, in whatever situation and of whatever description, the author has found the tincture, applied as above, preferable to leeches, lotions, incisions, scarifications, or caustic. In common *phlegmon*, the same beneficial changes are seen. Where pain and throbbing only exist, it will be found that one application of the full-strength tincture will cut short the disease; and where suppuration has commenced, its repeated use has not only checked the progress of the disease, but caused the deposited matter to be absorbed. In *very* deep-seated inflammation among the muscles, as in the thigh or loins, the efficacy of the remedy is perhaps doubtful. Not a single case of failure was met with, where the tincture was applied in superficial phlegmon before suppuration occurred; and even then the quantity of pus was much less than where poultices were used. In *extensive sloughing of the cellular membrane* after phlegmonous erysipelas in the lower extremities, where the tissue protrudes through ulcerated openings in the skin, and sloughs in large quantities, the tincture is the most valuable application. While the usual remedies have no effect in checking the inflammatory process, the iodine at once arrests it, and gives the living parts a chance of casting off the dead slough. Cases illustrating the utility of the remedy in these three states are adduced, and which are selected from amongst numerous others that could have been brought forward.

In diseases less threatening to life, the remedy is perhaps still more successful. In *acute inflammation of the joints*, it is more efficacious than any local application in common use. Over the knee, if the skin is delicate, it may be applied at first about half its strength, and increased gradually as required. When the hip is affected, the strong tincture should be painted all around the upper part of the thigh and groin. In these cases, the author prefers leeching the joint, and then using the iodine as soon as the bleeding ceases. In *inflammation of the breast*, as soon as the disease is discovered, the tincture of full strength is to be laid extensively over the part, and even if abscess should form, its extent will be limited. In *gout*, its application cuts short the attack; and in anomalous pains of the joints, supposed to be gouty or rheumatic, its effect has been very noted. The tincture is diluted to about two thirds its full strength. In *chronic inflammation and enlargement of the joints*, such as the hip and knee, leeches are first employed, and then the diluted tincture is laid extensively over the part, and repeated every two or three days according to its effects on the skin. The plan is to be persevered in for a period limited only by the duration of the disease. In the ankle or wrist, where the enlargement is of old standing, the iodine lotion is preferable to the tincture, a rag being wetted in it three or four times a day and laid round the joint. The strength of the lotion must be determined by the discretion of the attendant. In *inflammation of the absorbents*, the strong tincture applied along the whole track of vessels, generally suffices to subdue the disease. In *carbuncle*, used before or after incisions, it will dispel the inflammation, and enable the parts to cast off the dead cellular tissue and form granulations. It is equally applicable to *boils* and *buboes*; in the latter cutting short their progress, or else, if used after suppuration, limiting the extent of the abscess. "These," says the author, "are real facts, facts easily tested by trial." In *lupus*, or *noli me tangere*, the strong tincture laid upon the ulcerated surface has cured the disease without any internal remedies; in one case, however, it failed, although assisted by the internal use of the arsenical solution. In *malignant ulcers of the tongue and tonsils*, the tincture of full strength, brushed all over the parts, arrests the affection however threatening. The only internal remedy was the ioduretted solution, in doses of ten drops twice a day in water. In *scrofulous swelling of the glands*, it either resolves the inflammation and causes absorption of the morbid deposits, or limits the formation of matter and assists in the cicatrization of the sore. In *whitlow*, the strong tincture is to be immediately painted over the whole finger or thumb, and repeated in *twelve* hours, unless the morbid sensation has ceased. Where this has been done before suppuration, it never fails to subdue the disease. Should matter exist, a free incision must be made, and the tincture then applied over the finger or hand if swelled. In *chilblains*, the remedy of full strength is to be applied over the sore and beyond the boundary of the surrounding inflammation, and repeated daily for some time. The affected parts should be immersed every night in water as hot as can be borne. When the ulceration looks healthy, and the skin around has lost its livid colour, the strength of the tincture may be reduced. After each application, the sore should be dressed with some stimulating ointment. In cases where the inflammation spreads along the foot or leg, the affected parts must be *painted* with the strong tincture. It is not stated what effect the remedy has before ulceration takes place.

The tincture of iodine has been found preferable to every plan of local treatment in *lacerated, contused, and punctured wounds*. When the accident is one of *simple laceration*, after the blood or dirt are wiped away, every point of its surface is touched over with the remedy, generally of full strength, and its application extended a little distance beyond. After allowing it to dry, the edges are brought together with sticking plaster, which is not renewed for three or four days, when part will be found united and the rest granulating. The latter, with the surrounding skin, is again brushed over, and then dressed with common wax-ointment. The cure is generally rapid. Where *contusion* only exists, the tincture is applied every day or two to the surface, and quickly causes absorption of the extravasated blood. Where there is a *combination* of laceration and contusion, the treatment is compound. The surface of the wound and contusion is brushed over, and the edges of the former then approximated and kept together by plaster or a roller. The remedy is reapplied according to the necessity of the case. In *punctured wounds*, from whatever cause, the remedy liberally applied is used with that undeviating success which it exerts in local diseases and injuries attended by inflammation. In such cases it should be allowed to insinuate itself freely into the wound, and be thickly painted upon the surrounding skin. In *burns* and *scalds* it seems to act as it does in erysipelas. When the integuments are not destroyed, although the cuticle may be in blisters, one or two applications of the tincture, of moderate strength, subdue the pain and redness, after which the parts only require to be kept free from injury. Further trials will be necessary to determine its effects where the violence has been extensive. Lastly, the remedy is eminently successful in *ulcers.* We have already noticed its efficacy in lupus. Several cases of chancre have yielded sooner than to the ordinary treatment, while in the malignant ulcerations about the lips, tongue, or tonsils, no topical remedy we possess is equal to it. In all cases of irritable, or sloughing sores, the tincture of full strength should be applied to the surface and surrounding skin. After being allowed to remain some time, the ulcer should be covered with simple ointment in preference to a poultice. The application is repeated daily till the sore becomes clean and healthy, when the granulations may be touched with the diluted tincture every two or three days. Under this plan, the cavity of the ulcer fills up rapidly. Besides these various affections, the tincture has been employed with good effect in gouty and rheumatic swellings of the small joints from thickening of their ligaments, fistulous openings, malignant warts or adventitious excrescences, ganglions, the stinging of wasps, diseases of the spine, ununited fractures, hernia humoralis, inflamed urethra and chordee, inflammation of the bursæ, chronic ophthalmia, and opacities of the cornea (much diluted), dissection wounds, or scratches exposed to the dead body in dissection, &c. The strength of the remedy in the several cases must depend upon the judgment of the practitioner.

We have been anxious to present our readers with a short sketch of the various cases in which iodine has been found so efficacious as an external remedy. We have done so, with the twofold view of inducing them to study the work itself, and put the remedy, as soon as possible, to the test of experience. The statements respecting it are put forth in a plain, lucid, and straightforward manner, and without any desire to exaggerate its effects. The trials to which it has been subjected have been in every instance numerous, and apparently run over a period of ten years. We do not pause, therefore, to make any comments on the treatment here recommended; the facts to us, and we presume to many others, are novel and extraordinary; and it would be unbecoming to say anything of a line of practice which we have never witnessed, far less followed out. We trust that those who enjoy the opportunities will give it a fair and impartial trial, and communicate the results in such a way that they will be diffused amongst those whose means of acquiring information are more limited. Its efficacy appears so surprising, that this will only be an act of public justice towards the author and mankind; and we doubt not that the former will agree with us in courting every enquiry; if, as we expect, it shall be found considerably less *heroic* in other hands—a circumstance readily explicable without the least imputation on the author's truth and judgment—Mr. Davies will still merit the thanks of his professional brethren; and we strongly recommend his Essay on Iodine to their perusal.

Art. XVI.

An Inquiry into the Morbid Effects of Deficiency of Food, chiefly with reference to their occurrence among the Destitute Poor; also Practical Observations on the Treatment of such cases. By Richard Baron Holland, m.d.—*London*, 1839. 8vo, pp. 77.

When we learn, from some good people, that man may live well on a vegetable diet, and that his clay requires nought but water to moisten it, instead of gainsaying them, we refer them to the corn mart, in Mark Lane, or to some other assemblage of tillers of the soil, for specimens of the connexion of air and exercise, meat and beer, with large, athletic, vigorous, healthy forms. And when we find some of our well-meaning acquaintance educating their children in habits of abstinence and water, and complaining that, notwithstanding their careful dietary, the little growths are not so vegetative as they could wish, we have often found reason to congratulate them, that the daily allowance of a glass of stout and a little more meat has entirely altered the appearance of things; has put an end to a catalogue of pains, and aches, and disorders; has coloured the pale cheek, and has made what promised to become a rickety bantling into a reasonably healthy boy. Why mankind requires the diet which we should recommend is a question which would be better answered by an experienced breeder of cattle than by any one beside. At present we are disposed to be dogmatic; and to say that, in the great majority of cases, whether from the habit of parents having communicated a particular disposition to their offspring, or from the effects of injudicious general intellectual and physical management, the strongest organization will not be raised in this country on oatmeal and potatoes—add to these as much water as you may—but on good meat and bread, with *genuine* infusion of malt and hops: we say "genuine," because the scandal which attaches itself to beer is—sometimes at least—owing to the poisoned compound which it too often pleases and profits brewers to sell under that name.

Dr. Holland has written a useful book, and one which merits, and, we trust, will receive a more extended application than he has chosen to make of it. Connected with the Royal Infirmary and Poorhouse of Manchester for more than eight years, he has thereby enjoyed considerable opportunities of studying the subject of his little work. The habits of the manufacturing poor of Manchester are those of manufacturers generally. They are improvident, careless in their diet, clothing, and lodging; confined almost wholly in a vitiated atmosphere; wearied with long-continued exertion, and never refreshed by sufficient repose; intemperate in the use of fermented liquors, for the sake of which they deny themselves wholesome food, and live on that which is bad and innutritious. The consequences of such a mode of life are, that "the digestive organs become impaired, and the function of digestion is so feebly and imperfectly performed, that even much less nutritive matter is extracted from the indigestible and impoverished diet they use than would be the case if the stomach and its appendages were in a healthy and vigorous condition—a disordered state of the mucous membrane of the alimentary canal is produced; it becomes irritable and morbidly sensitive—the secretions are vitiated, profuse, or defective, and abdominal pains, diarrhœa, or constipation is the consequence." (p. 6.)

The symptoms produced by deficiency of food are carefully noticed by Dr. Holland. In cases of very gradual starvation, an urgent feeling of hunger is not a prominent symptom; and even when it exists at first, it usually soon diminishes, and is succeeded by a feeling of exhaustion and faintness, and even by loathing of food, if the abstinence has been long protracted. The mental condition connected with poverty is mentioned as in part accounting for this deficiency of appetite. A morbid state of the nervous system is among the most interesting circumstances connected with deficiency of food. We do not know any subject of more practical importance than this in all its bearings, and we shall quote Dr. Holland's remarks upon it.

> "The depression produced on the nervous system is very early manifested in the impaired energies of all the vital functions, the weakened conditions of the intellectual faculties and moral feelings, and diminution of the general sensibility. Disturbance of the cerebral functions is at first shown by an unnatural languor, despondency, and listlessness; slowness and hebetude of intellect, with an inability to employ the thoughts steadily and profitably on any subject. Notwithstanding all this general languor, however, the patient sometimes manifests a highly nervous state; he is startled by any sudden noise, and hurried by the most trifling occurrences. He is liable to attacks of giddiness, 'swimming in the head,' staggering, dimness of sight, with temporary delirium, and either falls as in an apoplectic fit, or lapses gradually from a lethargic state into one of stupor, or even of complete coma. In many respects, the symptoms in these cases have considerable resemblance to the effects of exposure to cold. Mania, or mental imbecility, has sometimes been produced by defective nutrition..... In consequence of the torpor of the brain and intellectual faculties, it is often extremely difficult to obtain the requisite information from patients. Instead of showing any anxiety to communicate the symptoms and cause of their illness, or to relate the privations they have undergone, they generally have an unwillingness to be questioned—lie in a listless or lethargic state, without taking any notice of what is going on, and seem desirous only not to be disturbed. .... Careful observation has convinced me that the listlessness and torpor of the mental faculties—the tendency to fainting or to perfect syncope, and, finally, a state of cerebral oppression, amounting in some cases to coma, are among the most characteristic symptoms of defective nutrition, and the surest indication of its existence to a serious extent. The effects too on the moral feelings are very striking, and often truly deplorable." (pp. 26-30.)

Dr. Holland observes that the symptoms which he has mentioned as characterizing a deficient nutrition vary from those which are commonly associated with that state; but it must be borne in mind that other causes, of a depressing character, to which we have already alluded, in addition to deficiency of food, are in constant operation on those individuals from whom his observations are taken. But there is a multitude of cases of minor degrees of suffering, in which the symptoms are less marked than above mentioned.

> "Such a state is indicated by a sallow and dingy appearance of the skin, a soft and flabby feeling of the flesh, more or less emaciation, general debility, feebleness of the circulation, and frequently swelling of the ankles. The stomach becomes disordered, the appetite defective, and digestion impaired. The individual feels languid and desponding, is soon fatigued, incapable of exertion, and has an irresistible dis-

position to fall asleep, from which he is apt to awake suddenly and in a fright. The body is easily chilled, breathlessness and palpitation are experienced after slight exertion; attacks of vertigo, tinnitus aurium, and transient blindness, are common, and there is a peculiar forlorn and dejected aspect of countenance which is very characteristic. This state of things is commonly soon succeeded by some specific disease, though it sometimes continues, with only slight variation, for a very protracted period, until the patient falls by slow degrees into a state of mental as well as physical incapacity, and, being no longer able to procure any employment, is completely invalided, and applies for medical relief." (pp. 33-4.)

The above remarks must be regarded as applying to the less frequent cases of the effects of deficient nutriment. The instances in which this deficiency acts as a predisposing cause of many diseases are far more numerous. In a political view, the condition of the intellectual faculties and moral feelings which is induced by defective nutriment is a subject of the highest importance. And in carrying out any system of education which shall afford a prospect of really bettering the condition of the poorer classes, we entirely agree with Dr. Holland, admitting his saving clause, "that the first step towards improving the mind is to preserve the health, by providing efficiently for the wants of the body." It is now well and, we trust, generally known that a plethoric and an opposite condition of the system may give rise to symptoms nearly resembling each other; the treatment required, however, being different in each. The discrimination of these differences is of great importance, both in states of simple exhaustion and where with this is combined any actual disease. On this subject we shall quote some of Dr. Holland's observations.

"The accession of coma is one of the most severe and fatal signs of exhaustion from defective nutrition. This symptom has been too indiscriminately regarded as indicating a determination of blood to the head or pressure on the brain, and as requiring abstraction of blood for its removal. It must never be forgotten, however. that it arises from a very opposite state of the system—defective circulation, exhaustion or deficiency of nervous power. Coma itself affords no evidence of the state of the brain, unless taken in conjunction with other symptoms and the history of the case. .... I may also mention here a circumstance, which I have often observed, viz., that when coma supervenes towards the termination of diseases of exhaustion, and the pulse becomes slower, it often acquires a degree of fulness and gives an idea of strength, quite at variance with its previous character, and little to have been anticipated from the debilitated state of the system." (pp. 65-9.)

With respect to the treatment of the morbid effects of deficiency of food, it is well known that, in individuals long accustomed to an impoverished diet, no sudden change should be made to food of an opposite character. Such food should be given as will support returning strength, without either burthening the powers of digestion or producing a too stimulant effect. Where exhaustion is so great as to endanger life, much more is required; the horizontal posture should be strictly maintained; heat applied to the surface; diffusible stimulants administered internally.

"If positive coma exist, the sesquicarbonate of ammonia ought to be given with persevering assiduity. We must not be deterred from resolutely using this remedy, with brandy or wine, from any fear that the brain is 'labouring' or congested, or be

> tempted, with the view of relieving this, to abstract blood at this stage of our treatment; because, whether such be the case or not, that is not the treatment calculated to remove it." (p. 73.)

Symptoms of reaction follow those which indicate the languor produced by exhaustion; such are "flushing of the face, intolerance of light, headach, restlessness, delirium, a dry tongue, and quick pulse." And these require patience and care, frequently with the use of a sedative for their relief.

> "During convalescence, from the effects of deficiency of food, congestions are very apt to occur, which require blisters, and sometimes leeches or cupping. This is a natural consequence of the weakened state of the circulation. Pains of a neuralgic character, particularly of the head, are often experienced, for the removal of which, quinine and iron, alone, or in combination with morphia, is generally effectual. If palpitations and irregular action of the heart should be troublesome, they will be most speedily relieved by the judicious combination of tonics and sedatives." (p. 76.)

The subject of treatment of the effects produced by defective nutrition—and under this head we include both food and drink—might have received, with advantage, a more extended notice than Dr. Holland has given to it. There are several points on which, seeing the attention which the author has given to the subject of his essay, we should have been gratified to have had his opinion. According to our experience, there are several classes of individuals, not in good health, to whom Dr. Holland's remarks and observations are especially applicable. To some of these we shall briefly allude. We would, however, premise that, in reasoning on the subject of diet, it should always be borne in mind that men are rarely living in a wholesome state, but are habituated to practices very foreign to health; that the exertions which, in a simple state of society, would have sufficed for maintenance, are not regarded as sufficient now, and that they are too often accompanied by an anxiety and disappointment which are as exhausting as the exertions themselves; and, moreover, that the effects of these and many other circumstances, which must occur to every reflecting practitioner of medicine, are such as diminish and exhaust power. It would be very easy, and on that account it is unnecessary, to give any illustrations of the above remarks. In applying Dr. Holland's observations, we would allude, first, to the poor, who cannot get food convenient for them; secondly, to those who, having been used to a full and somewhat stimulant diet, have, either from necessity or from some conviction of its moral propriety, diminished their allowance to what would be sufficient for those who have never been accustomed to anything better, but which is insufficient for them; and lastly, to those who have always been used to such a diet as would suit the majority, but which is not proportionate to the demands of their constitution, and the exertions, either mental or bodily, to which their daily avocations call them.

A careful investigation of the history of a case, and great attention to both hereditary and acquired constitutional peculiarities, and to temperament, are necessary to the practical application of any dietetic rules to these several classes. But we believe that it is the diet, far more than any medical treatment, that is important in these cases; and, not until diet is made a subject of more permanent importance than is at present the case, will the physician obtain, in the management of the vast class of cases termed nervous, the credit which he assuredly does not at present possess. It has fallen to our lot to observe cases of this kind, of years' duration, and some of the most distressing character, which have been subject to all the systems of simple medical treatment which ingenuity could devise for their relief, but which have speedily, we might almost say instantaneously, been relieved by the discontinuance of medicine, and the employment of diet and regimen instead. We could derive, from facts of this sort, the strongest arguments in favour of change in the present mode of remunerating medical practitioners in this country; such a change, in fact, as should tempt the practitioner, on seeing his patients, to ask himself—instead of "What medicine shall I give in this case?"—"Is it necessary to give any medicine at all?" But we must terminate our notice of Dr. Holland's work by a few remarks illustrative of those which it has already called forth.

The first class of cases which we have mentioned is that treated of by Dr. Holland; and to his judicious observations we have nothing to add. Concerning the second class, we should say, admitting that, with all the conceit and pharisaism which it engenders, the system of total abstinence from fermented liquors is not, in some instances, without its advantages; there are very many persons who have so habituated themselves to a certain stimulus, that it is to the complete sacrifice of their comfort and of the bodily energy which is requisite for the due performance of their duties, for them to discontinue it. We should quite as soon think of recommending such persons to throw off the flannel waistcoat which has saved them from or cured them of rheumatic pains; or the great-coat which has preserved them from shivering throughout the whole winter; or to discontinue the meat breakfast, which has built them up for the entire day, as to abstain from a good allowance of ale or wine; the latter of which, we are assured, on the best authority, is good for the stomach sake and our often infirmities. There are many individuals belonging to the third class; and in these it is frequent to observe some one or more of the symptoms alluded to by Dr. Holland, and which we have quoted. They eat their daily meals with appetite, and the quantity and quality of them are such as would suffice for many; but they suffer from palpitations and intermittent action of the heart, or from very frequent despondency, and sometimes of the most distressing character, or from repeated headachs, increased by the upright posture, or any bodily exertion, or from some other functional derangement. A moderate daily allowance of good beer or wine, with an increase of animal food is a rapid, certain, and perfect cure for these complaints; and a life previously miserable may be thereby rendered joyous and happy.

But we have extended these observations further than was at first our intention, with, however, no regret, as we are perfectly convinced of their practical importance. We conclude by a cordial recommendation of Dr. Holland's valuable little work.

PART SECON

Bibliographical Notices.

Art. I.—*Practical Observations on Diseases of Women.* By William Jones, m.r.c.s. *Illustrated with Cases and Explanatory Plates.* —*London*, 1839. 8vo, pp. 226.

Although we think Mr. Jones ought not to have published this book, and although we believe he will himself shortly admit, if he does not do so already, that there was no sufficient reason for his doing so, still we by no means wish to condemn it entirely. There are several useful and carefully related cases in it, but there is much of a questionable character, and there is very little that has not been abundantly published before. Mr. Jones's chief object is to magnify the utility of physical examination in uterine diseases, especially with the aid of the speculum uteri, and to urge upon those who are willing to aid in so desirable an object, the expediency of founding an institution especially devoted to the treatment of the diseases of females. The first chapter treats "of physical examination in cases of disease generally," and is a very careless chapter indeed. Thus we read that "pathology, while it teaches that alterations of structure are the causes of disease, teaches also that these alterations of structure vary *ad infinitum;* and that disease, Proteus-like, assumes a thousand varying forms, according to the numerous influences and agencies to which the human economy is subjected, each alteration being recognizable by certain physical characteristics, either during the life of the individual, or upon *post-mortem* examination." We have always been disposed to regard the alterations of structure, not as the causes of disease, but as part of the disease itself, the causes consisting in something antecedent to the change of structure. A little further on we read, "Whenever, therefore, we are called to a case of disease, we have two duties to perform, first to determine what organ it be whose altered structure gives rise to the symptoms observed; secondly, to determine the nature of the alteration which the organ may have undergone." If the preceding quotation is applied to disease generally, we can conceive nothing more incorrect; if it have any more limited application, it is carelessly indefinite in its expression. Again, "the nature of disease is to be determined by its physical symptoms, which can only be recognized through the medium of the perceptive faculties, or of what may be called physical examination." This is a simplification of the nature of disease which is quite beyond our comprehension.

Mr. Jones is an ardent advocate of the use of the speculum uteri. His advocacy, in fact, sometimes makes him incautious, for he both tells us that "he has rarely met with the objections and prejudices which some practitioners assert to exist against its use," and that "too frequently has he had to encounter the many difficulties connected with the employment of the means he advocates, to be unacquainted with the prejudices of the public respecting it." The latter statement we are more disposed to credit, and to a certain extent not to discourage the feeling whence the prejudice arises. We have on previous occasions stated our opinions respecting the use of the speculum uteri, and are not inclined to alter them. An interesting chapter is given on the history and use of the speculum, with drawings of the various forms of these instruments, which have been and still are employed, and the objections which have been urged against the employment of the instrument are considered. We are quite disposed to agree with Mr. Jones, "that the speculum is a very valuable instrument; that, by its use, information may be attained that cannot be acquired by any other means; and that, consequently, remedies may be suggested for the relief of uterine diseases, materially differing from those which would have appeared most beneficial in the absence of its employment; and hence we must admit that the practitioner who, in the present advanced state of pathological science, neglects its employment in *the cases indicating its necessity*, incurs an awful responsibility, if he be not highly culpable." We have taken the liberty of marking in italics the qualifying words in the above quotation, as we might perhaps differ from Mr. Jones in the choice of cases in which the necessity was indicated, and certainly there are cases related in his work which we feel convinced he would have treated quite as successfully without, as he did with the employment of the speculum. The following is a description of an apparatus contrived, and made use of by the author (and of which a drawing is given) for placing a patient in the most favorable position for the introduction of the instrument.

"It consists of a frame supported on four legs, to which a cushioned top is attached by hinges at one extremity; by means of these hinges and a rack placed beneath it, the top of the table may be made into an inclined plane, so as to give to the pelvis the requisite elevation; by means of a cushion for the head, those inconveniences may be obviated which might result from retaining the head in a dependant position. To prevent the legs of the patient getting in the way of the operator, two iron rods with semi-circular cushions are attached to the table, over which the legs are placed; these rods are moveable and are capable of giving different lengths, to correspond with the varied extent of the femora of different females." (p. 68.)

The book terminates by a collection of cases of various forms of disease of the generative organs, many of which are deserving of a careful perusal. In the section on amenorrhœa, the following theory of menstruation is given:

"In the earlier periods of infancy, the development of the ova is extremely slow, but as the period of puberty arrives, one or more acquires a rapid development, enlarges, and becomes distended, and making pressure upon its investing membrane excites irritation in it and in its neighbourhood;—the termination of one of the fallopian tubes participates in and conveys that irritation to the uterus; the uterus thus excited becomes a centre of determination, and favoured by the vascularity of its tissue becomes a seat of congestion, until the minute terminations of its vessels, no longer capable of resisting distension, allow the congested fluid, somewhat altered by its passage, to escape in the form of the catamenia, meanwhile the ovum, grasped by the fimbriated extremity of the fallopian tube and becoming distended by the excitement of which itself is the centre, bursts from its confines, passes through the tube into the uterus, and subsequently is expelled, if it be not impregnated; the excitement gradually subsides after the detachment of the ovum from the ovary, and reappears at the next catamenial period, with the same train of phenomena. In support of this theory of menstruation, we have daily opportunities of remarking, at post-mortem examinations, not only that the ovaries of unmarried women bear the traces or cicatrices of detached ova, but in many instances also of proving that the number of those cicatrices corresponds with the number of catamenial effusions the individual had experienced. It has occasionally happened also that at the post-mortem examination of a woman who has died during the existence of the catamenial effusion, an ovum has been found partly within the expanded extremity of the fallopian tube, and partly detached from the ovary, whilst the ovary itself has exhibited signs of congestion, and that portion of its membrane which surrounded the ovum has borne traces of recent rupture. Some fourteen months since, this condition of the uterine appendages was witnessed by myself and Mr. Lovegrove, in the left ovary of a young woman (who died of congestion of the brain on the first day of the catamenial effusion)—in whom the hymen was perfect, and whose ovaries exhibited six or seven cicatrices; the same circumstances have been witnessed by my late colleague, Dr. Richmond, of Fenchurch street, as well as by many other individuals." (pp. 157-9.)

We have quoted the above for those who have the opportunities of so doing, to examine its correctness or the contrary. We have never seen the above-mentioned cicatrices,—possibly because we have never looked for them. This theory of Mr. Jones is an extension of that of Dr. Power, as quoted by Dr. Churchill, "that a woman menstruates because she does not conceive; that certain changes take place in the ovarian vesicles preparatory to the transmission of the ovum, and that parallel changes may take place in the uterus which may issue in the formation of the decidua;" but that, "if the stimulus of impregnation be denied, this increased action is not carried to a sufficient height to produce properly that effect; nevertheless, it is sufficient to give rise to the effusion of a fluid, *which fluid is the menstrual fluid.*"

Art. II. — *The Unity of Disease analytically and synthetically proved: with Facts and Cases subversive of the received Practice of Physic.* By Samuel Dickson, m.d.—*London,* 1839, pp. 200.

We noticed in a previous number (*Brit. and For. Med. Rev.* vol. III. p. 469,) a remarkably silly book, of which the volume before us is an amplification. Our remarks, it appears, have been read by Dr. Dickson. Those which he regards as the most vulnerable he very feebly attacks; others, he wisely allows to pass without comment. We should have taken no notice of the present production but that when such pseudo-regenerators of medical science repeatedly attempt to puff themselves into an unworthy notoriety we think it right to abate somewhat the self-glorification of such pompous gentlemen. Did we believe that to ascertain truth was their object, we should have the additional desire to afford them a light upon the exceedingly dark path which they are taking in pursuit of it. For the doctrine of disease propounded by Dr. Dickson, and for its refutation, we refer to our former article, merely observing on this occasion, that whatever is new in the said doctrine is as silly as if the identity of diseases were attempted to be shown from the fact that mankind are always more or less wet, more or less heavy, more or less anything which may be said of the entire organism. Assuming for one moment that the author may wish to attain that important something which is said to lie at the bottom of a well—a very deep well, we fear, it must prove to his centripetal powers—we should recommend to his careful consideration the following advice of a man much used to close and continued reflection: "Whether you are reflecting with yourself or reasoning with another, make it a rule to ask yourself the precise meaning of the word on which the point in question appears to turn. By this means, and scarcely without it, you will at length acquire a facility in detecting the *quid pro quo.*" Let him then take the words "cause," "health," "fever," and subject them to rigid definitions, reading afterwards the article already referred to in the Third Volume of this Journal. Having done these things in a proper spirit, we feel satisfied that, even if his great dedicatee, "*William Lord Viscount Melbourne,*" should demand it, Dr. Dickson will not venture to publish a second edition.

That such of our readers as have not seen either our previous notice or Dr. Dickson's book, may not think our present commentary extravagant, we shall quote the author's own exposition of his doctrine in his concluding remarks.

> "We have proved, we hope, to the satisfaction of all but the prejudiced and the interested—
>
> "1. That the phenomena of perfect health consist in a regular series of alternate actions—each embracing a special portion of time.
>
> "2. That disease, under all its modifications, is a simple exaggeration or diminution of the same actions; and being universally alternative with a comparative state of health, strictly speaking, resolves itself into fever, remittent or intermittent, chronic or acute; every kind of structural lesion or disorganization, from the caries of a tooth, to the plumonary decomposition of phthisis, and that state of knee which is termed white swelling, being merely developments in its course (Tooth-consumption,—Lung-consumption,—Knee-consumption.)"
>
> "3. That the tendency to disorganization, usually denominated acute or inflammatory, differs from the chronic or scrofulous in the mere amount of temperature and action: the former being more remarkably characterized by excess of both, and consequently exhibiting a more rapid progress to decomposition or cure; while the latter approaches its respective terminations, by more subdued, and consequently slower and less obvious alternations of the same action and temperature. The slow and rapid caries of a tooth vary [varies] in nothing, from the chronic and "galloping" consumptions, except in the difference of tissue involved, and the degree of danger to life, arising out of the nature of the respective offices of each."
>
> "Disease, thus simplified, will be found to be amenable to a principle of treatment equally simple. Partaking of the nature of ague, throughout all its modifications, it will be best met by a practice in accordance with the proper treatment of this." (pp. 188-9.)

ART. III.—*First Annual Report of the Registrar-General of Births. Deaths, and Marriages, in England.*—*London,* 1839. 8vo, pp. 168.

WE regret exceedingly that we are unable to notice this most important document in our present Number with that minuteness of detail which it requires and deserves. We hope to do this in our next publication; but we cannot allow the present to come before our readers unaccompanied with the expression of our opinion as to the admirable manner in which the Report has been drawn up, and our earnest recommendation of its contents to every member of the profession. We think no one can read it without being strongly impressed with the vast advantages to medical science and to human happiness which may confidently be expected from the Registration Act of which this Report is the first-fruits: and we are quite sure that no right-minded member of the profession can hesitate for a moment to promote the successful working of this Act by giving every facility to the furtherance of its characteristic and most important feature, the registration of the exact causes of death. By the publication of this Report, the registrar-general (Mr. Lister) has demonstrated his peculiar fitness for the high office which he fills. He seems duly impressed with the importance of its duties, and displays a comprehensiveness of view, a clearness of method, and an industrious zeal, which cannot fail to lead to great results. In no point, however, does he appear to us to have shown more judgment than in the appointment of Mr. Farr to draw up the medical portion of the Report. His "Letter to the Registrar-General, with Abstracts of the recorded causes of 141,607 Deaths," is such as the profession were prepared to expect from the acknowledged preeminence of that gentleman in the science of vital statistics.

Art. IV.—*Human Physiology, for the Use of Elementary Schools.* By C. A. Lee, m.d. late Professor of Materia Medica in the University of New York. *Second Edition.—New York,* 1839. Post 8vo, pp. 336.

In our January Number we noticed a small work by Dr. Hayward, of Boston, written on somewhat the same plan as the present, and with the same praiseworthy view of rendering the science of physiology intelligible to general students. Dr. Lee's little volume, as will be seen by its title, goes even a step lower, and professes to instruct children, the boys and girls of elementary schools. We refer to our former notice for our views on this important subject; but, as we think the thing cannot be too strongly impressed upon the mind, we here repeat our conviction of the expediency, we may say the necessity, of having this branch of study introduced to our British schools. It would appear from the volumes of Dr. Hayward and Dr. Lee, that our American brethren have taken the lead of us in this as in many other things. The general plan of Dr. Lee's little book is excellent; the descriptions being in general clear and simple—in some cases we think not sufficiently explicit,—and every difficulty smoothed by copious illustration from woodcuts. These woodcuts, by the way, are, like all those in American books we have yet seen, stiff and harsh, and very inferior to the present state of the art in England. As we observe that Dr. Lee is about to publish a *New Medical Flora of North America,* illustrated by woodcuts—a work which is much needed and which could not be in better hands—we beg to call his attention to this subject of wood-engraving, in order that the excellencies of the author may not be impaired by the demerits of the artist.

ART. V.—*Six Mois de Séjour en Angleterre, pendant l'année* 1839. Par SIRUS PIRONDI, D.M. *Marseille*, 1839.

Six Months' Residence in England in 1836. By SIRUS PIRONDI, M.D. —*Marseilles*, 1839, 8vo. pp. 435.

As it is always interesting, and often useful, to know what our brethren of other nations think of us and our institutions, we were induced to look into Dr. Pirondi's volume, in hopes of finding something in the journal of a physician that might be acceptable to our readers. In this we have been rather disappointed, as the author has, for the most part, reserved his medical notes with the view of making use of them in another work which he intends writing. He has, however, dropped a few observations, here and there, on medical matters, some of which we may notice. As Englishmen we may, indeed, be said to owe this courtesy to Dr. Pirondi, for surely he gives a most favorable report of us. He is full of admiration of almost all he saw, and overflowing with gratitude for the manner in which he was treated. Like most travellers who pay a mere flying visit to a foreign country, he falls into various errors and misconceptions, some rather ludicrous, particularly where he sets down some particular fact or incident as a general truth or custom. Thus he tells us that the highland regiments "are *commonly* preceded by a fine deer, which obeys the orders of the commanding officer" (p. 264);—that the young men in Oxford "study chiefly Greek and Latin,—*but especially the Bible*" (p. 131);—that the Zoological Gardens are only accessible to the "bearers of tickets from members of parliament" (p. 29);—that the fashion in Scotland is to drink their toasts after dinner "with one foot on the chair, and the other on the table," (un pied sur la chaise et l'autre sur la table, c'est ainsi qu'ils crient *hurra!*") (p. 263);—that Portobello *and Leith* are two pretty villages on the shore near Edinburgh" (p. 252); and, finally, that the very trees in England partake of the precise and formal character of the people! ("Je dirai même, sans crainte d'être démenti, que ces mêmes arbres diffèrent essentiellement de ceux du continent. Depuis leur tronc jusqu' à leurs ramifications on y voit une coupe particulière; ils tiennent encore à cette symétrie générale qu'on distingue ici partout; ils ne veulent pas détruire l'ordre grammaticale: ce sont, en un mot, des arbres Anglais.") (p. 66.) Lest, however, our Transtuedan readers should be wroth with our traveller on the score of "le toast des hommes après le diner," we must not omit to state that, while he pronounces "the hospitality, kindness, and honour" (loyauté) of the Scotch to be at least equal to the same qualities of the English; he declares "their manners to be more polished, and their ladies matchless!" (Les formes extérieurs me paraissent pourtout plus polies en Ecosse, où les femmes surtout sont d'une amabilité extrême.) (p. 262.)

Dr. Pirondi speaks most favorably of the general state and administration of our hospitals; but he complains of the patients in Guy's Hospital ("which is in itself a perfect piece of grammatical logic, never putting the adjective before the verb,") (p. 43,) being too well fed, "from the maniacal fear of seeing patients die of hunger." (p. 317.) Unless certain travellers are greatly belied, this "maniacal fear" by no means disgraces the administration of some of the hospitals in Dr. Pirondi's country.

Like all foreigners, the author is amazed at the state of organization, or rather disorganization, of the profession in this country. He confounds, it is true, the granting of degrees and licences in surgery and pharmacy with the graduation of physicians, stating that no titles can be obtained but at the great universities; and informs us that at the time he is writing "there is building in London a magnificent university" to grant all sorts of degrees; he speaks a sad and disgraceful truth, however, when he tells his countrymen that, "by a singular absurdity, no graduate can practise his art in the capital without being previously examined by one of its colleges;" and that thus "London arrogates the right of testing the medical knowledge of a doctor of medicine, although it has no power of conferring the degree." (p. 120.)

We will conclude this notice with Dr. Pirondi's account of the mode of carrying on the business at our medical societies in London, and will add an anecdote of Sir Astley Cooper, the characteristic truth of which, we are sure, no one who knows that great surgeon will call in question.

> "The medical societies generally meet in the evening, and the members always arrive half an hour or more before the beginning of the business, and often take tea in an adjoining room. The president, generally armed with a small ivory hammer, opens the meeting and states the order of proceeding. The member whose turn it is rises immediately, and *reads* his paper, in the midst of profound silence, and, when he has finished, sits down. Frequently three or four minutes elapse before any one thinks of replying; but then some one gets up and delivers his sentiments. But you must not fancy that he turns to the first speaker, or rather reader: by no means—his whole discourse is addressed to the president. When he has finished his speech he seats himself, awaiting a reply. After another lapse of four or five minutes, the original orator rises and replies to the last speaker—but without ever looking at him—M. le President is the constant object of his address. In other words, there are always two advocates who seem to plead their respective causes before a judge from whom there is no appeal! This discussion at an end, silence resumes her sway once more for some minutes; but you must not from that imagine that the meeting is over: far from it—it will last two or three hours longer. One may perhaps say (adds our worthy Doctor) that this is taking things rather too coolly; but to this, again, it may be answered that there can never be too much calmness in discussion." (p. 428.)
>
> "After the death of Dupuytren and Scarpa, medical Europe venerates in Sir Astley Cooper the first of existing surgeons. Tall, healthy-looking, robust, and broad-shouldered, he possesses one of those happy constitutions which easily supports all the labours which a superior genius can impose on it. . . . He was kind enough to make us acquainted with his researches on 'the Structure and Functions of the Thymus Gland,' with which he was then occupied; and I am the more pleased to recall this circumstance, because it enables me to record a reply of Sir Astley's, which proves delightfully the perfect truth and honesty with which he conducts his researches and experiments. While he was pointing out to us, on a most delicate preparation, the two membranes which he has found in what he calls the reservoirs of the thymus, I said to him, '*You said, and it is.*' '*No!*' he replied, '*it is, and I said.*' The scientific character of the great English surgeon breathes in this response." (pp. 320-1.)

We doubt not Sir Astley is one of those English savans "whose distinctive character," he says in another place, "is to unite a genius entirely French with a patience truly German."

ART. VI.—*The Surgeon's Vade Mecum: a Hand-book of the Principles and Practice of Surgery. Illustrated with numerous Wood Engravings.* By ROBERT DRUITT, M.R.C.S. *London,* 1839. Post 8vo, pp. 429.

SUCH of our readers as recollect the fierce onslaught we made on the writers of guide books, in our Eleventh Number, (*Brit. and Foreign Med. Review,* vol. VI. p. 202,) need not be informed how jealous we are of everything that is calculated to draw our younger brothers from the straight and narrow path of legitimate study to the royal road—the *facilis descensus Averni* of the grinder. One of the works there condemned was by the author of the present; but we had no fault to find with it, except on the general principle. The small volume before us has somewhat higher aims, being intended not so much as a help for the student as a prompter to the practitioner. It contains "a familiar account of the nature and treatment of the various diseases that are commonly assigned to the surgeon's care;" and will, no doubt, be found useful to those whose knowledge is not always ready at command. The author shows himself well acquainted with the most approved views both of pathology and practice; and the general arrangement of the subjects and the style in which the information is conveyed are particularly good. We have noticed some errors and more omissions; but, on the whole, the work is very creditable to the author, and we recommend it to the advanced pupil and to the young surgeon as a useful index to the great book of surgical knowledge.

ART. VII.—1. *Essay on Cruelty to Animals.* By JAMES MACAULAY, M.A. —*Edinburgh,* 1839. 8vo, pp. 135.

2. *The Animals' Friend; or, the Progress of Humanity.* Published periodically, for the Animals' Friend Society. No. VII.—*London,* 1839. 8vo, pp. 72.

WE are induced to call the attention of our readers to Mr. Macaulay's unpretending volume, from the fact that, although addressed to the public at large, it presents many subjects for serious reflection to the professional student. The author was formerly in the medical profession: and the calmness and ability with which he states his views show that he was properly qualified for the task. To this essay the Theological Faculty of Edinburgh have already awarded a prize; and we believe that much good will be done by its diffusion. It would be foreign to our purpose to give an abstract of this work. The whole subject is treated in a very systematic manner, and the conclusions are well supported by sound reasoning.

A considerable portion of the volume is devoted to an analysis of the supposed benefits which have been conferred on science by experiments on living animals. While Mr. Macaulay treats this part of the subject without prejudice, we think he bestows no more than a just censure upon many physiologists who have experimented on animals, not so much to add to the facts of science as to support their own peculiar doctrines. Magendie's experiments in France, and those of Dr. W. Phillip in this country, have very much this character. The one comes to the conclusion that the nerves are nothing more than electric conductors, and that the stomach digests food by a galvanic power: the other, that the stomach is of no use in vomiting, since, when it was cut away, and a pig's bladder substituted, there was no difference in the effects! It would be a curious point for enquiry, as to how many animals these two physiologists have destroyed, to acquire opinions which are now only retained by themselves. In a preceding article in the present Number, we have shown the inconclusiveness of many of Magendie's experiments on living animals, in relation to the properties of the blood. The author, who is entitled to speak from his personal acquaintance with that physiologist, fully confirms the opinion which we have expressed.

> "M. Magendie himself, whose name and reputation are so identified with our subject, is not more successful than any of his colleagues in his hospital practice, notwithstanding all the light which he conceives he throws upon the nature of disease by his dissections. His mind is, indeed, counteracted by prejudices, derived from his peculiar pursuits, so that in a coarse and illiberal manner, he affects to despise and ridicule the researches of pathologists; and he is constantly falling into errors from rash generalizations, founded upon observations of the animal economy in unnatural states." (p. 91.)

We agree with the author, that the severest experiments are justifiable, when they are undertaken with a view to ascertain any definite and important point from which improvement in practice may reasonably be expected to result, but under no other circumstances whatever. We commend this volume to those of our readers who take an interest in the subject.

The second publication on our list speaks for itself. Its object and design are most praiseworthy, and we doubt not that it has already done much good. We are happy to make known to our readers the benevolent society from which it emanates. It would be strange if the professors of the medical art, whose special object is to relieve the pains of man, should be insensible to the sufferings of his inferior brethren.

ART. VIII.—*On the Enlisting, Discharging, and Pensioning of Soldiers, with the Official Documents on these branches of Military Duty.* By HENRY MARSHALL, F.R.S.E., Deputy Inspector-General of Army Hospitals. *Second Edition.—Edinburgh,* 1839. 8vo, pp. 259.

THIS is a most valuable book, and ought to be in the library of every medical officer in the public service, whether of the army or navy. "In a financial, a political, and, perhaps, I may add, in a medical point of view, I am not aware," says the author, "of any part of the duty of a medical officer which is of more importance than the inspection of recruits on a large scale and the examination of inefficient soldiers; and consequently these duties deserve a very careful consideration." We can add, that in Mr. Marshall's pages will be found the completest and most efficient aid for enabling the officer to discharge this duty with accuracy. Under the head of "Diseases and Disabilities, which disqualify Soldiers for service in the Army," is given a comprehensive and interesting view of the feigned and factitious diseases of the class of men termed *malingerers* in the army and *skulkers* in the navy, which we would recommend not merely to the notice of officers in the public service, but to the medical staff of our civil hospitals and workhouses.

Art. IX.—*School Botany; or an Explanation of the Characters and Differences of the principal Natural Classes and Orders of Plants belonging to the Flora of Europe, in the Botanical Classification of De Candolle.* By John Lindley, ph.d. f.r.s., Professor of Botany in University College.—*London*, 1839. 8vo, pp. 218.

This little work is expressly composed "for the use of students preparing for their matriculation examination in the University of London," but it is admirably adapted for botanical students of all kinds. It is, in fact, a compendious introduction to botany, on the natural system, and is such as might have been expected from the author's great knowledge and long experience as a teacher. We recommend it to every beginner in the study of the natural system of botany, whether young or old, as the best guide in his early progress. The style is simple and clear; and every difficulty in the descriptions is lessened by numerous woodcuts.

Art. X.—*The Surgical Anatomy of the Arteries, and Descriptive Anatomy of the Heart; together with the Physiology of the Circulation in Man and Inferior Animals.* By Valentine Flood, a.m., m.d., t.c.d. &c.; and Lecturer on Anatomy and Surgery in the North London School of Medicine.—*London*, 1839. 12mo, pp. 237.

The present work, although consisting of but one volume, and that not a very thick one, is characterized by being more comprehensive in its reach than any of its predecessors on the same subject, in the class of *manuals;* indeed, whatever fault we might be disposed to find with its author, we cannot accuse him of having begun in the middle of his subject. The introduction, which extends through some sixty pages, presents us first with Cuvier's classification of animals; and the circulation in each class is considered, commencing from the zoophytes, and proceeding upwards in the scale till we arrive at mammalia. The circulation in man being thus ushered in, the physiology and development of the propelling and conducting organs of the blood are then discussed. The descriptive anatomy of the heart succeeds, and prepares us for the next and grand division of the work, "the Descriptive Anatomy of the Arteries." Considerable care and attention seem to have been bestowed on the compilation of the different divisions which compose this work, particularly in collecting the best information relating to varieties in the course of important arteries; and the more momentous operations on these vessels are illustrated by a selection of the most remarkable cases: the opinions of able operators have been carefully consulted, and are judiciously commented on. The production is altogether very creditable to its author. We are not, however, disposed to believe that the present volume will supersede the excellent work upon the same subject by Dr. Harrison, whose descriptions are, as we well recollect his vivâ voce teaching to have been, most lucid and satisfactory. Dr. Flood's work, however, comprises much which Dr. Harrison's does not profess to contain; and of necessity, as its bulk is greater, the descriptive department is treated of more at large by the latter author. The distinction of type, adopted by Dr. Flood, for those parts of the text which are necessary whilst dissecting, and such as may be left for after-perusal, will obviate the confusion which might otherwise distract the attention of the student whilst simply tracing the course of an artery.

Art. XI.—*The American Medical Library and Intelligencer.* Published Semi-Monthly. Second year: from April 1838 to April 1839. Edited by Robley Dunglison, M.D., Professor of the Institutes of Medicine in Jefferson Medical College, &c.—*Philadelphia*, 1838-1839. 8vo.

We noticed this publication on its first appearance, and refer for its general plan to our Fourth Vol., p. 495. It has proceeded since that time with uninterrupted regularity, and every part bears evidence of the learning, industry, and sound discretion of the editor. Although, as we said on the former occasion, this plan of wholesale republication cannot fail to be injurious to the pecuniary interests of the authors and proprietors of medical works in this country, there is still some compensation afforded to those who write for fame or for the good of their fellow-creatures, in the ready and speedy diffusion of their works, by means of this "Library," throughout the vast country in which it circulates. The following are the works reprinted during the last twelve months, and which (including *The Intelligencer*, forming a large volume in itself,) are sold for ten dollars—about one sixth of the cost of the separate volumes as imported!—Dr. Kramer, on Diseases of the Ear; Dr. Hamilton's Practical Observations on Midwifery; Mr. Syme, on Diseases of the Rectum; Dr. Osborne, on Dropsical Diseases; Dr. Green, on Diseases of the Skin; Mr. Coulson, on Diseases of the Bladder; Mr. Liston's Practical Surgery (with Notes by Dr. Norris); Mr. Morgan, on Inflammation; Dr. Granville, on Counter-Irritation; Dr. Hodgkin, on the Morbid Anatomy of Membranes; Mr. Ryland, on Diseases of the Larynx and Trachea; Dr. Rowland, on Neuralgia; Dr. Dunglison, on the condition of the Insane Poor; Dr. Churchill, on the Diseases of Females; Prof. Lallemand, on Seminal Discharges (translated); Dr. Clendinning's Croonian Lectures; Sir A. Cooper, on Spermatocele.

Henceforth English medical authors will not have to complain that they are not read by the profession in America, at least. We hope that this ready, easily-accessible, and never-failing supply from a foreign source will not tend to dry up or to let stagnate the rich well-spring of native talent in America. If such should prove to be, in any respect, the case, we think, even at the moderate cost of "ten dollars a year," our Transatlantic brethren would—in the language of their greatest citizen—"pay much too dear for their whistle."

Art. XII.—*An Experimental Inquiry concerning the presence of Alcohol in the Ventricles of the Brain, after Poisoning with that liquid; together with Experiments, illustrative of the Physiological Action of Alcohol.* By John Percy, M.D.—*London*, 1839. 8vo, pp. 112.

We have already had occasion to notice, and with much commendation, more than one thesis, to which a gold medal was awarded by the medical faculty of the University of Edinburgh. The thesis before us is also a prize essay, by a graduate of the same University. Its chief value consists in the experiments, by which the question respecting the presence of alcohol in the brain, after poisoning with that fluid, is determined.

The general opinion has been—although the evidence on which such opinion rests is not extant—that in such cases alcohol might be found in the brain and ventricles after death. Dr. Percy has shown, beyond a doubt, that alcohol is taken up into the system; he having obtained it, by a process to be described, from the brains of dogs poisoned by it. But none of his experiments have enabled him to determine whether the fluid of the ventricles, under such circumstances, contains any alcohol. It would rather appear that there was some peculiar affinity between the substance of the brain and the spirit, especially as, after analyzing a much larger quantity of blood than can possibly exist in the cranium, he could generally obtain much more alcohol from the brain than from this quantity of blood. In his experiments, Dr. Percy also detected alcohol in the blood, urine, bile, and liver; and he connects the last fact with the frequency of hepatic disease in drunkards.

The following is the principle of the method adopted for obtaining the spirit from any fluid or organ to be examined. For a minute description we must refer to the work itself. The spirit is separated from the solid or fluid supposed to contain it, by distillation with water. The spirit is separated from the fluid, which distils over, by means of subcarbonate of potass; and the mode of testing this spirit (always small in quantity) is by its inflammability and its power of dissolving camphor. There is no doubt of the correctness of Dr. Percy's tests, nor any appearance of fallacy in his experiments or the conclusions which he draws from them. The essay contains the minute account of many experiments, the subjects being mostly dogs; and the spirit being injected into the stomach, jugular veins, or carotid arteries. The conclusions to which Dr. Percy's experiments have led him, respecting the modus operandi of alcohol are not of an exclusive character. He alludes to the discrepant opinions of the various writers on the action of poisons; and, in reply to that which maintains that the action of poisons depends exclusively on an impression upon the extremities of the nerves, he says

> "We should certainly expect that this cerebral derangement would, in every instance, almost instantaneously succeed the exhibition of alcohol, especially when in large quantity, and in a concentrated form. On a careful review, however, of the experiments detailed, it will be observed, that generally an interval of several minutes elapsed before the slightest manifestation of cerebral derangement was afforded. Hence it is inferred that in some, or rather in the greater number of cases, absorption is required for the development of the narcotic effects of alcohol. . . . . In some of the experiments, on the other hand, total loss of sensibility and voluntary fever so instantaneously followed the introduction of the poison into the stomach, that we cannot conceive that absorption to a sufficient extent could possibly have been instantaneously effected. Hence there can be little doubt that alcohol can produce its fatal effect without being absorbed." (pp. 108-9.)

In these cases we must suppose the action to be upon the extremities of nerves. The following remarks are worthy of attention, as applicable to the modus operandi of alcohol.

> "It has been objected, that the circumstance of intoxication being in some instances almost instantly abated by vomiting, is incompatible with the idea that alcohol may act by absorption directly upon the 'central organ of the nervous system.' But independently of the fact that alcohol may be detected in the brain and blood, I may answer, *first*, that in dogs I have never (although I have watched with great attention) seen such immediate abatement, even after repeated and violent vomiting, as

> generally represented to take place in the human subject; *secondly*, that, even admitting the correctness of the observation upon which the objection is founded, the very act of vomiting may probably be a sufficient stimulus to account for the subsequent relief; and this is supported by the current opinion, that vomiting is much more efficacious than complete evacuation by the stomach-pump; *thirdly*, that I recently witnessed a case of profound intoxication in a man, in which complete evacuation by the stomach-pump was not attended by any immediate abatement of the symptoms; indeed, the intoxication suffered no diminution for two or three hours afterwards, during which time the breath continued strongly alcoholic." (pp. 111-12.)

The experiments in Dr. Percy's essay detail with minuteness the effects produced upon animals by the presence of alcohol in their system, from the time of its introduction until death; and the appearances after death are likewise described. An investigation, conducted with similar care to that which we have just described, in which various individual substances should be made the object of experiment seems to be an object worthy of consideration, as likely to lead us somewhat farther onwards in the obscure path of the action of medicines. It is something for which we have to thank Dr. Percy, to have ascertained beyond a doubt the presence of alcohol in the brain; it is something more to have had it shown as a matter of high probability, that this spirituous fluid has apparently a considerable affinity for the cerebral substance. We know that medicinal substances have been detected in the blood, and that the action of many is more directed upon particular organs than others. How far this fact may be explained, on the supposition of a particular affinity of the medicine for the organ, is a subject well worthy of enquiry, and one which we should recommend Dr. Percy, with his aptitude for experimental investigation, carefully to perform.

Art. XIII.—*Illustrations of Cutaneous Diseases.* By Robert Willis, M.D. Fasciculi IV, V, VI, VII, VIII.—*London*, 1839. Folio.

In our last Number we gave a brief notice of the first fasciculi of this work, and spoke somewhat disparagingly of them, as artistical productions. We condemned, also, in some respects, the meager plan adopted by the author, as scarcely allowing him to do justice either to his subject or to his own knowledge. We were fully aware that Dr. Willis's experience and great ability well qualified him for the task he had undertaken, and regretted that he had allowed himself to be hampered by a faulty plan, or a too thrifty publisher. The general defects of the plan still exist; but we are happy to observe that the character of the drawings, and still more of the colouring (so essential a point in all pathological illustrations) is considerably improved in the fasciculi now before us, more particularly in these last published. We observe, also, in Dr. Willis's brief annotations, so many indications of a good practical knowledge of his subject, that we have more cause than ever to regret the limitation imposed upon him by the original plan. We once more recommend this work to all our readers who feel the want of a guide through the obscure labyrinth of cutaneous diseases.

ART. XIV.—1. *A Series of Anatomical Sketches and Diagrams, with Descriptions and References.* By THOMAS WORMALD, Assistant-Surgeon and Demonstrator of Anatomy at St. Bartholomew's Hospital; and ANDREW M. M'WHINNIE, Teacher of Practical Anatomy at St. Bartholomew's Hospital. Part II. *London*, 1839.

2. *The Surgical Anatomy of the Groin, the Femoral and Popliteal Regions.* By THOMAS MORTON, formerly one of the House-Surgeons of University College Hospital. *Illustrated with Lithographic Plates and Wood-Engravings.—London*, 1839. Roy. 8vo, pp. 207.

WE have already had occasion to notice the preceding parts of both of the above works in recent Numbers of our Journal, (Vol. VI. p. 202, and Vol. VIII. p. 244.) The present subjects are equally well treated by their respective authors. The "Sketches" are particularly well executed; and we cannot but again express our approbation of the style which Messrs. Wormald and M'Whinnie have selected for their illustrations: their outlines are accurately and elegantly executed, and, to our taste, are far more satisfactory than the most highly-finished engravings would be. Mr. Morton's *illustrations* are drawn with more freedom than those which adorned his former number on the Perineum; but they strike us as being almost too much finished: a more sketchy style and rather less paint would save trouble, whilst the desired result would be more effectively obtained. Much care has been bestowed by Mr. Morton in collecting information relating to herniæ, popliteal aneurism, dislocations of the thigh, &c., from the most approved authorities; and the production is altogether one which we can conscientiously recommend to the *working* student. We observe on the fly-leaf that Mr. Morton promises two more parts to complete his work;—whilst Messrs. Wormald and M'Whinnie intend extending their sketches to six numbers: each work will, when entire, comprise "views of those regions of the body most important to the student and surgeon,"—while Mr. Morton's will constitute a complete and elaborate treatise that cannot fail to be highly useful to surgeons in general. It speaks highly for his school, and still more highly for the writer, when a work of this kind is the production of one who is almost yet a student.

ART. XV.—*Observations on the Disorder of the general Health of Females called Chlorosis, showing the true cause to be entirely independent of Peculiarities of Sex.* By SAMUEL FOX, Surgeon.—*London*, 1839. 8vo, pp. 130.

WE are somewhat disposed to publish a work to prove that a "chronic functional derangement of the liver" is the grand cause of all the indifferent or bad books which, as reviewers of medical literature, it is our fate to peruse. Our theory would be as follows. Individuals whose brains are, by temperament, inactive, are desirous of seeing their thoughts in actual print. They labour hard to think, and their cerebral powers are tried to the uttermost. Attention, memory, judgment, imagination—all diminish with the cerebral exhaustion. The direct nervous sympathy of the liver with the brain, or the indirect sympathy through the stomach, produces a languid action in that large and important viscus. The whole process of digestion is impaired, and consequently the blood which is required to keep up the energy of the brain is no longer of the requisite quality. The brain is thus, indirectly as well as directly, unfitted for its functions, and when the author attempts to put his thoughts into language, the faculties which had begun by diminishing are found to have utterly failed. He writes a bad book, and "chronic functional derangement of the liver" is the cause. Now the symptoms by which we judge of this derangement are headach, sleeplessness, exhaustion, loss of appetite, slow bowels, unhealthy stools, a feebleness and perhaps irregularity of circulation, &c.

This is a specimen of the kind of argumentation in the book before us; the object of which is, to prove, among other curiosities, that chronic functional derangement of the liver is chlorosis, and *vice versâ*. The absurdity of such reasoning may possibly strike the author in our theory, though it may escape him with regard to his own; for, unfortunately, the intellectual condition which permits an individual seriously to publish such an argument, unfits him for ascertaining its fallacy. In the present day, we can hardly think it worth while seriously to criticise such a book as the one before us; but, as the world is not yet quite freed of its "liver-doctors," and that Mr. Fox may not complain that we have not fairly treated him, we will take the trouble to expose his theory to his own view, apologising to our readers for thus occupying their valuable time:

> "In describing," says Mr. Fox, "the symptoms of this chronic functional disorder of the liver, which I conceive to be the primary cause of these distressing ailments, (*i. e.* chlorosis), I must necessarily confine myself to those which are considered its leading, or more prominent features, while it would be an endless task to enumerate a multitude of sympathetic affections resulting from this disorder, which for the most part depend upon idiosyncracy; the greater number, however, of the symptoms, vary considerably as the disorder advances. At the commencement of this complaint there is generally a morbidly craving appetite, with but little thirst and fever; and, before there is much apparent loss of flesh, even whilst the appetite is verging on voracity, there is yet considerable debility, or rather an inaptitude for bodily exertion; though sometimes, for short periods, the patient is capable of using considerable exertion, owing, apparently, to a temporary mental stimulus. The disease insidiously gains ground, and the only alteration at first noticed by friends and relations, is the possession of less than usual animation, gradual loss of colour and complexion; sluggishness, (too often called laziness, for which the unfortunate patient is upbraided,) strong inclination for sleep, and great disinclination to rise in the morning; the patient throwing aside her usual amusements and occupations, becomes soon tired with slight exertions; frequently complaining of pains in her limbs. After a time, in many cases, there seems to be some degree of imbecility of intellect, as indicated by crying from slight causes, absence of mind, general inattention to surrounding objects, &c. Fetid breath, faintness, nightly perspirations, disturbed sleep, irritable stomach and nausea, are affections to be met with in almost every case of chronic functional derangement of the liver. The tongue is generally loaded, especially at its posterior part; sometimes, however, white, and sometimes particularly clean; the bowels are irregular, but when either loose or costive, the fecal evacuations are always unhealthy. The loss of strength and flesh now becomes real, and the body and legs are frequently swelled, whilst the latter, particularly about the ankles, are œdematous. By this time a vitiated appetite succeeds the former craving for food, and even when food is taken, it is not unfrequently ejected from the stomach. There is great depression of

> spirits, clammy perspiration about the face and breast, and especially about the nose and mouth; the complexion is now sallow, the eyes sunken, with dark areolæ formed around them, and the eye itself loses its wonted lustre. There is no discharge of the catamenia." (pp. 73-5.)

Before drawing Mr. Fox's serious attention to the foregoing extract from his volume, we may assure him that we have no particular theory respecting the nature of chlorosis to support, and that our object is simply to expose what appears to us to be obviously not merely incorrect, but absurd. We have little doubt that Mr. Fox, during his long practice, "extending over a period of between thirty and forty years," has found a large proportion of his patients very much satisfied to learn that their complaints are "bilious," and that he has likewise found it an effectual mode of escaping from difficulties of diagnosis, to ascribe symptoms connected with the nutritive, circulatory, nervous, or other systems, to sympathies with the liver. But we ask him, in considering with us the previous quotation, to endeavour to lay aside all preconceptions, to consider the subject, if possible, *de novo.* Among the early symptoms of this supposed functional liver affection is a "morbidly craving appetite," "but little thirst or pain," "debility, or rather inaptitude for bodily exertion;" "gradual loss of colour and healthy complexion," "sluggishness, strong inclination for sleep," "pains about the limbs," "imbecility of intellect," &c. On what sort of *evidence* does it rest that any one of the above symptoms arises from disordered hepatic function? We see distinctly a debilitated and depraved condition of the functions of the nervous and nutritive systems, but not a single symptom which we can satisfactorily associate with the liver. It may be true, as stated by Mr. Fox, that by some these are considered as the leading or most prominent features of chronic functional affection of the liver; but, by others, they are otherwise regarded, and we wish for the best demonstration of which the case will admit in deciding who are right. Sure we are that there is quite as much *proof* that the moon is made of green cheese, as that the above symptoms depend on chronic disorder of the liver. We consider it unnecessary to specify the other symptoms noticed by Mr. Fox, and which we have extracted. They may be variously explained; but we can find none among them, unless it be the characters of the fecal discharges, which cannot be quite as easily and rationally connected with other organs as with the liver. Having added a few more symptoms to those already mentioned, the author continues:

> ‘This functional disorder of the biliary apparatus, I consider to be identical with the disordered state of health called chlorosis; and if we compare the symptoms of chlorosis, as described by Drs. Cullen, Mason Good, and others, with those above detailed, we shall find but little difficulty in assuming them as one and the same sort of symptoms." (p. 76.)

Correct pathology and common sense leave no other conclusion than that these notions of Mr. Fox are nonsense. We are sorry to pass so harsh a sentence on a work proceeding from a gentleman, no doubt, of great respectability, and who is, probably, even a successful practitioner; but Mr. Fox has evidently stepped beyond his line in turning author, and we counsel him to eschew all such follies for the future.

Art. XVI.—*The Collected Works of Sir Humphry Davy, Bart.* LL.D. F.R.S. Edited by his brother, John Davy, M.D. F.R.S. Vol. I. *Memoirs of his Life.—London.* 1839. 8vo, pp. 475.

The warm affection of a brother has guided the pen of the author throughout this volume. Biography in such circumstances necessarily becomes eulogium. For every virtue of Davy, and for his immortal services to science, the gratitude and esteem of the public are not less than the admiration of his affectionate relative; and if in this history of a great man by one who had just reason to be proud of him, fraternal partiality has sometimes given importance to trifles, and sometimes found excuses for foibles with which others only were offended, it is not for us to ascribe it as a fault of great magnitude in Dr. John Davy. Assuredly, we think many particulars of the early life of Davy might have been omitted without loss to the reader; and we should be disposed to extend this observation to nearly all the poetical fragments, which, although they bear the impress of Davy's elevated intellect, belong to that order of verse which no true lover of imaginative poetry reads but for the thoughts, or reads more than once. But readers of the highest order of intellect, and of the most exalted imagination, may and will peruse with deep gratification the fragments, expressed in prose, and taken from Davy's note-book, in which the great philosopher's habit of advancing his thoughts from the known and proved towards regions not yet opened to the distinct apprehension of human beings, is splendidly conspicuous. In contemplating the life and thoughts of men of this high class, whose clear vision, ranging from the vantage-ground of science, extends farther than that of ordinary men, we always feel desirous to question them of their ultimate views of themselves, and of the express and admirable universe around them. Few have thought so much on these high topics, and few have dared to be so candid as Davy: none ever arrived, by the paths of philosophy, at more noble and encouraging views of the progress and destiny of mankind, and of the vast unknown from which the dark curtain is not yet raised. If for the advantage alone of becoming acquainted with these, the innermost thoughts of a reflecting and powerful mind, we would strongly recommend every student to read the life of Davy. It will, however, be found both interesting and useful on other grounds. A clear, accurate, and succinct account is given of Sir Humphry's principal discoveries and writings; and several of his letters, all animated by the same spirit of investigation and enquiry, are interspersed with the details of his progress.

In all biographical reading, the most impressive, perhaps the most instructive, result is, that we see the whole drama of life, which in each individual's own case is so expanded as seldom to be contemplated as a whole, compressed into a small and appreciable space; the hopes of youth, the triumphs of manhood, and the yielding up of all to sure decay, perhaps to decrepitude and physical degradation. The decline of Davy was gradual. As in the case of Cuvier, Scott, and other great men, (however various their greatness,) the intellect survived much of the mere bodily power; and, when the physical organs were more than ever bound to the earth, with which they were soon to be mixed, soared with even more earnest pinion towards another existence, into which they aspired to ascend. Believing his malady to be mortal, he rejected no human means of controlling or remedying it, making exertions, often of the most painful kind, to overcome the torpor of paralysis, which was but the beginning of death, and yet chiefly anxious to complete his intellectual labours. "I fight," said he, "against sickness and fate, believing I have still duties to perform, and that even my illness is connected in some way with my being made useful to my fellow-creatures." In such instances, the great gift of intellect is seen in all its value, and over it death has no victory. Like the dying gladiator (a subject consecrated alike by ancient art and modern poetry), the philosopher "consents to death, but conquers agony;" and, having combated, not for conquest, but for truth, passes through these worst humiliations of our mortal nature with a calmness, which truly gives to his final departure the air of a transition from this world of limited knowledge to a world of higher intellect, where more enlarged faculties will find a still more expanded field of exertion. Or if such thoughts be too presumptuous, it is still deeply interesting to watch the calm exit of an incomprehensible element of life, which has been exerted, in this short existence, for the never-ending benefit of all mankind.

We think the public under great obligations both to the editor and publisher for this beautiful and cheap edition of Sir Humphry Davy's complete works. It cannot fail to be patronised, as it well deserves to be, by every lover of science.

ART. XVII.—*Retrospect of the Progress of Surgical Literature for the Year* 1838-9. By Messrs. NEWNHAM, W. WICKHAM, and SALTER. Read, June 1839, before the Annual Meeting of the Southern Branch of the *Provincial Medical Association*, and published at its request. —*London*, 1839, 8vo, pp. 44.

THIS is a very excellent report, comprehending in brief space notices of all the important additions made to surgical science during the past twelve months, and illustrating each by a judicious commentary. It cannot fail to be useful to every practitioner, and more particularly to those who have not access to the recent surgical literature of France. We quote the following observations on the subject of revaccination, as well because of their intrinsic importance, coming as they do from a gentleman of great experience and accurate observation, as because they constitute a critical commentary on the views of Professor Heim, first communicated to the profession in this country in the Thirteenth Number of this Journal.

> "On the subject of revaccination, your reporters can only appeal to further experience; 'adhuc sub judice lis est.' But they would endeavour to bring their quota of observation towards the settlement of this great question. Professor Heim states the efficacy of revaccination to consist in the complete progress of the vesicle. The experience of your reporters would lead them to say that it never was complete except in the almost solitary instance, which would confirm rather than invalidate the rule. In 150 cases of revaccination which have been noted by one of your reporters, only one could be said to approximate towards a regular vaccine vesicle; and this reporter is disposed to consider revaccination in no other light than as a test of the constitutional influence of the former vaccination. In the instances which have come under his notice, the local action has been observed much earlier than it ought to be, and, in some cases, has subsided without further mischief; in others, a vesicle has been soon formed with an undefined, ragged border, surrounded from the first by an inflammatory blush, and always wanting the defined circular edge of

the genuine vesicle; much too much local action has been produced; there has been almost always swelling of the axillary glands, which is unusual in a first vaccination, unless the lymph has been too aged; there has arisen a much higher and a much earlier state of constitutional irritation, originating and ceasing at very uncertain periods, governed by the local cause, and leaving a sore arm with protracted swelling, and a flattened, undefined crust, instead of the regular, raised, well-defined chocolate-coloured scab. We are well aware that the statistical report (as well as other observations) of the result of revaccination of the Prussian army would seem to militate against these conclusions, but we believe that if tried by the above tests, a very different result would be exhibited. According to the views of your reporters, it follows either that we have no test of what is a genuine vaccine vesicle, or that this is not the progress of such vesicle. If we adopt the former proposition, vaccination may be abandoned; if the latter, it is evident that the symptoms are the result of the local agencies of a poison introduced into the constitution, but, like other specific agents, capable of producing its regular influence only once during life. Yet, if this be true, the efficacy of revaccination cannot consist in renewing its protecting influence over the constitution, but simply in forming the test referred to. Your reporters must vehemently protest against the employment of lymph taken later than the ninth day, and their experience would lead them to give the meed of incalculable superiority to that taken on the eighth. To the want of attention to this most important rule, many failures may doubtless be attributed; and also the probable origin of that unfortunate hypothesis which represents the necessity of going back to the cow for a renewal of lymph, which had become deteriorated or effete by passing through so many subjects; an hypothesis so contrary to nature, to reason, to science, and to fact, that we can only be surprised at its endurance. Notwithstanding the conflicting testimonies of the past year, your reporters would be disposed to believe that a genuine vaccine vesicle may be accelerated or retarded without impairing the security of the patient, provided that its course be otherwise regular; and they would also believe that the efficacy of vaccination in protecting its recipients from the fatal consequences of smallpox casually introduced into the system, is much greater than that of previous smallpox itself, whether natural or inoculated." (pp. 8-10.)

Art. XVIII.—*Political Medicine; being the substance of a Discourse lately delivered before the Royal College of Surgeons in Ireland, on Medicine, considered in its relations to Government and Legislation.* By H. Maunsell, m.d., one of the Professors in the Royal College of Surgeons in Ireland.—*Dublin,* 1839. 8vo, pp. 45.

We recommend this pamphlet to all our readers, as treating admirably of a subject of the very first importance, but one which has been neglected, in these kingdoms, to an extent which would be truly astonishing, were it not well known to be one of the common results of a popular government and free institutions, that the people are, for the most part, left to regulate their own affairs in the way they prefer. For our own parts, although we are as jealous of our political freedom as any one, and would much rather bear a hundredfold more of the social and economical evils, which are its legitimate offspring, than be tyrannised into better manners and healthier habits by the most benevolent of despotisms—still we do think that the government of this country might interfere more than it does in not a few matters of domestic and social life, with infinite advantage to the community, and without trenching, in any degree, on the wholesome liberty of the subject. And in no part of our social economy would such an interference be more justifiable and more beneficial than in regulating those branches of medicine which relate to the public health, or the means of preventing diseases, and of mitigating their ravages when they prevail. In this respect Great Britain is far behind almost all other European countries. The object of

Dr. Maunsell's pamphlet is to call attention to this department of medicine (termed by him *political medicine*), and of which the surpassing importance is well and clearly illustrated in its pages. In relation to his subject the author rapidly reviews the questions of vaccination, the health of the army and navy, the health of prisons, the quarantine system, epidemic and endemic diseases, unwholesome trades, lunatics and lunatic asylums, medical attendance on the poor, colonization and emigration, &c. It is quite clear that, for the due regulation of these and other similar matters, of the highest moment to the welfare of the community, there must be a general reform in the actual constitution of the medical profession generally. One necessary result of this would be the formation of a Board of Health, constructed on liberal and sound principles, to be the organ of communication with the government on all questions of medical police. We are happy to think that things are progressively, though slowly, ripening towards so desirable a consummation; and as pointing out some of the grievous defects and evils of our present system, and a few of the benefits to be expected from a better, Dr. Maunsell's little work will be highly serviceable to the great cause of medical reform; we therefore cordially recommend it to the notice of our readers.

Art. XIX.—*Medical Lexicon; a new Dictionary of Medical Science, containing a concise account of the various Subjects and Terms; with a Vocabulary of Synonymes in different Languages, and Formulæ for various Officinal and Empirical Preparations, &c. Second Edition, with numerous modifications and additions.* By Robley Dunglison, m.d., &c.—*Philadelphia*, 1839. 8vo, pp. 821.

The first edition of this work was published in 1833, in two volumes, and has ever since been the standard authority for reference with American students. It reappears in a single volume, considerably altered, and much improved. All the biographical and bibliographical articles are omitted, while a vast number of new words are added; and the scientific and medical definitions and descriptions are modified, according to the present state of knowledge or opinions. This dictionary is on the same plan as Dr. Hooper's, being not "a mere lexicon or dictionary of terms, but affording, under each, a condensed view of its various medical relations, and thus rendering the work an epitome of the existing condition of medical science." (Preface.) Being only of about half the size of Dr. Grant's edition of Hooper, its scientific and medical articles are necessarily much less perfect; but we hardly consider this as a defect, as the knowledge of systematised *facts* must be sought elsewhere than in dictionaries. There is, appended to the work, a list of words, termed "Index to the synonymes," extending over nearly 200 pages, and containing, the author tells us, more than 20,000 vocables, which is a feature peculiar to this dictionary, but which ought to be found in all works of this class. The provoking difficulty of being unable to find the word you are looking for, so constantly experienced in consulting dictionaries, is, in a considerable degree, obviated by this arrangement. Professor Dunglison's Lexicon will be of infinite use to medical students, and we recommend it to the attention of the ardent alumni of the hundred medical schools throughout the Union.

PART THIRD.

Selections from the British and Foreign Journals.

I. THE FOREIGN JOURNALS.

ANATOMY AND PHYSIOLOGY.

On the Physiology of Nutrition. By J. F. Simon.

The following experiments were undertaken with a view to determine whether the stomach of the child possesses the same properties of coagulating milk as the stomach of the calf.

I. A piece of the stomach of a child, which had died immediately after birth, was introduced into a dish containing cow's milk. Into a second dish, containing an equal quantity of milk, a portion of a calf's stomach was introduced; and a third dish contained milk, without any addition. The three vessels were placed in the water-bath, at a temperature of 30° R. In twenty-eight minutes the milk containing the calf's stomach had coagulated, but that in the other two dishes, after the lapse of an hour, remained uncoagulated These two dishes were left standing during the night in a temperature of 17° R; in the morning the milk containing the portion of child's stomach was still uncoagulated; that in the other dish had coagulated from acidity.

II. A portion of the stomach of a child, five days old, having been previously well washed, was introduced into cow's milk, as in the preceding experiment, with a like result. A quantity of the colostrum of the child's mother was then obtained and divided into three portions of about six drachms each. Into one portion a piece of the child's stomach was introduced, into another a piece of calf's stomach, and to the third no addition was made. The milk of the first and second portions began to coagulate nearly at the same time; but the coagulum of the first was firmer and the whey clearer than those of the other. The third portion did not coagulate. From these experiments it is inferred that the stomach of a mammæ is adapted for the coagulation and digestion of the milk of its own species only.

III. Into each of three test tubes a portion of curd of cow's milk, coagulated by runnet, from which the whey had been expressed, was introduced. The curd, in two of the glasses, was covered by a piece of fresh calf's stomach, and in the third by a piece of child's stomach. A fourth glass contained a portion of the curd of woman's milk, coagulated by the stomach of a child. Into each of the glasses a quantity of acidulated water was now poured, and they were placed in the temperature of 30° R. In an hour and a half artificial digestion commenced in all four glasses; and in nineteen hours the curd of the fourth glass was, with the exception of a few flakes, completely dissolved. In twenty-three and twenty-four hours the first and second glasses had advanced to an equal stage; but at the end of thirty hours, when the experiment was broken off, the curd contained in the third glass was still not fully dissolved. It should be remarked, however, that the quantity of curd in the third glass was only about a third of that contained in each of the others; but the portion of stomach was small in proportion. Experiment showed that the casein is changed by the artificial digestion into albumen. The solution of the curd coagulated on the application of heat, and comported itself towards the chemical reagents precisely as albumen. *Müller's Archiv.* Heft i. 1839.

On the Production of Urea in the Animal Body. By Dr. R. MARCHAND.

THE following experiments were undertaken with a view to determine whether urea can be detected in the blood of an animal which has fasted for several days. With a view to lessen the cruelty of the experiment, Dr. Marchand fed the animal upon pure candi-sugar, a substance which contains no nitrogen, and which could therefore exert no influence upon the result of the experiments.

A large and powerful shepherd's dog was fed for fourteen days upon milk, in order to ascertain the quantity of urea contained in the urine of an animal thus simply nourished. During the first five days the quantity was 2·6 per cent.; during the next five days it was 3 per cent., at which figure it remained during the rest of the experiment. The animal was now fed upon pure sugar and distilled water, and it consumed daily about 10 oz. sugar. During the first six days the urine contained 2·8 per cent. urea; in the next five days the quantity was 2·4 per cent.; and in the next five 1·8 per cent. The animal was now very weak and lean, but there were no ulcerations of the cornea. The diet was again changed, and the dog was fed on milk and bouillon; but the increase of the quantity of urea did not correspond with the rapidity with which the animal recovered its flesh. The quantity of urea was only 2·4 per cent. after the dog had recovered its former good condition, and it did not regain its full proportion of 3·2 to 3·35 per cent. for some days afterwards. Sugar and water were again given, and in eight days the proportion of urea was 2 per cent. The abdomen of the animal was now laid open, and a ligature was applied to the renal nerves, an operation which Dr. Marchand regards as equivalent to extirpation of the kidneys. The incisions soon healed, and nothing remarkable occurred for six days, when vomiting ensued, but the matter vomited was too small in quantity to allow of chemical examination. Ten days after the operation the jugular vein was opened, 3 lbs. of blood were taken, and the animal died immediately. The blood yielded 4·88 grammes of nitrate of urea.

Dr. Marchand is of opinion that urea exists in the blood in the healthy state, although in extremely minute quantities. The nicest test for the detection of urea is the crystallization of chloride of sodium in octoedrons, which ensues, when it is present. Dr. M. found that $\frac{1}{10}$ or $\frac{1}{20}$ urea dissolved in 100 to 150 parts of water was sufficient to cause the production of at least some octoedral crystals of the salt; and, as yet, urea is the only substance which we know to possess this property. Twenty pounds of the serum of cow's blood, treated with alcohol, to free it from albumen, and then evaporated to dryness, gave a residue which was exhausted with alcohol and again evaporated. The residue thus obtained, on being dissolved in water and mixed with salt, caused it to crystallize in octoedrons.

Müller's Archiv. Heft i. 1839.

Observations on the Plague. By Dr. A. BULARD.

[THIS is a paper of some value on the morbid anatomy of the plague, communicated to the Editor by Dr. Bulard, while on a recent visit to Berlin. As this gentleman spent six years in Turkey and Egypt in investigations respecting that disease, his observations are certainly entitled to consideration. We can only afford room for some of the most important particulars, which we the less regret, as we are promised a four-volume work on the subject, by and by. The dissections were made at various intervals, from half an hour to twenty hours after death, and in every instance with uncovered hands, and without using any disinfecting materials.]

Skin. There were always more or less livid patches on the anterior surface of the neck and chest in whites, as well as ordinarily on the scrotum and labia pudendi; it is presumable, therefore, that the same existed in people of colour. The petechiæ, observed during life, persisted after death; and the buboes and carbuncles always collapsed.—*Muscular system.* Slight cadaveric rigidity, diminished cohesion of muscular tissue, which is soft, somewhat moist, and light coloured

—*Nervous system.* Sinuses of dura mater and meningeal vessels generally very full of blood; membranes themselves normal; medullary substance of brain presents numerous minute accumulations of blood on section; the cortical substance has generally a polished appearance; consistence of the organ diminished. Sympathetic nerve neither red nor softened; its ganglia everywhere healthy, petechiæ in some cases in the lower end of its thoracic portion.—*Sympathetic system.* Here existed the only really constant, the most extensive, best-marked, and at the same time least-known morbid changes. The glands varied in size from that of a pistachio-nut to that of a goose-egg, and upwards; in colour from that of the cortical substance of the brain to the deepest livid; in consistence, from that of scirrhus to that of putrid detritus. Considerable effusion of blood is found in the abdomen, on the same side as the inguinal buboes; and knotty tumours extending in the course of the lymphatic vessels up to the diaphragm. The substance of the glands presents the evidence of every stage of morbid change from sub-inflammation to suppuration; the lymphatic vessels appear never to participate in the diseased changes. Effusion of blood takes place in the axilla also, as well as into the integuments of the chest, when the disease is centralized in the glands of those regions. The lymphatic system is not diseased in all its parts at one and the same time; buboes never coexist in both axillæ, both groins, both cervical and popliteal regions, nor are the homonymous glands of both sides ever simultaneously attacked.—*Respiratory organs.* Lungs crepitating; bronchial mucous membrane healthy.—*Heart, &c.* Pericardium frequently contains red serosity; right side of the heart completely filled with clotted black blood, often with fibrinous concretions; similar blood in the veins, which often presents something like drops of oil on its surface; arteries generally empty and healthy. —*Organs of digestion.* Softening of the membranes was an ordinary appearance; the stomach frequently loaded with blackish fluid; its external membrane ordinarily palish yellow, thickened, and softened; internal surface marked with petechiæ, and in advanced cases ulcerated, a state never observed in the small intestine; the vermiform appendix of twice or thrice the natural size; Brunner's and Peyer's glands normal.—*Secretory organs.* Liver loaded with black blood; gall-bladder distended and spotted with petechiæ; its membranes thickened in twelve cases; bile in nothing remarkable. Spleen enlarged to treble or quadruple its normal size, and filled with blood like wine-lees. In four or five cases only was this organ healthy; in six instances it contained carbuncles. Pancreas normal; kidneys usually twice or thrice their natural size, loaded with dark-coloured blood; with an appearance of real hemorrhage in the pelvis.

These lesions are divided by Dr. Bulard into two classes, the primitive and consecutive. The former are those affecting the lymphatic system, and originating in a morbid state of the *lymph* itself. Among the secondary alterations of most importance are those of the *blood*, and they are induced, according to this observer, through the direct communications between the lymphatic vessels and veins; the condition of the blood accounts in its turn for that of the *various organs.* That fluid never presented any buff; its cohesion was found greater than natural during venesection; it emitted a peculiar odour in some cases, and occasionally remained perfectly fluid. It was analyzed thrice, and found to contain in 100 parts the following ingredients:

Clot	Water	35·576
	Fibrine	00·624
	Colouring matter, with some fibrine, albumen, and fatty matters	3·800
Serum	Water	54·420
	Albumen and colouring matter	4·704
	Extraction	00·252
	Chloride of potassium and sodium	00·408
	Carbonate of soda and fatty matters	00·216
	Distinct traces of sulphurous acid.	

The black-coloured mass found in the stomach was also analyzed, and 100 parts found to contain:

Water	95·75
Oxide of iron	0·25
Resin	1·75
Mucous and fat	0·25
Albumen with colouring matter	2·00

Wochenschrift für die gesammte Heilkunde, No. 42. *Oct.* 1838.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Case of Apoplexia Cutanea. By Dr. OTTO, of Copenhagen.

I HAVE seen and treated a case of this disease which, I must confess, I should not have known, unless I had read the observations and remarks on the subject by Dr. Léveillé, in the *Revue Médicale.* I was one morning early called to a carrier, who, the evening before, had gone to bed half drunk, and on awaking in the morning found a great part of his body—the breast, the back, and the upper extremities, covered by widely-spread, dark-blue ecchymoses, not, however, elevated above the level of the skin. He and his wife suspected that he had bad during the night an epileptic attack, and had during it so beaten himself as to cause the blue spots; but as such spots do not usually occur so soon after blows and falls, and as the usual yellow colour on the places which have suffered long from blows was nowhere to be seen, and especially as the patient had never been affected with epilepsy before, I could not agree in the opinion. Although he was so far in his senses as to answer in an ordinary way my questions, and to express his fear of the blue spots, still he was sometimes quite stupid, and suffered from *tremor artium,* which, however, was not surprising to me, as I knew him to be a confirmed toper. His face was not red, his eyes gave no indication of congestion towards the head, and he affirmed that he had neither headach nor vertigo. There was no paralysis or any other symptom indicative of apoplexy. I had therefore every reason to consider the disease as a true *apoplexia cutanea,* of the kind called by Léveillé *apoplexia cutanea ecchymatica,* to distinguish it from *apoplexia cutanea exanthematica,* which is characterized by large irregular spots elevated above the surface of the skin; in the last-mentioned form of the disease, the blood in its way to the skin is still in the arterial capillaries; in the first-mentioned form, as in the present case, the blood is effused into the subcutaneous cellular tissue. The pulse was full and quick, the tongue clean, the skin warm, and the bowels open.

I took blood from the patient, ordered rest, a regulated diet, sulphate of magnesia dissolved in water, and to bathe the ecchymosis with a saturnine lotion. The same day the man ceased to speak incoherently. No other symptom occurred, the spots gradually became paler, but did not completely disappear until three weeks. He has had no relapse. *Casper's Wochenscrift. May,* 1839.

Case of Chronic Farcy terminating in Acute Glanders. By M. LENEPVEU.

A. P. æt. fifty, groom, admitted to the Hospital St. Louis, under M. Jobert, 8th Sept., 1838; has been employed for eight years in an establishment for hackney carriages; a number of the horses had been affected with glanders and farcy during the course of the year. When admitted, he had been four months ill; his complaint having commenced with wandering pains in the shoulders, loins, and limbs, without fever, followed, about a month after, by swelling of the left ankle-joint: almost at the same time several soft tumours, free from redness or pain even on pressure, appeared in the limbs, the latter being elsewhere free from tumefaction. These tumours were, *on the patient's admission,* found to be subcutaneous abscesses, unattended with engorgement either of the veins, lymphatics, or neighbouring tissues; inguinal glands slightly swollen but indolent; no fever, appetite good, general state satisfactory; no traces of syphilis discoverable either on the skin, in the pharynx, or nares. The patient was put on a course of tonics; the tumours to be rubbed with an iodurretted ointment.—*Sept. and October.* Tumours slightly diminished in size; two on the foot and right leg have opened spontaneously and continue to discharge—the former a serous yellow fluid in small quantity, the latter grayish dusky-coloured pus in much greater abundance, gradually changing to a blackish tint, and mingled with shreds of gangrenous cellular tissue.—*November.* The patient is still without fever and has a good appetite, but has lost flesh considerably.—*December.* He grows morose and low-spirited; has lost appetite and sleep; decumbiture constantly dorsal; movements difficult; pain in head, limbs, and trunk; tumours now exceedingly tender; the cicatrix adjoining the left eye has inflamed, and furnished for some days past a good deal of yellow pus; cough, and white ropy expectoration, lasting a few days only. A fit of shivering now came on, followed by fever; which never disappeared till the patient's death; stupor without delirium; extreme emaciation; involuntary stools without diarrhœa or gastric symptoms; tongue natural.—*25th.* Abscesses of the thighs and arms, apparently absorbed; fistula of the left foot changed into an ulcer, with grayish fundus and livid perpendicular borders.—*28th.* Erysipelatous redness of forehead extending to the scalp; violent nocturnal delirium, prostration by day.—*29th.* Livid spots over each eyebrow; white discharge from surface of right eye; under frontal ecchymosis are small flat soft tubercles; vesicles on the eyelids; dark-coloured ulcer over right frontal eminence; violet tubercular elevation at the root of the nose; the erysipelatous tinge is changed into a blueish yellow hue. Similar ecchymotic appearance on shoulder, with serous phlyctenæ; on right thigh three isolated white pustules, surrounded with a small red circle, resembling in form those of modified smallpox: the redness of the skin, ecchymosis, vesicles, and pustules, were all developed in about twenty-four hours. The patient expired at three P.M. —*Dissection forty-two hours after death.* Inguinal glands swollen and red, with a small white nucleus in the centre; under the skin of the thighs, in the situation of the tumours, is some matter of dirty white colour, concrete, soft, and not surrounded with a pseudo-membrane; similar matter on surface of left deltoid muscle, and small purulent collections in that of the right side, pus in the left ankle-joint, other articulations healthy, bones of the limbs normal, muscles of right leg infiltrated with purulent sanies.—*Head.* Brain healthy; meningeal vessels and sinuses gorged with blood.—*Nares.* Pituitary membrane of left nostril healthy; that of right side injected and studded with pustules, extending from the inferior turbinated bone to the Eustachian tube; the membrane appears ulcerated in several points after the removal of the thick yellow opaque matter deposited on its surface; within the group of pustules the mucous membrane is swollen, of violet colour, and speckled with whitish points. The injection extends into the frontal and sphenoid sinuses; the lining membrane of the latter contains a small clot of blood in its substance.—*Larynx.* Small deep ulceration above the right corda vocalis; a very minute cartilaginous-like patch between the laminæ of the corresponding arytæno-epiglottic ligament.—*Lungs.* Inferior lobes engorged; numerous points of lobular pneumonia, varying in size from that of a millet-seed to that of a filbert, the centre generally formed of a yellow concrete matter, in some instances containing pus, and surrounded with dark and hardened pulmonary tissue; slight redness of ultimate bronchi.—*Heart and pericardium* healthy.—*Liver* contains some fibrinous matter, such as that described in the lungs.—*Spleen* much enlarged, firm and denser than natural, and of reddish colour; contains several small clots of blood and a number of large, yellow, fibrinous, dense, and resisting masses; a layer of similar matter underneath the fibrous capsule.—*Stomach and intestines* healthy throughout.—*Mesenteric glands* enlarged.

[We refrain, until the publication of M. Rayer's promised work on Chronic Farcy in the Human Subject, from commenting on this remarkable case; the post-mortem examination was made in the presence of several hospital physicians, and the diseased parts shown to the Members of the Royal Academy of Medicine.]

L'Expérience. Janvier, 1839.

On Cod-liver Oil and Ray-liver Oil in Scrofulous Affections. By M. GOUZEE.

THIS oil, formerly recommended by Dr. Percival and many German physicians, has lately been employed in France and Belgium. M. Gouzée declares it to be one of those rare medicines which produce marvellous results; that it is useful in scrofula,—above all in that form producing osseous alterations in young subjects; and even in chronic articular rheumatism, &c.

CASE I. About ten years ago, M. G. was consulted regarding the health of a child afflicted with rickets, having a pigeon-breast and a dorsal curvature. The child had constant fever, acute pain in the precordial region, with great impulsive action of the heart, laborious breathing, sleepless nights, constant reclination upon the left side, constant diarrhœa, and irritability of the stomach. It had been in this state some time, and was gradually sinking. The oil of the liver of the ray (*Raia pastinaca*) was administered in the dose of half a teaspoonful morning and evening, and gradually increased to half a tablespoonful. The fever rapidly diminished, the precordial pains disappeared, the diarrhœa ceased, and in less than a month, the little patient was walking about. The remedy was continued for several months, and at the present time, nothing remains but a slight narrowing of the chest, and a trifling projection of the head forwards, the result of the dorsal curvature.

CASE II. A child four years old, had an enlarged head, red and swollen countenance, large abdomen, thin and feeble extremities, especially the inferior. The oil was given in the same doses, and in two months the infant walked; the abdomen assumed its usual normal size, and the enlargement of the head was the only remaining trace of the disease.

CASE III. A little girl, ten years of age, had been suffering for three years from a scrofulous enlargement of the left elbow, for which all the usual remedies had been unsuccessfully administered. The integuments were white and shining, the joint was completely anchylosed at right angles, there were acute pains, and for the last twelvemonth, a large fungous ulcer occupied the surface. There was considerable emaciation, violent pains in the abdomen, constipation, and loss of appetite. Local emollient baths were ordered, and ferruginous preparations given internally without success. The oil was administered in the usual doses, and after a month, the general health was reestablished, the pains in the joint had ceased, the swelling was less, and the ulcer had contracted and lost its unhealthy appearance. In two months the patient could move the elbow with facility, the joint had almost regained its normal size, and nothing remained of the former disease but a slight swelling and a want of power to perform perfect extension.

The fish is taken in the summer, and the oil is extracted only from the liver, which is sold at Antwerp. In commerce, there is an impure oil met with, extracted from the livers of various other fishes, and called in Flanders *lever traen*; this is not possessed of the therapeutical power of the former. M. Van Camp, with Hopfer and Hausmaus, have discovered the existence of iodine in this oil. It has a brownish yellow colour, and an insipid taste which leaves in the mouth a particularly disagreeable fishy odour; however, patients, even infants, take it without difficulty, especially after the first doses. M. G. prescribes it, without any adjuvant, morning and evening, a teaspoonful for young children, and a tablespoonful for adults, gradually increasing the dose till it is doubled.

Bulletin Général de Thérapeutique. Mai, 1838.

On Fatty Disease of the Liver (*Pimelosis Hepatica.*) By Dr. ALBERS, of Bonn.

THE fatty disease of the liver has of late been sometimes regarded as hypertrophy of that organ, sometimes as cirrhosis, and sometimes as the result of chronic inflammation. But it is a peculiar affection, and perfectly distinct from all these: it differs from hypertrophy in the alteration which the tissue undergoes; from cirrhosis, by the lobules being unaltered or but slightly atrophied; and from chronic inflammation, by the absence of all symptoms of such an affection even when the disease is completely developed.

The liver when affected with this disease is always large, and surpasses the normal size; the increase commonly affects the whole organ, but especially its basis and the right side; and the more advanced the disease, the greater is the enlargement. The colour of the liver is externally reddish yellow, or marbled with shades of yellow; internally, it is yellow shaded with red in the early, and golden yellow in the later periods of the disease. The substance is firm and compact like that of the liver of a phthisical patient, and it retains its firmness for a day or two after its removal from the body. Its weight is increased. Its vessels are all permeable and may be traced to the minute parts; the branches of the vena portæ are all very large; the blood which they contain is black and greasy. The hepatic artery and ducts appear healthy; the bile is very dark and fluid.

If a portion of a liver, thus golden-coloured, be examined with the microscope, it appears like a pale white spongy substance, containing separate cells of a bright colour, with a delicate fluid, and here and there some dark brown points the acini of the liver, which are smaller than in their normal state. Thus, while the tissue between the lobules is abundant and hypertrophied, the lobules themselves are atrophied; and, in all stages of the disease, the increase of the one is proportioned to the decrease of the other. It is in this hypertrophied interlobular cellular tissue of the liver, that the deposition of fat chiefly takes place.

In general, the fatty disease of the liver appears in that organ alone, and all the other parts of the body are atrophied; enlargement and accumulation of fat in the liver being generally connected with an extreme condition of emaciation. In the late French works on pathological anatomy, it is said that the fatty condition of the liver is very common in tuberculous affections of the lungs. But the liver naturally contains a remarkable quantity of fat, which is sometimes increased in many diseases, as well as in phthisis, without constituting what can fairly be called fatty disease of the liver; for the organ retains its brownish colour, the lobules remain normal, and the cellular tissue is only a little increased in quantity. The surface of a section of such a liver is not yellow, but a mixture of brownish yellow and gray; and much of the enlargement which it presents, depends on the great accumulation of blood in the vessels.

With respect to the symptoms of this disease, it commences with all the manifest signs of disordered digestion, pain, and weight in the epigastrium and right hypochondrium, occasional vomiting, and entire loss of appetite. At a later period, there are prominence and tension of the hypochondrium, the pain being increased by pressure, but often relieved by the abstraction of blood. The skin becomes pale yellow, but has the hue rather of that which is seen in organic disease of the stomach, than of common jaundice. As the disease advances, the colour of the skin increases in intensity, and the greatest emaciation takes place; but if the disease be not complicated, dropsy never occurs, though the emaciation is extreme. It continues for from four to fifteen months, till at the last, difficulty of breathing and great weakness supervene, and the patient dies gradually exhausted.

The most important diagnostic is the great emaciation without dropsy; for in hypertrophy, cirrhosis, and all other chronic diseases of the liver, the latter always occurs, both in the form of ascites and of anasarca. The pain relieved by bleeding is also an important sign, being more rarely and less distinctly observed in hypertrophy or cirrhosis.

As yet no remedy is known for this affection; and the immediate cause of the accumulation of fat is equally uncertain. It affects persons of upwards of thirty-five years of age, and both sexes equally.

Rust's Magazin für die gesammte Heilkunde. 1839.

On the Medical Use of Alum, in Diseases of the Heart. By Dr. SCHLESIER.

IN this short paper, Dr. Schlesier calls attention to the great benefit derived from the internal use of alum in dyspepsia, and also and especially in dilatation of the heart. He quotes the case of a girl, aged 10, whose heart was so much dilated, that her case had been considered hopeless; but whom he succeeded in curing by the prolonged exhibition of alum. The stroke of the heart was perceptible over all the left side to the fourth false rib, and its motion appeared to the eye in the intercostal spaces like a rolling wave. The sternum and ribs of the left side were drawn upwards; the respiration was short and hurried, and accompanied by a short, dry cough, and considerable dyspnœa; the pulse was about 100, soft and small, and the child was emaciated and pale. In this state alum was ordered, in combination with rhatany and digitalis, and its effects were truly wonderful. The apex of the heart became weekly more elevated, and the stroke of the heart more limited in extent, whilst the general condition of the patient improved so much that in four weeks she could be removed home. The treatment was continued for a considerable time with great benefit; but still a slight degree of dilatation remained, and the heart was easily affected by exercise and mental emotions.

[The auscultatory diagnosis of the disease ought to have been given, as also the dose of the remedy.] *Medicinische Zeitung*, No. xlvii. 1838.

On Arteritis. By E. BONETTI.

THE author of this paper endeavours to establish the reality of inflammation of the internal tunic of arteries in opposition to the doctrine strenuously maintained by Rasori, of the physiological impossibility of such lesion.* He believes he has succeeded, grounding his belief on the following results, obtained by observing the effects of the introduction of croton oil into the arteries of rabbits, sheep, and horses. 1st. The internal tunic of the arteries possesses sensibility. 2d. It contains capillary vessels, which serve as the nidus of the inflammatory process. 3dly. The same phenomena are observed to take place in the internal tunic of arteries, as in every other system endowed with nerves and blood-vessels, namely, the successive appearance, under the action of a stimulus, of irritation, congestion, and inflammation. 4thly. Inflammation of this membrane comes rapidly to an issue, which consists in ulceration and destruction of tissue, by suppuration and sphacelus.

[We have made this extract simply to show the state of opinion in Italy, on a very interesting question, and need hardly remind the reader that the accurate researches of the French school have proved that truth lies between the opposing and exclusive dogmas of Frank and Rasori. M. Bonetti will not, we trust, in the ardour of discovery, arrive at the adoption of M. Bouillaud's whimsical absurdity, that "*fever is an angeio-carditis, of more or less intensity.*"† The value of proposition 4 may be tested by the results of Bizot, noticed in a recent number.‡]

Giornale delle Scienze Med.-Chir., No. xxxviii. *Agosto*, 1837.

Clinical Experiments on the Emetic and Sudorific Properties of the Hydrated Sulphuret of Antimony, with excess of Sulphur. (Berzelius.)

By M. A. TOULMOUCHE, M.D.

FROM a series of experiments, of which the individual results are given in a Table, M. Toulmouche draws the following conclusions on the properties of this salt.

1. This preparation of antimony acts with greater certainty as an emetic, when given in doses of one or two grains, than when exhibited in larger quantities.
2. It induces vomiting in smaller doses than kermes.
3. Its emetic action, although less uncertain than that of kermes, is still far from sure, inasmuch as it is only developed in somewhat more than half the cases of its administration.
4. It acts much less frequently as a purgative than as an emetic; whereas the contrary is true of kermes.

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* Rasori, Teoria della Flogosi, p. 250 et seq.

† Art. Fièvre, du Dict. de Méd. et de Chir. Prat. tom. viii. p. 86.

‡ Brit. and For. Med. Rev. vol. VI. p. 45.

L. Bartlett

5. Like the last-named compound, it may be given with impunity in large doses, and in other affections besides rheumatism and pneumonia; in these cases its emetic and purgative effects appear to diminish in proportion to the increase of the dose.

6. The sudorific properties attributed to it by writers on materia medica are by no means incontestable; in 102 cases of its exhibition it acted on the skin thirteen times only. *Gazette Médicale de Paris*, No. xiv. *Avril*, 1839.

A Contribution to Psychical Anthropology. By Dr. MALIN, of Lübbenau.

WE have read, we think, in Dr. Prichard, the case of one of the members of a very stupid family, who was himself formerly as stupid as the rest, receiving an injury of the head, and after that manifesting more than ordinary intellectual endowments. Dr. Malin relates under the above title, in Casper's Wochenschrift, three similar cases, and concludes his communication thus:

"What a benefit to humanity, if it could be determined under what condition, and in what cases—at what place and with what force such a blow or fall would be of use! At any rate, I can adduce in favour of my assertion the experience of a very intelligent veterinary surgeon, who saw a horse affected with the staggers (Dummkoller) radically cured, after it had received from its master a heavy blow on the head with a hammer, with the intention of killing it."

Casper's Wochenschrift. *May* 11, 1839.

SURGERY.

On the Treatment of Lateral Curvatures of the Spine, by the Subcutaneous Division of the Muscles of the Back and the Vertebral Column. Abstract of a Letter addressed to the Academy of Sciences. By JULES GUERIN, M.D.

I HAVE the honour to communicate to the Academy the first result of a new operation, which I have already practised twelve times successfully in subjects affected with lateral curvature of the spine. This operation consists in the section of certain muscles of the back and vertebral column. The muscles which I have at present divided are the trapezius, the rhomboideus, the levator anguli scapulæ, the sacro-lumbalis, longissimus dorsi, and the semi-spinalis.

The greater number of lateral distortions of the spine are the result of active muscular contraction, and their anatomical varieties are the result of this contraction, variously exhibited in the muscles of the spine and back The active treatment of this class of deformities must, therefore, consist in the subcutaneous section of the muscles, to the shortening of which each deformity is due. The following are some details of the applications which I have made of this mode of treatment:

I have applied it to subjects of both sexes and of different ages: the youngest was thirteen, the oldest thirty-two years old. All the cases were deformed in the second or third degree, with proportionate twisting of the column, and gibbosity. In some a single division of the retracted muscles sufficed; in others, two or three had to be made. In all I obtained, immediately after the operation, a marked degree of straightening of the column; and in a young man of twenty-one, whose deformity had been subjected for eighteen months to mechanical treatment, I obtained, by dividing the corresponding longissimus dorsi and semi-spinalis, an immediate removal of the whole deviation. In other subjects I have been able to pursue, with a constant success, the treatment by mechanical means. In none of the twelve operations have I met with the least accident; there has been no hemorrhage; but little pain; no fever; and, in every case but one, immediate union of the wound has taken place, without suppuration. I may add that, though delicate, this operation may be performed almost as easily as the similar one in the neck or foot. *Gazette Médicale.* *June* 29, 1839

Statistics of Tracheotomy.

SINCE the introduction of tracheotomy in croupal affections into France, there has, doubtless, been reason to deplore a great number of failures; it may be presumed, however, that one of the chief reasons of its insufficiency depends on the delay and the obstacles that are generally thrown in the way of its performance. The following are the results which the different most celebrated operators have by their own declarations, in a late discussion at the Royal Academy of Medicine in Paris, obtained.

	Operations.	Cures.	Deaths.
M. Amussat . . .	6	0	6
Baudelocque . . .	15	0	15
Blandin . . .	5	0	5
Brettonneau . . .	18	4	14
Gerdy . . .	6	4	2
Roux . . .	4	0	4
Trousseau . . .	80	20	60
Velpeau . . .	6	0	6
	140	28	112

So that, of 140 patients operated on, 28 only have been cured, and 112 have died.

Journal des Connais. Méd. Juin, 1839.

Gangrene resulting from the employment of a Starched Bandage in a case of Fracture of the Patella. By Dr. DEFER, of Metz.

A MAN, æt. 40, received a transverse fracture of the patella, from a fall. The medical officer who was called in, applied first a bandage, to bring the portions of bone together, and then a starched roller, which extended from the toes to the upper third of the thigh; the limb was then placed on an inclined plane. The patient was occasionally visited; but, as he suffered scarcely any pain, the apparatus was not altered. About six weeks after the accident his attendant, on proceeding to remove the apparatus, found the smell such as to induce the presumption that gangrene had supervened; and Dr. Defer was called in. He found the odour that exhaled from the limb such as could not for an instant permit a doubt of the existence of gangrene. The toes which the roller had not covered were mummified and completely insensible. On removing the bandage, the gangrene was seen to extend to within seven inches of the knee. The foot was cold and insensible, and the epidermis had begun to separate. The ankle-joint was exposed; the ligaments were destroyed, and the tendons, being no longer restrained, were extended like cords. The bones of the leg were also exposed in their lower third, and the tendons had begun to slough. Amputation was performed, and the patient recovered.

[The practice in this case was shameful; but the error was less in the employment of a particular bandage than in the subsequent inattention to the progress of the injured part. Any other bandage would probably have produced the same effects, when applied immediately after the accident, and suffered to remain for six weeks.]

Gazette Médicale. Juillet 13, 1839.

On the Superiority of M. Reynaud's Operation for Circocele. By M. JULES ROUX, M.D.

M. REYNAUD takes, with both his hands, the spermatic cord of the diseased side; he isolates and pushes inwards towards the root of the penis the vas deferens, whose hardness distinguishes it from the vessels and nerves of the testicle; pinching then the scrotum with the fore-finger and thumb of the left hand, so as to embrace the spermatic vessels and nerves, he passes through the base of the fold thus formed, a curved needle, with waxed thread. The scrotum then let go, there remains, between the entrance and exit of the needle, an interval of about an inch; upon this interval is placed a thick cylinder of lint, but not very long; and the two ends of the ligature brought together over it, and tied by a *bow-knot*, so as to admit of untying or relaxing, should it become necessary, to diminish its pressure. Small pledgets, with cerate, are put upon the punctures: no bandage is necessary to keep them, but a simple compress is placed over all. The patient remains in bed, with the scrotum on a cushion, and is treated with diluents and lavements. A few days after, inflammation round the ligature takes place, ordinarily slight, and not preventing a tighter tie of the contained parts. If, however, the inflammation be violent, or the pain excessive, M. R. unties and slackens the ligature, reduces the inflammation, and again reties it; which occupies about two or three days' time. As the soft parts become divided, the ligature is tied again with more pressure. Towards the fifteenth or eighteenth day, the nerves and vessels of the testicle have been divided, and there remains only the skin to be cut. M. Reynaud now passes a directory in the line of the thread, and divides the skin with a bistoury. A simple wound succeeds to this incision, and quickly cicatrizes; so that, in about twenty-five days from the commencement of the operation, the patient is ordinarily cured. Two cases have been successfully treated in this manner by M. Reynaud. [Does no mischief arise from compression of the nerve?]

Journ. des Connais. Méd. No. v. *Feb.* 1839.

On Torsion of the Arteries. By Dr. REMAK.

DR. REMAK recommends a modification of the operation of torsion of the arteries, which consists in seizing the vessel transversely with a pair of sharp wedge-shaped forceps, and then pressing forcibly, so as to divide the internal coat. The extremity of the arterity is then seized with another pair of forceps and twisted, while the torsion is prevented from extending up the artery by the first pair. The vessel is thus less injured than in the common proceeding, and the internal coat, which shrinks after being divided, offers an effectual barrier to the blood. The operation was tested experimentally upon a horse: the carotid was divided, and torsion, performed as recommended above, was sufficient to restrain the hemorrhage, even when the horse was made to trot briskly.

Medicinische Zeitung. No. vi. 1839.

On Congenital Dislocations of the Femur. By M. BOUVIER.

AT the meeting of the Academy of Medicine, on the 16th of April, M. Bouvier presented three specimens of this rare malformation. One of them was from a woman, æt. 26, who had died of phthisis. Both femurs were dislocated. On the right side the head of the femur was still attached by a portion of the capsular ligament, which lay between the head and the outer surface of the ileum. The cotyloid cavities were reduced in size, and triangular; and, notwithstanding the wasting of the heads of the femur, the cavities were both too small to admit them.

The second case was one of double dislocation, in a woman, aged 29; but M. B. had received only the left hip-joint, and that, like those of the preceding subject, in an imperfect state. In the place of the cotyloid cavity there was only a slight superficial triangular depression, quite incapable of receiving the head of the femur, though it was much atrophied. The capsule was lengthened, and, as in the preceding case, interposed between the femur and the dorsum of the ileum. It was dilated at its extremities, but contracted, like an isthmus, at the middle, so that the head of the femur could not, during life, have been made to pass through it.

In the third case the left femur only was dislocated. The patient had been a porter, lame from birth, but robust. The head of the femur was higher and more forward than in the other cases. Enveloped above by its capsular ligament, it was placed on a small false articulating surface, on the ilium, to the edge of which the ligament was attached. The muscles attached to the upper part of the femur were all healthy, except the quadratus, which was atrophied, and in part fatty. On trying to draw down the head of the femur, by pulling it parallel to the axis of the body, and at the same time pushing up the ileum in the opposite direction, an insurmountable resistance was found to arise, from the tightness of the anterior and inner part of the capsule. But when the femur was strongly flexed and at the same time rotated very much outwards, and in this position (as in M. Desprez's mode of reduction) drawn parallel to the axis of its body, the head could easily be placed opposite to the acetabulum, though, from the contracted size and form of that cavity, it could not be placed in it.

[In reference to the important but still problematical question of the reducibility of congenital dislocations of the femur, these cases would show that the chief points to be considered are, the more or less complete obliteration of the cotyloid cavity, the contraction of the capsular ligament between the acetabulum and the part which surrounds the neck of the femur, the extreme resistance which the anterior and internal part of the capsule presents to any efforts at elongation of the limb, the difficulty of moving the thigh during life into the position in which, after death, a kind of reduction may be effected; and, lastly, the difficulty of retaining the limb during extension in the reduction which has been effected by its flexion.] *Bulletin de l'Acad. Roy. de Méd. Juin* 30, 1839.

Chronic Hydrocephalus cured by Compression of the Head.
By Dr. Löwenhardt, of Prenzlau.

Charlotte M., aged two years and a quarter, had enjoyed good health, till about nine months ago, when she received a fall on the head. Some weeks afterwards she was observed to be indisposed; she had become dull, her appetite had failed, and she was frequently affected with sickness, which, after a time, ended in almost constant vomiting.

Dr. Löwenhardt saw the child for the first time, about five months after the fall. He found her very much reduced, the skin shrivelled, and the face and feet œdematous. The head measured twenty-five inches, nine lines, in circumference, and the bones stood so wide apart, that Dr. L. could lay his fingers between the two frontal bones, at the upper part of the suture. There was no squinting, but the pupil was dilated and immovable; the respiration was unequal and sighing; the pulse slow and weak. The only nourishment which the child took was a little milk, which it almost immediately vomited; the bowels were relieved by injection. She did not seem to recognize even her mother, and she was completely lost.

In this apparently hopeless state, Dr. L. resolved to try the effect of compression, combined with other suitable remedies. The head was shaved, and three strips of adhesive plaster, which filled up the space between the eyebrows and the margin of the hair, were passed firmly round the head. A stream of cold water was poured from a height of two feet, for thirty seconds, four times a day, upon the vertex; a blister was applied to the nape of the neck, and kept open; frictions with mercurial ointment were made upon the neck and extremities; and, lastly, powders containing a quarter of a grain of calomel, combined with digitalis and chalk, were given twice a day.

In about a fortnight the vomiting had become less frequent, and the child now retained some milk upon the stomach. As it was found inconvenient to renew the adhesive plaster as frequently as was necessary, a strap with a buckle was substituted, which was occasionally drawn a hole tighter. The cold affusion was soon abandoned, from its wetting the strap, and the mercurials were given up as soon as the mouth showed symptoms of the system being affected, and diuretics were ordered instead.

The treatment had commenced on the 23d December, and on the 20th January the circumference of the head had diminished three-quarters of an inch. On the 27th February the circumference of the head was nineteen inches, seven lines; the bones were consolidated, and the appearance of the head was natural. The pupil had become moveable; the face, though pale, had a healthy appearance; the body had gained in size and strength, and the principal functions were in a normal state. Her gait, however, was staggering, and speech was very imperfect.

The child died of a species of typhus, about three years and a half afterwards. The body had continued to grow in strength, but the mental faculties had not kept pace with the corporeal development. *Casper's Wochenshrift.* No. 37, 1838.

On the Treatment of Inguinal Hernia by Trusses. By M. MALGAIGNE.

[WE give the following observations, not because we consider them applicable in their full extent to the surgery of this country, but because they contain remarks which will be very valuable to many, being founded on ample experience and well-digested and correct principles. The paper of M. Malgaigne we shall quote almost unaltered.]

The presence of a direct or oblique inguinal hernia shows a manifest predisposition to the development of a second; so that after an uncertain time, every one who has one hernia should reckon upon having another.

Every bandage hitherto devised for maintaining in place either a congenital or accidental oblique inguinal hernia is formed on a vicious principle, and requires a complete alteration. They all compress chiefly the external ring and act scarcely at all upon any part of the canal; the new principle which I wish to establish and which I have already applied, both in public and private practice, in a number of cases, consists in making pressure on the whole canal, but chiefly on the internal ring. The chief inconveniences of the old method are, 1, that, in closing simply the external ring, it allows the hernia to remain in the canal, and thus does no more than transform a complete into an interstitial hernia; 2, it only by chance produces a radical cure, and, even in children, the proportion of unsuccessful cases is enormous; 3, the hernia is evidently much less effectually detained, as is instantly confessed by the majority of those patients who have tried both methods; 4, when the hernia requires very forcible compression, all the bandages at present employed, by pressing on the pubes, compress the spermatic cord, and hence arises a frightful number of engorgements of the cord and of the testicle, an effect which is not produced by the new method. This method was applied and described by Sir A. Cooper, but is not known or is neglected in England, and is not mentioned in the writings of Samuel Cooper or Lawrence. This singular fact may possibly be somewhat explicable by the following consideration. Ruptures present themselves in practice under two general forms; either simple or reducible with facility, or complicated with serious accidents relating to their strangulation. The latter case, which is somewhat rare, requires prompt decision, and a dexterous and practised hand; it belongs to the domain of surgery, properly so called, and all the great and magnificent works of the modern schools have been chiefly engaged in the consideration of strangulated hernia. Surgeons have disdained the simple and reducible hernia; they have only superficially studied them, and they have abandoned their treatment to the hands of rupture-bandagers. So that these lesions, so numerous and so important, present in our days the strange anomaly, that surgeons study the disease, but do not occupy themselves about its treatment, and that bandagers are charged with the treatment of the disease without being acquainted with it. I was first struck with this state of things, when first appointed to the care of cases of hernia at the "bureau central" of the Parisian hospitals. The average number of those annually applying for trusses and pessaries is 3000; and in the two months of October and November 1835, I was able to collect 435 written observations, and to obtain results worthy of communication to two Academies. Since that time I have silently continued my work, wishing to arrive at results as complete as possible. During the last three years, my employment at the "bureau central" and in the hospitals, my connexion with the chief bandagers of Paris, and my private practice, have enabled me to see more than 2000 cases of hernia, to try almost every known bandage, and to determine the conditions under which bandages should be employed, and to place on exact foundations the science of prognosis and of indications. In this place, I merely wish to speak of the treatment of oblique inguinal hernia, the most common of all, and consequently the most important to the practitioner. The oblique inguinal hernia does not always present the same degree of development, and I have assigned to it the following degrees or periods: 1. When the hernia projects only through the abdominal ring; this I call *commencing hernia*. 2. When it occupies the inguinal canal; M. Goyrand has applied to this the name of *interstitial hernia*, a useful name to continue. 3. When it projects beyond the external ring; this is *bubonocele*. 4. When it descends into the scrotum; *oscheocele*. The latter two degrees are well known; the only practical difference which they present relates to the strangulation, which is more dangerous in bubonocele than in oscheocele, but in common cases they offer the same indications, and I shall not stop any further to notice them.

Interstitial hernia is often not recognized, unless it is very large, which is rarely the case; since, on applying the finger upon the external ring, no projection is felt; the complaints of the patient are ascribed to an imaginary feebleness of the abdominal parietes, or to some other cause. This degree of hernia is very common, and, as strangulation may happen in this case, it is necessary to fix to it serious attention. Lastly, *the commencing hernia*, the first degree of the disease, has been entirely neglected, both by bandagers and surgeons. The reasons of this are easily given. The patient never consults a surgeon at this period of his disease; and I confess that I have not yet had occasion to decide in a case of this kind. It is only in secondary hernia that I have learned to recognize it, after having appreciated all the importance of such a diagnosis; and as this importance results from a fact unknown before my time, it is not to be wondered at, that surgeons, occupied by a large hernia of one side, should be but little careful about an almost imperceptible swelling of the opposite side. But this swelling, however small it may be or may appear to be, is the certain sign of a near approach of a second hernia, and one may vainly check in the most perfect manner the primitive hernia, the second will not the less certainly appear as soon as this slight projection has been perceived. I will return to the subject of these secondary herniæ; I now wish only to establish the course which inguinal hernia generally follows in its development, as far as the fact bears upon treatment.

Hernia most frequently passes successively through these four degrees. Thus, an individual exerts himself and feels a crack in the inguinal region; he sees nothing at first, and it is only after eight or fifteen days that he sees a small tumour projecting beyond the ring, and afterwards the bubonocele becomes oscheocele. From this account, which is applicable to the majority of patients, one may conclude that the first effort has opened the internal ring, and that the hernia has subsequently passed to the extreme degree. Frequently, in these accidental herniæ, the intestines descend at once into the canal; I have seen some of them suddenly pass to the third degree; and sometimes the hernia becomes strangulated at the moment of its production. But I have never known an accidental hernia suddenly arrive in the scrotum. Each of the first three degrees may remain a longer or shorter period: thus, for example, I have seen the interstitial hernia develope itself so as to acquire half the size of the first, and remain for many years without escaping from the inguinal ring; sometimes, after having made its nidus in the canal, it finishes by making its escape externally; and this long delay in the canal, recognizable by the dilatation of its anterior wall, appears to me to be the essential cause, not hitherto recognized, of the displacement of the vessels of the spermatic cord. This being the state of things, one may judge of the advantage derivable from the application of bandages to the external ring, according to the common method. They transform bubonocele or oscheocele into interstitial hernia, and only prevent strangulation by the external ring, leaving the individual exposed to the danger of strangulation by the abdominal ring. They do not even remedy the common inconveniences of simple hernia; and as the pad does not allow anything to escape externally, and very much conceals the projection of the canal, the continuance of inconvenience has been attributed to the most fanciful causes. [A case is related showing the inutility of the ordinary bandages. And M. Malgaigne continues,] I might multiply cases of this kind; but any one may make upon the first hernia which he witnesses, an experiment which will lead to the same results. Apply the thumb upon the external ring and nothing escapes externally; the patient says that his hernia is kept up. Place the thumb upon the internal ring, and the patient will say that it is much more effectually kept up, and that he finds, on making any exertion, that there is greater firmness of the abdomen. One may easily understand, also, that as the hernia is not completely maintained by the common mode, a definitive cure cannot be obtained. The external ring only is acted on; the obliteration, if it take place, is only of the external ring, and the canal will always remain open for interstitial hernia. Now and then, as exceptional cases, cures have taken place; but as it has not been possible to reproduce them with any certainty, by the same means, the facts have been doubted. It has been simply believed that the truss sufficed for the cure of young subjects; nevertheless the number of exceptions to this would scarcely be believed. M. Bourat, one of our best bandagers, told me, with an air of triumph, that, out of fifty children, he probably did not fail to cure ten. But this I cannot, from my own experience, believe. I have had innumerable cases of congenital hernia under my own eyes, which have existed twenty, thirty, forty, and in one instance, fifty-three years, notwithstanding the employment of the common bandage. But how comes it that some have been cured, but that others have not? In my opinion, this depends on the form and on the size of the pad. If the pad rest only on the external ring, there will be no more cases of cure in children than in adults. But if by a happy circumstance, the pad be badly made, too large for the object had in view, it rests on the canal, and may effect its obliteration. In young subjects, the canal is so short as to be readily compressed by a pad of moderate size, even badly placed; and the greater vitality explains the more frequent success; but in the adult, very large pads would be required to produce the same effect; and in simple cases, too large a pad is considered as ill made: I have seen a cure of this kind entirely accidental. Lastly, as a pad of moderate size may rest upon the external ring, without being at all supported by the os pubis, it happens, if the pressure is at all considerable, that pain and excoriations of the skin are the consequence, together with engorgements of the cord terminating in varicocele or engorgements of the testicles. Many patients, treated during late years by the wooden pads of Carpenter, which rest upon the external ring and the pubis, have been compelled to give them up in consequence of pains and excoriations. M. Devergie sent to me a young man with a double inguinal hernia, and at the same time, an orchitis of each side, the effect of pressure by a common truss. Excepting the complication of hernia with the testicle in the canal, I cannot imagine a more embarrassing case. I have examined at the "bureau central," in a certain number of individuals, what was the proportion of engorgements of the cord and of the testicle. In 200 cases of hernia, I have found sixty-five lesions of this kind; i.e.,

Engorgements of the cord, most frequently with varicose dilatation	40
Engorgements of the testicle	23
Atrophy of the testicle	1
Hydrocele	1

One must not, certainly, attribute all these secondary lesions to the action of the truss; for I have found varicocele and engorgements of the cord and of the testicle in individuals who have had old hernia, but who had never worn a truss; and here the cause is the pressure of the hernia itself. And it may be asked, if in the other cases the pressure of the hernia would not have sufficed to produce the same effect; but, in addition to the fact that the action of the truss is too direct to admit of denial, the most favorable conclusion for the common method would be that it is as likely to be the means of producing engorgements of the testicles as herniæ which are not reduced.

The motive which has induced M. Malgaigne to write this paper, is that the old method of employing trusses is almost universal in Paris, and because, as he adds, although the revolution is made in the science, it still requires to be made in practice.

The consequences deducible from what has been said, are, that every hernial bandage which presses upon the external ring, in inguinal hernia, is a bad bandage; and that the first principle applicable to the maintenance of hernia within the abdomen is to make pressure upon the internal ring and upon the canal. The form, dimensions, and degree of pressure of the pad are subjects to be hereafter treated of, together with the questions—In what cases is a radical cure, by means of a truss, possible? and what are the indications and chances of success?

Bulletin gén. de Thérapeutique. Tome xvi. 1839.

MIDWIFERY.

Singular Case of a Woman delivered of Five Children.

Giuseppa Califani, of Naples, at the age of fourteen years and three months, was married to a man aged twenty-seven, by whom she had ten children at eight accouchements; at the fifth and sixth producing twins. She lived with her husband ten years, and remained a widow three years after his death; she then took a second husband, whose age was about twenty-nine. After two regular accouchements, upon her third pregnancy she became enormously large; so that, at seven months, she appeared to be at the termination of her natural period. She was taken, however, at seven months, with labour-pains, and brought forth successively, and by natural presentations, five living children, all of whom were baptized. The mother did not suffer anything extraordinary. Four of these children were females, and one male. The male infant was delivered first, and, after a few minutes, one female; then, after a cessation of fifteen minutes' interval between each, the other three followed. The infants much resembled each other, and were of a regular form, and well grown, and very nearly of the ordinary size of a seven months' fœtus; each weighed about 3½lbs., and measured in length a French foot. The insertion of the umbilical cord was about four lines lower down than ordinarily. The placentas with their membranes were four instead of five; and each had its proper umbilical cord, except the fourth, which contained two in one large sac. The fœtus, with their membranes, placenta, and umbilical cords, are preserved in the Royal Anatomical Museum of the University of Naples. Vincenzo Licci, of Calimera, in Otranto; Vincenzo Massari, of Molfetta, in Bari; and Dr. Antonio Scacani of Naples, conducted the examination.

[The above case was originally reported in the *Giornale delle due Sicilie*, June 28, 1838, by Dr. Pasquale Cattolica, Clinical Professor of Midwifery, and Sig. Antonio Nanula, Professor of Pathology in the University of Naples—and bears every mark of authenticity: we extract it from the under-mentioned journal.] *Bulletino delle Scienze Mediche. Agosto e Settembre*, 1838.

On Morbid Accumulation of the Liquor Amnii in Pregnant Women.

By Dr. Bunsen, of Frankfort on the Maine, and Dr. Kyll, of Cologne.

Case i. Mrs. C———, a healthy woman, aged twenty-six years: when she first came under Dr. B.'s notice, in the spring of 1826, she was pregnant for the ninth time. All her labours had taken place at the end of the full term of pregnancy, and her first five infants were born alive and healthy; her sixth and seventh children, however, were born dead and putrid; and the expulsion of the eighth, which was very imperfectly developed, was accompanied by the escape of a very large quantity of liquor amnii. The patient arrived safely at the end of her ninth pregnancy, but the size of her abdomen was unusually great, and the motions of the fœtus were remarkably feeble. Labour came on, on the 6th of September, 1836; and no sooner did the membranes rupture, than an enormous quantity of liquor amnii escaped (twelve pints), together with the fœtus, the development of which appeared to have been arrested about the sixth month. The placenta weighed two pounds and a half; its structure was very spongy, which peculiarity, Bunsen remarks, he has always found coexisting with arrested development of the fœtus, and unusually abundant secretion of the liquor amnii. In March, 1827, Mrs. C. again became pregnant, and her abdomen once more attained a very large size, but the motions of the child were very distinct. On December 25, 1837, labour pains began; the membranes, when they ruptured, gave exit to nineteen pints of liquor amnii; the child was hydrocephalic, and its abdomen was so distended by fluid that, but for the large size of the mother's pelvis, its extraction alive would have been impossible. The placenta weighed a pound and three quarters. The child survived only forty-eight hours, but the patient recovered, as after her former labours, with great rapidity.

Having become pregnant again in the winter of 1829, the patient placed herself under Dr. Bunsen's care as soon as she felt the motions of the child; and from that time till her delivery, which took place at the end of the eighth month owing to over exertion, she made use of mild diuretics, wore a bandage round the abdomen, and took regular exercise. Labour commenced in the evening of November 24th; on the following morning the membranes ruptured, but the quantity of liquor amnii which flowed away was not unusually great. The child was malformed and labouring under a great degree of ascites, it gave but feeble signs of life, and soon expired. The placenta was again unusually large. During her next pregnancy, which took place in 1831, Mrs. C. did not consult Dr. Bunsen, who was not aware of her being pregnant, till he was summoned to attend her in labour on the 29th of September. The uterine action was energetic, and the patient soon gave birth to a feeble child, not more developed than children usually are at the sixth month, the expulsion of which was accompanied with the escape of a larger quantity of liquor amnii than in any preceding labour. The placenta weighed three pounds and a half. The feeble infant lived but for a month, and Mrs. C. did not again become pregnant.

CASE II. The next case related is that of a healthy woman who had thrice given birth to dead children, each labour having been accompanied by the escape of a very large quantity of liquor amnii. At the end of her fourth pregnancy she consulted Dr. Bunsen, on account of anasarca, with œdema and erysipelatous affection of the labia; and while under his care she gave birth, after a difficult labour, on April 16, 1831, to a female child, affected with ascites, and which lived only twenty hours. The quantity of liquor amnii was very great, and the placenta unusually large. The patient's health continued indifferent for some time after her delivery, but she gradually recovered under the use of steel; and becoming pregnant for the fifth time at the end of 1831, she again applied to Dr. Bunsen. Gentle diuretics with ether and bitters were prescribed; and on October 3, 1832, a healthy male child was born; nor was there anything unnatural in the state of the placenta, or in the quantity of the liquor amnii. Dr. B. has been unable to learn the subsequent history of this patient.

CASE III. A case follows, in which dropsical affection of the mother was unattended by any similar condition of the fœtus. The patient gave birth to three healthy children in the space of four years, though suffering all the time from dropsical symptoms, which at last proved fatal, a few days after her third confinement.

[We cannot but regret that Dr. Bunsen has given no account of the dissection of the children in these cases, since, had he done so, the value of these observations would have been greatly enhanced.]

CASE IV. The patient, a lady, twenty-eight years old, first came under Dr. Kyll's care in consequence of having been infected with syphilis, by a girl whom she had employed to draw her breasts, after her first confinement. After having suffered from this disease for eight months, she applied to Dr. Kyll, who prescribed the corrosive sublimate with advantage; but, when nearly well, she aborted at the third month of her second pregnancy. Three months afterwards, having perfectly recovered, she became again pregnant, and suffered much during this pregnancy from varicose veins of the thighs; venesection, however, afforded her great relief. At the end of the sixth month, without any assignable cause, the liquor amnii began to drain away; two days after which labour set in, and a female child was born, which struggled a little and then died. The expulsion of the child was accompanied with the escape of a very large quantity of liquor amnii. At the expiration of two hours, the placenta, which was universally adherent, was removed, when Dr. Kyll was struck by its remarkably large size. The circumference of the organ was more than a third greater than natural, and its thickness was double that of an ordinary placenta. It was of a pale red colour and spongy structure; but on dividing it, its tissue appeared perfectly natural, save that the blood-vessels were larger than usual, as were also the umbilical arteries and veins, although the child wanted three months of the full term. Three days after delivery, the patient lost a considerable quantity of blood from the uterus, but eventually she perfectly recovered. The large size of the abdomen of the fœtus had already attracted Dr. Kyll's attention; and on making an examination of it, a large quantity of straw coloured fluid was found in its cavity and between the folds of the omentum. The liver was very large, occupying the whole abdomen, and reaching downwards nearly to the bladder; but its substance, when cut into, presented no sign of inflammation, nor any other change in structure than great development of its vessels. This unusually large size is referred, by Dr. K., to the hypertrophy of the placenta and the consequently increased quantity of blood which the liver would receive. The enlargement of the placenta is, in his opinion, owing rather to congestion than to inflammation, since the results of inflammation are obliteration of vessels from exudation, and consequently diminished nutrition of the organ, owing to which it shrinks, and its structure becomes more compact and firmer than natural, sometimes attaining an almost cartilaginous hardness. On making a section of a portion of inflamed placenta, the fœtal surface is found of a straw colour, which gradually changes to a grayish red, and then to a dark red colour on approaching the uterine surface. Inflammation involves some portions only of the placenta, while hypertrophy extends to the whole organ, which is increased in all its dimensions; its vessels are often enlarged, and its tissue is rendered spongy and easily lacerable, though neither infiltration nor hepatization of its substance exists. *Neue Zeitschrift für Geburtskunde.* Band vii. Heft i.

Galvanic Obstetric Forceps. By Prof. Kilian, of Bonn.

Desirous of ascertaining how forceps, the blades of which were made of different metals, would affect the uterus in tardy labours, Professor Kilian caused such a pair to be manufactured. An opportunity soon occurred for testing its efficacy. Labour was proceeding slowly, from deficient action of the uterus, and for two hours and a half the head had remained in the same spot. The forceps were applied, and, as soon as the two blades were joined, the uterus was felt to contract powerfully, but not in such a manner as to assist in the expulsion of the child. [Was not this mere accident?] *Medicinische Zeitung.* No. xii. 1839.

Singular case of Malformation.

On the 20th March of this year (1839), at Schneidemühl, a stout woman, who had before borne five living and healthy children, was delivered of a remarkably stout female infant, presenting the following defective formation. The heart, and, under it, the stomach, both organs apparently separated by a partition, lie outside the thoracic and abdominal cavities, in a sack of skin nearly transparent. The protrusion is through a deficiency of the lower third of the sternum and upper part of the wall of the abdomen, as far as midway between the pit of the stomach and navel. The fissure is altogether 5½ inches long, and 2¼ inches broad, and is situate almost in the middle line of the body. The child is living, sucks, and is otherwise well. The prolapsed parts are protected by an appropriate bandage. *Med. Zeitung. April* 17, 1839.

Case of Mole Pregnancy with Spontaneous Amputation.

By Dr. SCHWABE, Stadtphysicus, &c. at Cölleda.

THIS was the patient's fifth pregnancy. After much incautious exertion, &c, pains came on with considerable hemorrhage, so that the ovum was supposed to have come away among the coagula. A frequent draining of bloody serum followed, and the hemorrhage returned very profusely so as to reduce her seriously. The uterus was found as big as a large fist, above the symphysis pubis, the breasts flaccid, the vagina relaxed, the os uteri very high up, somewhat backwards, soft and closed: bark was given, and afterwards acetate of lead and opium, with very good effect. The hemorrhage, however, returned so profusely as to threaten dissolution: all attempts to dilate the os uteri mechanically being fruitless, repeated doses of ergot were given; and on the fourth day, slight pains in the abdomen made their appearance, and soon became regular, and expelled a hollow mole without any considerable loss. It contained a fœtus of about three months, the surface of which, as well as the inside of the amnion, was covered with a firm reddish substance. The cord formed a knot round the right leg at the ankle, and had nearly separated the foot from it: immediately above the ligature was an enlargement somewhat greater than the foot itself, and resembling a clump-foot in form, and adhering to the left knee.

[We quote this case for the purpose of recording additional testimony in confirmation of Dr. Montgomery's valuable observations upon this subject, to which we were obliged to allude but briefly in our review of his work on the Signs of Pregnancy.] *Siebold's Journal.* B. xvii. St. 2. 1838.

Case of Spontaneous Evolution. By Dr. CARGANICO.

[THE following case is interesting, as it tends to confirm the original views of Dr. J. Douglas, of Dublin, upon this subject, to whom we are indebted for having first pointed out the real mechanism of this peculiar mode of expulsion.]

The patient was a robust, healthy primipara, æt. twenty-eight. The membranes had ruptured with the first pains, and an arm prolapsed. No assistance was requested for two days, when two midwives tried to turn, but did not succeed. The pains were violent, and the author was summoned. He found the patient suffering under incessant and painful uterine contractions, without a moment's cessation. The skin was hot, the pulse small and hard, the patient pale and exhausted. The arm (the left) was immensely swollen, livid and black, and the epidermis peeled off. The labia and vagina were much swollen and were beginning to be dry. The author made an attempt to turn, but could not succeed; he therefore bled her, and used narcotics both inwardly and outwardly, by which means the pains in the loins and abdomen abated; she became quiet and fell into a sound sleep which lasted from five to six hours, during which she perspired freely. Slight contractions of the uterus, and almost without pain, now came on; and by their action, the arm and shoulder were detruded still further, while at the same time the left side of the thorax began to press downwards from the sacrum towards the left arm: the hip followed the side of the trunk, and, as the child turned completely round, the nates followed, and together with the trunk and legs were now expelled. The head with the arm stretched along it followed without any difficulty, as also did the placenta. The whole process lasted only a few minutes, and the patient assured the author that she had scarcely felt anything of the pain. The child, which was dead, was full grown: it was emphysematous, the epidermis peeling off, and the limbs quite flaccid. It was to this state of perfect flaccidity, the result of incipient decomposition, that the patient was indebted for the perfect ease with which the whole was accomplished.

Medizinische Zeitung. Nos. 29-34. *Jahrgang* 7, 1838.

Case of Difficult Delivery of Twins which were united by the Breast. By the Berg-chirurgus Rath of Zellerfeld. (*From the Archiv. der Harzer Chirurgen-Verein.*)

The patient was turned forty years of age; mother of several children. She had passed the first half of her pregnancy in perfect health: but during the last few weeks before labour, complained of constant cramps and much pain from the movements of the child. Shortly after the os uteri was dilated, the pains subsided; and although they were again excited by the ergot, they had no effect in forwarding the labour. The forceps was now applied, and the head brought down as far as the os externum, but beyond this it was impossible to move it; and on further examination, a second head was found behind the first in the pelvic cavity. The patient was much exhausted, the abdomen tympanitic, very pendulous, and acutely painful; the external parts much swollen. A prolapsed umbilical cord was felt beating at the lower and posterior part of the vagina; but the patient had not felt any movement for some time. As there was no doubt that the first child was dead, and that it was an insurmountable obstacle to the delivery of the other, which might still be alive, and also increased the patient's danger, by protracting the labour, the head was separated from the body with some difficulty, owing to the closeness with which it was embraced by the external parts. It was effected by means of a gum-lancet, which the author considers to be an excellent instrument for the purpose. The second head could now be reached with the forceps, and was brought down as far as the chin and nape of the neck; but another obstacle now showed itself: after repeated examinations it was ascertained that the children were united by the breast, and that one had its back to the pubes, the other to the sacrum. The author endeavoured to give them such a direction that they should be in the longest diameter of the pelvis, by which means he succeeded in bringing them down as far as the shoulders. Having carefully disengaged the arms, and turned the children again into the antero-posterior diameter, and having fixed a blunt hook upon the band which united them, he gave it to an assistant, and then, with a fore-finger in the axilla of each child, he completed the delivery. Considerable hemorrhage required the speedy removal of the placenta, and the uterus now contracted. The children (two girls) weighed 15 lbs.; they were 17 inches long. The part by which they were united was 9 inches long and 3 broad, and extended from the upper extremity of the sternum to the navel, into which one umbilical cord, which was common to both, entered. The diameter of the two children, when laid close together, was between 7 and 8 inches, from back to the other. One child had two thumbs on the right hand. The cord was 19 inches long, and unusually thick. After suffering some time from peritonitis, &c. the patient recovered.

Siebold's Journal. B. 17. Heft ii. 1838.

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

On the Action of Arsenious Acid, and the Dose in which it may prove fatal to Man. By Dr. Lachèse, Jun.

[The following is an abstract of the cases upon which the opinions of the author are founded.]

Case i. On the 5th August, 1830, P—— went to the house of his uncle T——, and remained some time, sitting close to the vessel which held the domestic store of salt. The following day, T., his wife, and three other persons had some soup for dinner. Very shortly after the meal, they all suffered more or less from oppression at the stomach, an acrid sensation in the throat, general uneasiness, nausea, and vomiting. At supper the remainder of the soup was consumed, the symptoms reappeared with increased violence, and lasted throughout the night.

On the 7th, the whole of the party felt a burning heat in the stomach and throat, but there was no vomiting. The wife went to her work; and as she did not take her meals at home, she remained that day free from any illness. On the 8th, three of the party left home, but complained of lassitude and weakness of the limbs. After their dinner, on their return, they were again seized with vomiting, and T., with another of the family, suffered severely. They complained of violent pain in the stomach, and were attacked with frequent vomiting of biliary matter mixed with food. T. suffered from severe pain in the legs, so that he could not stand,—vertigo, and convulsions. On the 26th September, he died; his lower extremities having become completely paralyzed before death. The other person, a female, died on the 18th October. No inspection of the bodies was made. It is worthy of remark, that all who came to the house during the illness of these persons, and who partook of any food, suffered from similar symptoms. After a number of trials, the salt was thrown away, and the sickness disappeared.

CASE II. The wife of the deceased T., who had not suffered so much as her husband, was gradually recovering, when, about the end of August, P. made her a present of some plums, desiring her to eat them. She ate a few, and in ten minutes afterwards was seized with violent colic, vomiting, convulsions, and insensibility. For several days her life was despaired of; but she left her bed at the end of eight months, much enfeebled in body. She was worn and emaciated, suffered from constant gastrodynia and indigestion. Two other persons, who merely tasted these plums, were almost immediately seized with vomiting, which relieved them.

CASE III. On the 24th July, 1832, Pl——, who had inherited one half of the property of his uncle T——, paid a visit to his brother-in-law, M——, the inheritor of the other half; and after having several times examined some flour in a bin, left the house. On the 26th, the wife of M. baked, as usual, a hundred pounds of bread, and gave some of it to two of her neighbours. On the same day, about noon, thirteen persons partook of this bread, and soon afterwards they all suffered from symptoms of oppression and pain in the stomach, nausea, vomiting, accompanied by an acrid, burning sensation in the throat, colic, pain in the limbs, thirst, and general uneasiness. On the following morning, some of these persons, not suspecting the cause of their illness, breakfasted with the same bread. The symptoms reappeared in an aggravated form, in some with diarrhœa, in others with obstinate constipation. They also suffered from vertigo and general weakness. They recovered, after having been obliged to keep their beds for a few days.

CASE IV. On the 24th June, 1835, V—— took for supper some food prepared by his wife. He died in *five hours* afterwards, having suffered from vomiting, intense thirst, and excessive weakness. On inspection, arsenic, in very fine powder, was discovered in the alimentary canal, the upper portion of which was in a state of intense inflammation. Near the ileo-cæcal valve some white semifluid masses were found, which were thrown away without examination.

CASE V. On the 23d June, 1832, a young woman ate some bread, on which a quantity of coarsely powdered arsenic, mixed with butter and rice, had been secretly spread. In *two hours* she was seized with vomiting, diarrhœa, and severe pain in the stomach. On the 25th she was seen by a physician. Her features were altered, her eyes sunk, and her pulse small and irregular; the abdomen was tense and painful, the limbs cold, the arms and hands in continual motion. She died in the evening of that day, more than fifty hours after having taken the poison.

Dr. Lachèse then observes that an analysis of the cases of these twenty persons, who were certainly poisoned by arsenic, will enable us to resolve these two important questions: 1. *What is the action of arsenic?* 2. *In what dose does it begin to be poisonous?*

First. *What is the action of arsenic?* After adverting to the opinions of Brodie respecting the absorption and circulation of this poison in the blood, with its remote influence on the nervous system, the author attempts to settle, from the preceding facts, in what cases the alimentary canal, and in what the nervous system is liable to become more especially affected.

When arsenic is taken but once, and in a small dose, the stomach is but little affected. There is, perhaps, a sense of oppression in the stomach, an acrid sensation in the œsophagus, vomiting ensues, and the symptoms vanish. This was observed in the two individuals who tasted the plums in Case II. In a stronger dose, there will be nausea, vomiting, and colic, weakness of the limbs, and a feeling of lassitude, which may last several days. These were the symptoms manifested by the persons who had partaken of the bread, (Case III). The alimentary canal was here almost exclusively affected, but still there were traces of an incipient affection of the nervous system. If a still more powerful dose be administered *once* or *twice*, there will be, in addition to the gastro-intestinal irritation just described, pain in the limbs, general uneasiness and restlessness, with convulsive spasms of the muscles. The action on the nervous system is here more strikingly indicated; and to this must we ascribe the principal phenomena. For an illustration of this state, the cases of M. and his wife (Case III.) may be referred to, they having twice partaken of the bread which had been baked the day before. If a very powerful dose of arsenic has been given, the affection of the nervous system is as speedily induced as that of the intestinal canal; but the nervous affection is far more serious, and is evidently the cause of death, or, if the individual survive, of the chief symptoms under which he labours. The case of the woman T. (Case II.) affords an illustration of this kind of action.

Sometimes the operation of arsenic, in a large dose, is slowly manifested, a circumstance which would appear to be due to the state of the poison. In all the cases in which it was exhibited in fine powder, the progress of the symptoms was extremely rapid; for in all the individuals poisoned by P., and in the case of V. (Case IV.), who only survived five hours, the symptoms manifested themselves in a few minutes after the poison was swallowed. On the other hand, in Case V., the deceased swallowed the poison in the form of a coarse powder; she was not taken ill for two hours, and she survived its effects fifty hours. In a case related by M. Laborde, a girl continued to bite, during the greater part of the day, a lump of arsenic, which had been given to her. For several hours no symptoms appeared; and she did not die until after upwards of ten hours' suffering. In these two cases the arsenic was in lumps. Lastly, when small doses of the poison are successively administered for several days, the stomach and intestinal canal appear to be exclusively affected; and death may ensue from extensive morbid changes in these organs. The affection of the nervous system, although it may be observed especially at the beginning of the symptoms, is very slight. This view is borne out by a reference to the cases of the two persons who died in Case I. The following are the conclusions of Dr. Lachèse:

1. When arsenic is administered in a dose sufficient to prove fatal, it acts by suspending the functions of the heart and brain.
2. This action is more speedily manifested, in proportion as the arsenic is finely powdered.
3. When the poison is given in small dose, its action is confined to the stomach; and as the quantity is increased, so does the whole of the alimentary canal become affected. At the same time, the affection of the nervous system manifests itself with greater rapidity and intensity, the symptoms becoming more strongly marked, as the action of the poison is more powerful.
4. Several small doses of arsenic, given successively, produce slow poisoning. Death, in such cases, appears to be a result rather of extensive morbid changes in the alimentary canal than of any derangement of the nervous system.

Second Question. *In what dose does arsenic begin to be poisonous?*

In the facts above reported, there would have been nothing to guide us to the solution of this question, had it not happened, that the bread, mentioned in Case III., was submitted to careful analysis by experienced chemists. Several of those who analyzed it, thought that the proportion of arsenic was about half a grain to each pound of bread. This would give a ratio of one quarter of a grain to half a pound, and one eighth of a grain to a quarter of a pound. From the enquiries made, it appears that the smallest quantity of bread eaten by any of the party was about a quarter of a pound, from which it may be inferred that these persons had taken one eighth of a grain of arsenic. From this somewhat doubtful estimate, we may now proceed to judge of the symptoms respectively produced, in relation to the quantity of the poison.

One eighth of a grain of arsenic, mixed with three or four ounces of bread, by a reference to these cases, would produce no other effect, in a healthy adult, but that of speedy vomiting. This quantity of the poison does not appear to affect the mucous membrane of the stomach, or to become absorbed. It seems to give rise to a sudden anti-peristaltic motion in the stomach, by which its contents are expelled. When, at a meal, from half a pound to a pound of bread was eaten, and therefore from a quarter to half a grain of arsenic had been taken, the symptoms were more decided, and began to partake of the character of true poisoning. When this dose was repeated on the following day, the symptoms acquired still greater intensity, and with the usual severe disturbance of the stomach and bowels, there was considerable disorder of the nervous system. Four successive doses, i. e. from one to two grains of the poison, would give rise to gastro-enteritis, and such an affection of the nervous system as would be sufficient to destroy life. These conclusions are further corroborated by the following case.

CASE VI. On the 20th May, 1836, a young man, for the purpose of curing himself of a cutaneous disorder, took one eighth of a grain of arsenic instead of one sixteenth. This was the first time he had taken arsenic. In about a quarter of an hour after he had swallowed the dose, he experienced a sweetish but somewhat acrid sensation in the mouth, extending to the fauces. Ten minutes afterwards he passed a motion, and twice attempted to vomit, but without effect. In two hours he had lost all these symptoms.

Larger doses may, it is true, be given to individuals under medical treatment, without evil consequences; but this is usually accomplished by beginning with very small doses, and gradually increasing them. One half grain of arsenic, exhibited in a day, has been known to produce an illness of eight days; and it is stated by Monro, that a man was killed by a *quarter of a grain*, which had been prescribed for him by a quack. As an answer to this question, we may say that in a healthy adult one eighth of a grain of arsenic may give rise to a disturbance of the system; that from one quarter to half a grain would produce symptoms sufficiently severe to constitute true poisoning; and that in a dose of from one to two grains it may suffice to destroy life

[REMARKS. We cannot allow this paper to pass without offering a few comments upon it, as the subjects of which it treats are of considerable importance in a medico-legal view. The cases, although short and somewhat imperfectly given, are nevertheless interesting. Case IV. is remarkable for two circumstances; the short period within which death took place (*five hours*), and the fact of the mucous membrane of the alimentary canal having been found *intensely inflamed*, notwithstanding the rapidly fatal effects of the poison. One striking point in these cases is that in all, with the exception of the young woman, Case V., the symptoms appeared very speedily, i. e. *a few minutes* after the poison had been swallowed. In the case which constitutes the exception, they did not show themselves for two hours, which is beyond the average time for their appearance Dr. Lachèse is, we think, somewhat hasty in drawing the inference that the action of the poison is more speedy in proportion as it is finely powdered. The symptoms, we are ready to admit, are more generally retarded where the poison has been taken in lumps or in a coarse powder, than where it has been taken in a finely divided state; but the fatal effects of the poison are sometimes more speedily manifested in the former than in the latter case. We have known death take place within a shorter period, where the poison was swallowed in lumps, than where eight times the quantity had been taken in powder. We do not attempt to draw any inference from a solitary case; and we think after all, the difference in the state of the poison will be found to modify the time of access of the symptoms as well as their nature, and not the *period* at which it may prove fatal to life. Dr. Lachèse concludes, from these cases, that the affections of the alimentary canal and nervous system appear simultaneously, and are in a direct ratio to each other, when a strong dose of the poison has been taken. We cannot join him in this conclusion. The case to which he refers (Case II.), in illustration of his opinion, merely proves the slow after-effects of arsenic when it does not prove fatal. It is rare that these two classes of symptoms commence and go on together. In general, it has been observed that the symptoms of nervous irritation do not make their appearance until the derangement of the alimentary canal has subsided; and although the two affections may become occasionally mixed and combined, yet we see no instance of this, in the cases reported, sufficiently well marked to justify the inference of the reporter.

Dr. L. does not appear to us to have succeeded in solving the second question, as to the dose in which the poisonous action of arsenic commences. By analysis, it appears that half a grain of the poison was found in a pound of bread; and although the Doctor allows this to be a doubtful estimate ("*une estimation peu certaine*,") yet he proceeds to reason on it, as if these facts were well established: 1, that each loaf contained an equal quantity; 2, that each quarter of a loaf contained an equal fractional portion; and 3, that the quantity of bread eaten, by each individual, was accurately determined. Having remarked the nature of the symptoms in these cases, he connects them with the quantity of bread, and therefore with the quantity of poison taken; and applying this reasoning to his other observations, he draws the inference of the quantity of poison taken in those unknown cases from the nature of the symptoms. We need scarcely remark, that this method of reasoning is not exactly adapted to aid in the solution of a medico-legal difficulty. Allowing that the poison existed in the proportion ascertained by the analysis of a pound of the bread, not more than fifty grains could have been thrown into the flour. But is it at all probable that these fifty grains should have been so kneaded into the dough, in the ordinary process of making bread, as that each pound should have received its half grain, and each quarter of a pound its eighth of a grain? We confess we cannot view this as in the least degree probable; and therefore we may ask what becomes of the author's conclusions respecting the specific effects of specific doses of the poison? Besides, the report shows that the analysts did not agree in their results; and, therefore, there does not appear to us to have been any just ground for the author's estimate in respect to the quantity of poison taken. Case VI. does not establish more than was known before; and with regard to the case quoted from Monro, as to the fatal effects of a *quarter of a grain* of arsenic, we have some doubts of its authenticity. The author should have given a reference to it. Although we fully agree with Dr. Lachèse as to the general effects of small doses of arsenic, and the quantity required to prove fatal in a human adult, yet it is not in consequence of the case in which he supposes the proportion of poison to have been analytically ascertained.] *Annales d'Hygiène. Avril*, 1837.

On some New Signs of Suspension having taken place during Life.

By M. DEVERGIE.

IN a memoir presented to the Academy of Medicine, M. Devergie notices two circumstances which, in cases of hanging, will prove whether suspension has taken place during life or not. The facts of an ejaculation of sperm in the last moments of life, in cases of hanging, and of the existence of spermatic animalcules in urine, when an emission of urine has immediately followed an ejaculation, are well known, and have led M. Devergie to search for these animalcules in the urethra of persons who have been found hanging. If in such cases the urethra be slit open, or, better still, if its contents be pressed out into a watch-glass, we find a mucous matter, more or less thick, exhaling a strong odour of semen, and containing here and there the peculiar animalcules which are found in the human spermatic fluid alone. But the place of these is occasionally supplied by a number of small rounded bodies resembling the animalcules without a tail; these M. Devergie conjectures, may be spermatic animalcules in an imperfect or rudimentary state. However that may be, the presence of semen in the canal of the urethra is a certain sign that suspension took place during life. The second circumstance is that the end of the penis is so reddened and moistened by a mixture of semen and mucus as to give the idea of a gonorrhœa having existed; whilst the *corpus cavernosum* and *spongiosum* are so filled with thick black blood as to form a striking contrast to the paleness of the same parts in cases of natural death. This sign is of as much value as the existence of sperm in the urethra, and is observed with greater facility. *Bulletin de l'Académie. Nov.* 20, 1838.

New Process for the Separation of Arsenic in Organic Mixtures.
By L. MALLE, M.D.

NUMEROUS experiments having convinced M. Malle that the ordinary processes (those of Hahneman, Rose, Fischer, Orfila, Rapp, and Taufflieb,) resorted to for the detection of arsenious acid in animal matters are more or less ineffectual when the quantity of poison does not exceed one tenth of a grain, he was led to investigate the subject closely, and now proposes the following method, as superior to those hitherto employed. The suspected matters, whatever be their nature, are to be placed in a porcelain capsule; any vegetable poison they may contain having been previously dissolved with alcohol and ether: a solution of hydrosulphate of ammonia is then poured on them, in order to transform into sulphurets the arsenic acid and any other metallic preparations capable of undergoing such transformation which may chance to be contained in the mixture. The liquid is then slowly evaporated, and the residue treated with alcohol saturated with ammoniacal gas, in order to precipitate organic substances, and dissolve the sulphuret of arsenic. The contents of the capsule are next filtered, whereby a fluid, containing alcohol, ammonia, and the sulphuret in a state of solution, is obtained. The whole is next placed in a retort, communicating with a mattress, and then heated in a water-bath, so as to distil over the alcohol and volatilize the ammonia. The residue is then treated with nitric and a little hydrochloric acid, in order to destroy organic substances, and convert the sulphuret into arsenic acid and sulphuric acid. Once this is effected, nothing remains to be done but to separate the two acids by adding some ammonia and ammoniacal sulphate of magnesia to the liquid, which precipitates the arsenic acid in the form of an ammoniacal arsenite of magnesia. This precipitate is collected on filtering paper, and, after having been carefully washed, is reduced by exposure to a current of hydrogen gas, in a small tube, drawn to a fine point, as recommended first by Berzelius.

By this process M. Malle was enabled to obtain a metallic ring from half a pound of alimentary substances, to which five milligrammes* of arsenious acid had been added; in order to remove all doubts of the nature of the metallic substance obtained, it was placed in a small vessel, full of dry oxygen, and thus converted into arsenious acid, which again was precipitated with hydrosulphuric acid in the form of a sulphuret, soluble in ammonia. *L'Expérience.* No. lxxxi. *Janvier*, 1839.

On Hydrated Peroxyde of Iron (Ferri Subcarbonas) as an Antidote to Arsenic.
By a Commission of the Academy of Medicine of Paris.

[IN our First, Fourth, and Seventh Volumes, some accounts were published of the facts which seemed to prove the value of peroxyde of iron as an antidote to arsenious acid. The case in the Seventh Volume was one in which a patient that had taken arsenic was rescued from apparently certain death by the administration of this compound by Dr. Deville; and the subject was then deemed of such importance by the Academy of Medicine of Paris, that a commission was appointed, composed of MM. Deville, Sandras, Nonat, and Guibourt, to enquire into the value of the supposed antidote, and the best means of preparing and administering it. The following is an abstract of the therapeutical part of the Report which has been presented.]

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* The milligramme is = ·0154 English grain.

Dogs were first destroyed by arsenic, to determine the time in which that poison proves fatal when its action is not interfered with, and to obtain a standard by which the beneficial effects of the antidote might be more nearly estimated. Other dogs were killed by ligature of the œsophagus, to determine the influence which that necessary operation would have in their destruction. Four different oxides of iron were then used with different dogs, to whom arsenic had been given. A moist protoxyde, containing 19 per cent. of anhydrous peroxyde, and a black oxyde containing 7 per cent. were entirely inefficacious; the dogs died as soon as they would if no means had been used to save them. The moist hydrated peroxyde, and the common dry hydrated peroxyde of iron (the subcarbonate of iron of former pharmacopœiæ, and ferri-sesqui-oxydum of the present,) both produced more important results. Ten animals poisoned with arsenic were treated with the first. In the first experiments large doses (half a drachm) of arsenic were given, and comparatively small quantities (four, six, or eight ounces) of the magma of peroxyde of iron; yet all the animals lived many hours longer than when the action of the arsenic was left uncontrolled. In the subsequent experiments, twelve grains of arsenic were given, and from eight to sixteen ounces of the antidote; and all the dogs thus treated lived as long as the ligature of the œsophagus would permit them.

With the second compound (the subcarbonate or sesqui-oxyde of iron), in three experiments, in which four grains of arsenious acid and three ounces of the antidote were given, the animals, like those last mentioned, lived to the full period which is possible after tying the œsophagus. The arsenite of peroxyde of iron (the salt which was supposed to be found in the stomach in the preceding cases) was next given to dogs, and evidently acted as a virulent poison. But the apparent anomaly of these cases was proved, by some further experiments, to result from the arsenite of iron, thus administered, being decomposed by the hydrochloric and lactic acids of the gastric secretion, and some uncombined arsenious acid being set free; and this showed that, to counteract the effects of the arsenic, it would be necessary to administer the peroxyde of iron in considerable excess, so that every portion of arsenious acid, whether originally existing in the stomach or set free by the decomposition of arsenite of iron, might be completely neutralized.

The results of all these experiments on animals were completely confirmed by the chemical examinations by which M. Guibourt determined the composition and the mutual actions of the substances employed. Thus the first two oxydes that were employed were of no avail against the effects of the arsenic; and from M. Guibourt's chemical researches it was evident that they did not render arsenious acid insoluble. In the physiological experiments the moist hydrated peroxyde of iron neutralized the arsenious acid in the stomach, imperfectly, indeed, when there was a proportionally large quantity of acid, but completely when the proportion of arsenic was small and that of the peroxyde considerable; and in the chemical experiments it appeared that ten parts of the dry peroxyde were necessary to neutralize one of arsenious acid. In the physiological experiments, again, the dry peroxyde completely neutralized the arsenious acid; and in the chemical the same peroxyde reduced to traces scarcely perceptible the precipitate of arsenious acid that could be obtained by Marsh's apparatus.

It is evident, then, (the Report continues,) that in the circumstances which have been determined, dogs have taken arsenic sufficient to destroy them in a few hours, and that yet they have lived as long as if only the œsophagus had been tied. The poison, therefore, has had no effect;—the peroxyde of iron, which we have employed, is, consequently, a real and efficacious antidote to arsenious acid.

Now, it is demonstrated that we have *disempoisoned* dogs that had taken arsenic; we have determined what doses of hydrated peroxyde of iron are necessary to neutralize fatal quantities of arsenious acid; we have employed, with the greatest success, not only the moist peroxyde of iron (always an inconvenient compound), but also the dry hydrated peroxyde, so common in the shops under the name of subcarbonate of iron, and so easy to administer in all imaginable doses, and so little injurious that we can see no necessity to limit the employment that may be made of it in these cases of poisoning. The following, then, is the mode of treatment which we should advise to be employed in these cases.

The first duty of the physician is to remove from the stomach the greatest possible quantity of the poison by vomiting. For this purpose watery drinks are improper; but while waiting for the peroxyde of iron, the doctor should tickle the vula, and administer oil, which will not dissolve the arsenious acid; but above all, as soon as it can be procured, the patient should be gorged with warm water, charged with some ounces of the peroxyde. The water will cause vomiting, and the peroxyde of iron, which is suspended in it, will neutralize the particles of arsenious acid that are dissolved. This means fulfils at once the two chief indications in every case of poisoning. The antidote must be given as soon as possible, and in such a quantity and for such a time that there can be no reason to suppose a single atom of arsenious acid remains in the stomach. Four ounces of the dry hydrated peroxyde of iron (the subcarbonate of iron of the shops, the ferri-sesqui-oxydum of the London Pharmacopœia,) should be suspended in twenty-four ounces of water, and a good glass of the mixture should be taken every ten minutes. After four ounces are consumed, fresh doses of the same compound should be administered in the same way, and the patient should not be considered out of danger till he has taken at least half an ounce of the peroxyde for each grain of arsenious acid supposed to have remained in the stomach. If, afterwards, symptoms of inflammation should manifest themselves, prompt recourse must be had to antiphlogistics and other appropriate means.

In the short discussion which followed the reading of the Report, M. Devergie said he thought the conclusion too explicit; that the large quantities of the peroxyde which it was necessary to administer, even when but a small quantity of arsenic had been taken, would always be an obstacle to its use, and that it would be unwise to let the presumed antidote take the place of any of the other means usually resorted to, especially continued vomiting. At the conclusion of the discussion it was agreed to employ the words "   antidote," instead of "real and efficacious antidote;" and the Report was then unanimously ordered to be printed. *Revue Médicale. Mai et Juin*, 1839.

MEDICAL STATISTICS.

On Longevity and Mortality in Prussia. By Dr. HOFFMAN.

DURING the eighteen years, commencing 1st January, 1820, and ending 31st December, 1837, the mortality in 12,626,379, being the average population of Prussia during that period, amounted to 6,653,167. Of this number, 149,058 attained the age of from eighty to eighty-five years; 67,754 that of from eighty-five to ninety years; and 31,516 attained an age beyond ninety years. Thus, 248,328 individuals, or 3·732 per cent. of the population attained an age beyond eighty.

During the same period, the births amounted to 9,236,107. Of this number, 319,243 were stillborn; 1,577,018 more died before the completion of the first year; 751,737 died during the second and third years; 305,237, during the fourth and fifth years; 171,808, during the sixth and seventh; 154,124, during the eighth, ninth, and tenth; and 123,693, during the eleventh, twelfth, thirteenth, and fourteenth years. *Medicinische Zeitung.* No. iv. 1839.

II. THE AMERICAN AND COLONIAL JOURNALS.

PHYSIOLOGY.

Contributions Illustrative of the Functions of the Cerebellum.

By JOHN D. FISHER, M.D., of Boston.

THREE different functions have been attributed to the cerebellum by as many classes of writers. One class maintain the doctrine that this organ is the regulator of the movements of locomotion; a second, that it is the centre and source of sensation; and a third, that it is the organ of the instinct of reproduction. The advocates of each of these theories will read the following details with some degree of interest, and the followers of the founder of the new theory of cerebral physiology and of mental physiology, will not fail to summon some of them as proofs of their adopted doctrine.

CASE I. D. O. æt. forty-five. First seen a few hours after death, which took place from pneumonia, as was proved by dissection. On carefully examining penis (which, as was stated, appeared very small,) the prepuce was found covering the glans, and seemed as if it had been seldom or never retracted; in fact the glans was with difficulty made to pass through it, the aperture being so much contracted. When exposed it was pale, small, and pointed, and the urethra of exceedingly small caliber. All the parts of the organ resembled perfectly those of a boy not yet arrived at the age of puberty. The scrotum was soft and flabby; it contained no testicles, but it was thought that the spermatic cord could be felt at the upper part. An incision was then made, commencing at the inguinal ring and extending to the lower extremity of left side of scrotum. The skin, the dartos, and the tunica vaginalis, were of a natural appearance, but no testes nor any bodies of a glandular nature existed in the scrotum. In the upper part of the left tunica vaginalis, the spermatic cord was discovered extending into its cavity about half an inch, and terminating abruptly in a point of a semi-lunar shape. The cremaster muscle was seen extending in numerous small fibres beyond the terminus of the cord which spread themselves out upon the tunica vaginalis. The cord itself was much smaller than is usual in adults. The vas deferens was properly formed and nearly of natural size; its cavity terminated in a cul-de-sac at the end of the cord. The arteries and veins were exceedingly small, hardly distinguishable. The right side of the scrotum and the right spermatic cord differed in no respect from the left except that the latter extended to the bottom of the scrotum and turned upwards a quarter of an inch. So far as they were traced into the abdomen they presented no other peculiarities. Circumstances prevented examination of the *vesiculæ seminales*; it is therefore impossible to say whether they existed or not. From the perfect condition of the vas deferens, it is presumed they did exist, and might have been found.

The history of the individual, the absence of the testes, and other circumstances having brought to mind the doctrines of phrenology in relation to the functions of the cerebellum, the examination was extended to the cranium and its contents with a view to testing the truth of these doctrines. The size of the head was found by measurement to be as follows, viz. 22 inches in circumference from the middle of the forehead over the crucial ridge of occiput; 16 inches from the orifice of one ear to that of the other over the highest point; 6 inches from mastoid process to mastoid process; and 8 inches from ear to ear. The head, therefore, was a large one. On opening cranium and removing brain, it was found to be in a healthy condition, and of large size—but the relative proportion of the cerebrum to the cerebellum was strikingly unnatural; the latter being comparatively exceedingly small. Upon weighing the encephalon, comprising the cerebrum and cerebellum on the day following, it was found to weigh 51½ ounces—or 3 pounds 3½ ounces avoirdupois. The cerebrum alone weighed 47 ounces. The cerebellum alone weighed 4½ ounces. The weight of the cerebellum to that of the cerebrum was, therefore, as 1 to 10½ within a fraction. The cerebellum measured in its transverse diameter 4 inches, in its antero-posterior diameter 2½ inches, in thickness 1½ inch. According to Meckel and others the average weight of the cerebrum and cerebellum united is 3 pounds, and the weight of the cerebellum to that of the cerebrum as 1 to 7, or 1 to 8. Its usual measurement being in its transverse diameter 4 inches, in its antero-posterior diameter 2½ inches, and in thickness 2½ inches. The cerebellum, therefore, in this person was one third less in size and weight than is naturally the case in an adult male—and was the exact weight of that of a female child six years old, who died, and whose cranium was dissected at the same period.

The history of the patient who forms the subject of the preceding case, furnished by a near relative, is of much physiological interest. He was born in 1791, and was, therefore, 45 years of age at the time of his death. The late Dr. Warren discovered the deficiency of testes soon after birth, and observed that he would probably prove to be a natural eunuch. He grew up to the age of puberty without exhibiting any peculiarities distinguishing him from his fellows, except the non-appearance of the testicles. From the age of puberty to the age of twenty-five, and even to the day of his death, he presented the following peculiarities. His voice remained unchanged in its tone, which was decidedly effeminate. He was fond of music, and sung with much taste and effect, but always in treble and in concert with females. After the age of 25, however, it became grave, and he could no longer accompany female voices with ease. He had no beard, and was never known to shave. He never exhibited any amorous propensities or desire for female society. Although of a social disposition, he was very shy in company with females of his own age, and always approached them with evident timidity. He was extremely guarded in his expressions before ladies, and often reprimanded his associates for using in their presence language in the least degree expressive of an indelicate or amorous sentiment. When about 21 years of age he became acquainted with a number of young men fond of pleasure and frolic, and by degrees acquired a taste for the inebriating cup, but during the many scenes of dissipation in which he participated, he was never known to visit a house of ill-fame, or to address any of the numerous ladies of pleasure who walked the street. In short he was, as his mother expressed herself, "a virgin in feeling and conduct to the day of his death."

CASE II. T. P. B , æt. forty-one, on the 29th June, 1836, was a passenger in one of the trains of railroad cars which unfortunately came in collision on the track between Boston and Providence. While seated on a front seat of a car, with his back directed towards the engine, Mr. B. noticed that a sudden effort was made, by means of the "breaker," to arrest the progress of the cars. On observing this, he arose from his seat, and thrust his head out of the window to ascertain the meaning of this operation. At this moment, and while the back part of his head and neck were opposite the edge of the window-frame, the two trains came in collision with tremendous and fearful violence. The consequences of this accident were, that the cars were broken into many fragments, and that most of the passengers who occupied them were thrown out and seriously injured. Mr. B.'s head and neck were brought up against the edge of the window-frame with great force, and he himself was projected to a distance upon the ground, where he remained for sometime in a state of insensibility. When he was first lifted up, it was thought by his fellow-passengers that he was fatally wounded—that his skull was fractured, or the bones of his neck dislocated. He, however, regained his intelligence, and was soon conveyed to his dwelling in a carriage. On visiting him one hour after the accident, I found him in his bed, suffering great pain in the occipital portion of his head and upper part of the neck. He was lying on his back, unable to rotate his head on the pillow, or to move from a horizontal position. Every attempt to move himself was attended by excruciating pain, and he would not allow others to move him for fear of suffering.

On examination, some blood was found on his face, and a flesh-wound of minor importance was discovered in the integuments covering the left mastoid process, and the inferior portion of the occipital bone. These parts were somewhat swollen and tender, as were the integuments and muscles of the neck. No indications of fracture of the cranium existed, nor could any dislocation or fracture of the cervical vertebræ be detected.

For two or three days and nights he obtained little or no sleep, and could not be moved from his horizontal position. The antiphlogistic treatment was continued for some days, when he was so much relieved, that he could, by much care and cautious effort, raise himself from his bed. To do this, he was first obliged to turn cautiously from his back upon his left side, and then raise himself to the sitting posture by the aid of his left elbow. This was the only method he could adopt to raise himself from his bed without rotating or moving his head, which movement was constantly accompanied by extreme suffering. On the second day after the accident, he complained of a numbness in his right arm, and experienced a difficulty in passing his urine. The contractile power of the bladder seemed to have been diminished, and he was compelled to resort to artificial means to evacuate the organ. In the course of two weeks he was able to leave his bed and to walk into the street. And now another interesting symptom was manifested. As he walked about the house and in the street, he observed a singular appearance in the objects about him. Near objects seemed to him to be at a distance, and he felt as if he was much elevated above them. While walking, the street seemed to be interminable in length; and when standing by and conversing with a person of his own height, he experienced the feeling that he was vastly the tallest, and that he was actually looking down upon him during the conversation, yet all objects appeared natural in colour, size, and proportion. Between the fourth and fifth week after his injury, he made the discovery that he had lost the desire and physical power for sexual intercourse, and that no amorous sentiment or the approach of a female could excite it; and he was of opinion that the amative instinct and sexual desire had ceased to exist from the time he was wounded. These symptoms, which were the prominent and peculiar ones of the case, were exceedingly troublesome, and were for a long time combated by local bleeding, blistering, and other remedies. The bladder gradually recovered its power, and the aberration of visual perception was by degrees corrected, so that in the course of four months, Mr. B. was enabled to make water freely and naturally, and to view objects in relation to himself as he was accustomed to see them previous to his injury. He was still, however, unable to rotate his head, and when he attempted to do so, he heard a sort of grating noise, which evidently arose from the deranged action of the vertebræ of the neck, or of the ligaments or muscles attached to them. The numbness of the right arm still continued, and the limb had decreased in size, its circumference being considerably less than that of the left arm. The instinct of generation, for which he was particularly distinguished while in health, was still dead, and the idea of its total annihilation gave him much uneasiness. The trouble in moving the head and the numbness of the arm continued for some months, and the generative function remained completely silenced, according to B.'s own report, until the last summer, and is even now (Dec. 18, 1838,) but partially restored. The mental powers of this patient, particularly his memory of events, were for a time seriously affected, and his decision, courage, and resolution enfeebled. In these respects, however, as in most others, the individual now enjoys his accustomed health and strength.

Case iii. (By Dr. Whittimore). "Mr.——, ætat. seventy-three, has been married about forty years, has had eleven children, ten of whom, as also his wife, are still living. Mr.—— worked alternately as a shoemaker and farmer; frame large, and general appearance that of robust health. Soon after marriage he began to complain of dizziness and noises in head, to which he was more or less subject until his death. About four years ago he experienced, on rising from bed, for three or four mornings in succession, excruciating pain in the head, which was followed by a sensation as if something had given way in left side of head with an audible crack, such as to lead him to enquire if the bystanders did not hear the sound, and was surprised to find they did not. After this, he became partially deaf in left ear, and the dizziness increased. During these dizzy turns, he was obliged to catch hold of the nearest object to keep from falling, and at such times everything seemed to be whirling about like wheels, with a motion always from right to left; these symptoms were for the most part attended with great heat and pain about head, and with redness of scalp. During the severity of suffering he was at times delirious. Most relief was gained by cupping nape of neck and very little from other means.

"Two years ago had hemiplegia of right side, and has had two other attacks since, all slight. Since the occurrence of these he has had a *morbid salacity*, which continued with little intermission, and increased by degrees till three months ago, and then gradually subsided, so that the desire became imperious only once or twice during the night without ability to gratify it, owing to imperfect erection, and for the last year there has been no seminal emission. This lustful feeling aggravated all his other sufferings, and destroyed most of his comfort. Such is the account of the patient, as given by a particular friend; but the patient during his sickness, expressed himself strongly on the subject of this revived sexual propensity, and declared that the desire was felt many times during the day and night, and was scarcely diminished by the frequent attempts he made to gratify it. For the last year, he has been decidedly growing worse, both in body and mind—*mind in a state of imbecility*. Early in summer he had an *epileptic* paroxysm, and within a few weeks before his death had a second. For the last five months was occasionally delirious—screaming as if frightened, and sometimes as if in pain—was afterwards unconscious of what had happened. He died on the *18th September*, 1835, having been in a state of stupor for some days.

"On the following day the head was examined. The membranes of the brain presented some morbid appearances, such as very strong adhesion of the *dura mater* to the skull; thickening with white spots in the arachnoid and a large quantity of serous fluid in the pia mater; arteries undergoing ossification. The brain was healthy, except for the disease to be described. The cerebellum being removed for examination, the right lobe was found to be of its full size; the left about one fifth smaller, and the greater part of its under surface in a *remarkably* collapsed state—*hollowed in* as an organ usually appears *externally*, when there has been any very great loss of substance within. In one place, to the extent of about three lines square, the disease beneath had penetrated quite to the surface, and over this the *pia mater* contained a considerable quantity of dull yellowish serum, elevating the *arachnoid;* otherwise the surface of this lobe of the cerebellum looked well, retaining its integrity, natural colour, and consistence. An incision having then been made through the collapsed portion to the centre of the lobe, and another to cross it, it was fully demonstrated that the whole substance of the organ below the *crus cerebelli* was destroyed, and all traces of it gone, except a line or two in thickness, of the very surface which served as *parietes* to what may be called the cavity. The sides of the cavity were in contact, and were connected here and there by a very soft delicate tissue of a light rusty brown colour, and were separated by the slightest force. The same sort of substance lined the cavity, appearing in some parts a mere discoloration of the inner surface; in others like a distinct tissue, passing by invisible degrees into the *pia mater*, where it dips down between the lobes of the cerebellum. The cavity very probably contained some serum, but if so, it had escaped before it was laid open. The crus cerebelli had a dull, somewhat opaque yellowish colour, and was considerably firmer than natural, especially towards its under surface, where it bounded the cavity. This surface was also somewhat irregular, as if a small portion of it had been destroyed by disease. On cutting through its substance, there was found a coagulum of dark-coloured blood of the size of a duck-shot; the remainder of the cerebellum was healthy."

In this case, we have a remarkable pathological proof of a relation existing between the cerebellum and the instinct of reproduction, and the revival of the instinct and powers of propagation (which had for years been extinguished,) taking place on the accession of a disease of the cerebellum, and continuing active until the organ began to lose its firmness of texture, and to undergo disorganization, is strong confirmation of the evidence furnished by the two preceding cases, that this part of the brain is the source and centre of the instinct.

American Journal of Med. Sciences. Feb. 1839.

On the Physiology of the Iris. By James Bolton, m.d. of Baltimore.

By what means are the contraction and dilatation of the pupil immediately produced? The only modes of explanation adopted at present are the two following: the one that it is an erectile tissue, and the other that it is muscular, and composed of two sets of fibres, one radiating and the other circular. The first mode I do not think at all satisfactory, and its advocates are far less numerous than those of the second. The motions of the iris are by far too rapid to be accounted for in this way, and no contrivance has ever been discovered by which its sudden injection with blood and the sudden withdrawal of the blood can be accomplished. The dilatation of the pupil, when the head and face are gorged with blood, as in apoplexy, and the effects of injuries of the brain and its contraction when these are relieved by bloodletting, I consider an insuperable objection to this theory. Brodie has frequently observed the dilated pupils to contract after the abstraction of blood in cases of compression of the brain, and to dilate again as soon as the immediate effect of bloodletting had ceased.

We consider the point settled, that the iris contracts by a sphincter muscle. The only point then remaining to be settled is how the dilatation is produced. The advocates of the muscularity of the iris have generally considered that the dilatation must necessarily be produced by the same cause as that which produces contraction. It is true that many distinguished anatomists have admitted a set of radiating fibres, but we should be cautious how we attribute muscularity to any organ merely because it is fibrous. The cellular tissue may be drawn out into fibres, fasciæ: tendons, nerves, &c. are fibrous, but they are not therefore muscular. But, besides having a fibrous structure, the iris dilates: and these two circumstances, so far as they go, show that it possesses a set of radiating muscular fibres. Let us apply this theory to the phenomena. In amaurosis the pupil is permanently dilated. Now, can it be conceived that a muscle can remain for twenty, thirty, or forty years in a state of constant contraction? The idea is totally inadmissible. This disease is a paralysis of the nerve of vision, and in no way affects the nerves of the iris; we shall presently see why then the iris is at all affected in it. Belladonna and stramonium also cause dilatation of the pupil. This effect, when they are applied externally, is no doubt produced by their action being transmitted by those branches of the first branch of the fifth pair, which supply the conjunctiva and eyelids, to the ciliary branches of the same nerve. Do these substances, therefore, paralyze the circular fibres, without in the least degree affecting the radiating, and these latter then act constantly? I cannot conceive it at all probable that these powerful narcotics would thus distinguish between these muscles, when both (if they do exist) are supplied by the same nerves. I know this latter point also is contested, but there is no proof adduced of a different arrangement. It is merely an invention to explain a difficulty which can be much better explained in a different way. Besides, the theories of Magendie and Bellingeri, who contend for a different supply of nerves for each muscle, are directly opposed to each other as to which nerve goes to the contractor and which to the dilator. The iris has not been known to dilate from the galvanic stimulus. From all these circumstances we conclude that the idea of a dilator muscle in the iris is incompatible with some of its most important phenomena. Now, if we admit the radiating fibres to be elastic, we have an easy and satisfactory explanation of all the phenomena. In amaurosis the optic nerve no longer receives any impression, and none consequently is transmitted to the iris. The sphincter is therefore passive, and gives up the iris entirely to the power of the elastic fibres which dilate it. Expansion of the iris from belladonna is caused by a direct paralysis of the ciliary nerves, while the nerve of vision is not affected. A difficulty here presents itself, how the pupil is kept in a state of contraction for so many hours each day, if this be produced by a muscle. This difficulty, however, is readily explained. The sphincter is relieved from a state of constant contraction, 1st, by our passing through different shades of light, causing slight contractions and dilatations; 2d, by winking, which we do involuntarily every few seconds. When the eyelids are closed the pupil dilates, and on opening them it instantly contracts. This is the principal mode of resting the sphincter, and shows that the action of winking possesses a highly important use, besides that usually ascribed to it. That this momentary rest is sufficient is proved by analogy. The wings of some birds and insects move several thousand times in a minute, and yet the intervals between the contractions are sufficient to rest the muscles. 3d. During sleep, this muscle, together with the rest of the whole muscular system, rests and renews its strength. The eminent anatomist, Dr. Wistar, taught the same doctrine as that which I have advocated.

The following experiments will, I think, prove incontestibly the theory here supported. If a fresh eye be cut through parallel to the iris, and a little way behind it, and the front half be immersed in water, the elasticity of the iris may be proved by stretching it toward the pupil, and it will be found to resist and to return to its former position immediately on being relieved from this state of tension. The second experiment is still more conclusive. It occurred to me that if the mechanism of the iris were such as I have been endeavouring to prove, I might weary the contractor muscle by preventing the eye from winking for a considerable time. Accordingly, having a bright lamp placed about a foot from my left eye, I kept the eyelids open with the thumb and forefinger of the left hand· Then holding a mirror a few inches from the eye, I closely watched the iris. In a few seconds there was a smarting sensation, attended by involuntary attempts to wink. Immediately the pupil expanded and then quickly contracted. For two or three minutes alternate contractions and expansions took place incessantly, and then the pupil remained for a few seconds widely expanded, giving to the eye an amaurotic appearance. A partial contraction then took place, followed by an immediate expansion. In two or three seconds, contraction again took place, closing the pupil about as much as at the commencement of the experiment. The contractions and expansions were again incessant for about a minute, when the experiment was concluded. I have repeated this experiment several times on my own eyes and others, and always with similar results.

American Medical Intelligencer. 1838.

On the Pulsations of the Fœtal Heart and the Umbilical Cord in Utero.

By Prof. DUNGLISON, of Philadelphia.

[IT is well known that Dr. Hamilton, of Edinburgh, in his work on Practical Midwifery, has asserted that the fœtal pulse was never felt by him above 60, previous to respiration, whether examined in the region of the heart, or in the umbilical cord. This statement, so utterly at variance with the experience of all auscultators, is, probably, explained satisfactorily in the following extracts.]

To us, who have had so many opportunities of hearing the pulsations of the fœtal heart *in utero*, it would seem in the highest degree astonishing that an individual of Prof. Hamilton's experience could question the fact, were it not that he admits he has never attempted to verify or disprove it. It would be still stranger, however, were his observations accurate, that whilst the fœtal heart has been generally counted beating 130 or 140 times in a minute, the umbilical cord should not pulsate more than 50 or 60 times. Although satisfied of the inaccuracy of his deductions, we have repeated our own observations. Not many cases occur in which there is a good opportunity for examining this point. The best are those in which turning becomes necessary; but the practitioner is then so anxious to relieve his patient from suffering, and so absorbed with the operation, that the opportunity can rarely be embraced. Where the cord protrudes, this can be accomplished; and at times after the child is born. In these cases we can confirm the statement of our able friend, Dr. Meigs, in a note on this matter with which he has recently favoured us: who says, that he has very carefully observed the pulsations of the umbilical cord, while the ear was applied upon the region of the heart; and in every instance the pulsations were isochronous. The truth would seem to be, that in the cases examined by Prof. Hamilton, owing to the influence of the parturient efforts on the function of innervation, and through it on the circulation, the pulsations of the fœtal heart were unusually depressed; but in every case he would, doubtless, have found them isochronous with those of the umbilical cord, had he made the trial. It is obvious, indeed, that they must be so, seeing that the umbilical arteries are but a part of the circulatory apparatus of the fœtus. In a case observed by Dr. Vedder, an intelligent and zealous resident physician at the Philadelphia Hospital, whose name often occurs in these pages, and whose attention was directed to this subject at our request, he noticed that while the uterus was quiescent the pulsations numbered 140 per minute; but that immediately succeeding a pain, they were only 96, and then gradually rose to 140. After delivery, the cord and fœtal heart beat respectively 134 in the minute. The observations of Prof. Hamilton should not, therefore, be permitted to weigh with the observer. They are imperfect, inasmuch as the pulsations of the fœtal heart were not attended to whilst he numbered the beats of the cord; and, consequently, they in no respect conflict with the observations of almost every other obstetrical physiologist, that the sounds actually heard during pregnancy, referred to the fœtal heart, are owing to the pulsations of that organ.

In one of our recent German journals (*Neue Zeitschr. für Geburts.* B. vi. s. i.) we find an interesting communication elucidative of the matter by D. Von Hoefft, of St. Petersburg, detailing certain observations made on pregnant females in the Imperial Lying-in Hospital (*der Kaiserlichen Gebäranstalt,*) of St. Petersburg. Some of the conclusions of Dr. Hoefft are as follows:

a. In regard to the circulation between the mother and the child. From the great difference between the number of beats of the maternal heart and that of the fœtus, it is clear and evident that no community of circulation exists, as the pulse of the mother is ordinarily a third, or a half, and in rare cases, even two thirds less numerous than that of the child. Hence it is manifest, that after the death or cessation of the circulation, of the mother, the fœtus may continue to live for some time in utero, as experience has shown. Auscultation, consequently, has proved *biologically* what Hunter's preparations had exhibited *anatomically*.

b. The influence of uterine contraction on the circulation of the fœtus is exhibited in the most marked manner. During slight pains, the pulsations of the fœtus continue; but during more violent contractions, especially after the discharge of the waters, they are wholly interrupted, so that we may presume the fœtus to be in a state of temporary asphyxia in the last periods of labour, and that great danger may threaten the child if the pains continue for a long time without interruption. *American Med. Intelligencer. Jan. and April,* 1839.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Therapeutic Effects of Tobacco applied externally.

By Alexander Somervail, M.D. of Virginia.

I have suffered from diseased nerves in various ways. Some, such as I have never heard of, and which has introduced an application that I think may be useful in many distressing circumstances. The points of the two middle toes on each foot are affected with a burning sensation, increasing every afternoon, rendering washing painful, sometimes much so with a sense of swelling and weight, very unpleasant; the healthy feeling gone; this threatens my fingers too —when looked at, the point is a little red and painful to touch: it came into my mind to apply a snuff-plaster; this produced immediate relief; but I cannot leave it off for more than a week before the burning returns. Now one toe only resists this application, but is much relieved by it. I have often used this where I used to blister; lately I applied a twist of tobacco softened by boiling, but so as to retain the water to the side, in a case of pain in the side, with cough, fever, &c. in an aged woman, with speedy relief, and by the help of other remedies, recovery followed where I had no right to expect it. The tobacco applied thus is more powerful than the plaster. I have used this since the fever of 1815, occasionally—

no nausea, &c. is produced, unless the cuticle is abraded; in that case we cannot use the tobacco either way. I had a lady last June with severe pain in the face over the antrum; the plaster removed it, and was worn a couple of weeks. Perhaps tic-douloureux may be relieved by it. A negro man, in hewing timber, struck the corner of his axe into the inside of his knee, which lamed him for two months. When I saw him the cut was healed, the knee enlarged, the leg bent, very painful, and could not be extended. The cut appeared to have touched the tendon of the flexor. I applied the plaster; the pain soon mitigated; he got better daily, and soon admitted the use of a splint to extend the contracted muscles and is well.

The snuff-plaster was first prepared by using snuff in place of cantharides, in the preparation of blistering plaster; but as that is not adhesive enough, I have left out the wax in that plaster, and in its place put in empl. lytharg.

American Journal of Med. Sc. Feb. 1839.

Account of the Asylum for the Relief of persons deprived of the use of their Reason, near Frankford, Pennsylvania, with the Statistics of the Institution from its foundation (May, 1817) *to the end of* 1838. By CHARLES EVANS, M.D., Attending Physician to the Asylum.

[THIS is a valuable document, and happily illustrates the effect of the improved mode, now so generally adopted, of treating the insane. This Asylum belongs to the Society of Friends,* and was expressly instituted in imitation of the Retreat at York: it is the highest praise we can give it to say that it is worthy of its admirable model. Our limits will permit us to extract the principal statistical details only, and a few extracts from the description of the building, and the general mode of treating the patients.]

The centre building is sixty feet square and three stories high above the basement; having two wings standing back about eighteen feet from its front, each one hundred feet long by twenty-four feet in depth, and two stories high; terminating in end buildings which project four feet in advance of each wing, and are thirty-one feet four inches in front, by twenty-eight feet four inches in depth, and three stories high, exclusive of the basement. From each of these end buildings, a wing, running south, at right angles with the front, extends twenty-six feet eight inches in length, by twenty-two feet six inches in depth, and corresponding in height with the front wings.

The great extension of front (322 feet), in a building intended for the accommodation of but sixty-five patients, was deemed necessary in order to give to each a separate, well-proportioned room, having all the advantages to be derived from the free admission of light and air. Where the rooms are arranged on both sides of an entry of the usual width, these two essential requisites to health and cheerfulness cannot be commanded; added to which, the patients occupying opposite rooms are very liable to be mutually annoying, and in every respect (unless it be that of saving money), that mode of building for the insane is highly objectionable. On this account, the plan adopted at the Friends' Asylum is worthy of imitation. On one side of the wings are situated the chambers, ten feet square, each having a window, four feet six inches in height by two feet ten inches in width. These rooms open on to the loby, ten feet wide; and directly opposite the door of each room is a window corresponding in size with that in the room. Over each door is fixed a cast-iron sash, thirty-two by twenty inches in size, fitted with a moveable glazed sash, to be opened or shut at pleasure. By this arrangement, a full supply of light and a free circulation of air are secured; and the lobbies being comfortably warmed in cold weather, they afford pleasant places for walking and exercise of different kinds.

Connected with the various buildings described, is a farm of sixty-one acres, the greater part of which is under cultivation, and by giving the patients the opportunity for various agreeable and active out-door employments, affords the most powerful means for their restoration to health and reason. The woodlands cover about eighteen acres of ground, and are made up principally of the chesnut, beach and oak, affording a deep and delightful solitude and shade. A broad serpentine walk, more than a mile in length, winds throughout them, and a large summer-house and seats in various situations, are provided for the accommodation of the patients. Near the entrance to the woods, and inclosing a small part of them, is a park containing some fine deer.

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* In 1834, the Asylum was opened to the public generally.

In the treatment pursued at the Asylum, endeavours are used, so as to combine medical and moral agents, that each shall render the other its most efficient aid, and jointly exert their remedial powers with the greatest certainty and effect. The therapeutical treatment of course varies according to the disease, which, by affecting the brain, disturbs the manifestations of the mind. An accurate account of such treatment and its results, is constantly kept, and at some future day may afford data for ascertaining the relative advantages of the course pursued.

The moral means employed are various. Where it is found necessary, mild and gentle yet firm restraint is imposed, while the earliest gleams of returning reason are watched and cherished.

In the house, there are provided games of different kinds; reading, writing, drawing, &c. The females sew, knit, quilt, &c. The library is furnished with books, periodicals, drawings, &c. Exercise in the open air is always promoted, and the patients encouraged, whenever the weather will permit, to engage in walking and riding. A carriage and horses are always in readiness, morning and evening, for their accommodation. In the lawn fronting the house, is located a circular rail-road about 450 feet in circumference, with a pleasure-car on it, large enough to accommodate two, which is moved by hand. Riding upon this road is a very favorite amusement, and as it is attended with considerable exercise, it is found highly advantageous. Every exertion is made to interest the male patients in gardening, and in the various employments afforded in the cultivation of the farm. The diet of the patient of course varies according to the prescription of the physician, but in general it is plain and nutritious; fresh meat and a variety of vegetables being served up every day. Tea, coffee, and milk, are all abundantly supplied.

Whole number of admissions, - - - -		634
" Men - - - -	331	
" Women - - - -	303	
" Single - - - -	326	
" Married - - - -	234	
" Widowers - - - -	17	
" Widows - - - -	57	
Of these there were below 20 years of age - -	28	
From 20 to 30 years - - -	187	
" 30 to 40 " - - -	141	
" 40 to 50 " - - -	126	
" 50 to 60 " - - -	83	
" 60 to 70 " - - -	48	
" 70 to 80 " - - -	15	
" 80 to 90 " - - -	5	
" 90 to 100 " - - -	1	
		634

Of these 634 admissions, 127 were readmissions granted to 81 individuals, and leaving 507 *persons* who have been under care. The following table shows the duration of the disease at the time of admission of these 507 cases, and the results of treatment:

Duration.	Number.	Restored.	Much Imp.	Improved.	Stationary.	Remaining.	Died.
Less than 1 year	261	152	26	27	18	4	34
From 1 to 2 years	57	18	8	8	9	7	7
From 2 to 3 years	36	17	3	3	4	5	4
From 3 to 5 years	45	14	7	6	9	3	6
From 5 to 10 years	47	13	7	3	8	11	5
Over 10 years	61	0	7	5	22	13	14
Aggregate	507	214	52	52	70	43	70

The proportion of cures in these cases is 42·21 in every hundred; but if we deduct the sixty-one cases which at the time of admission had been deranged over ten years (and which included twenty who either were idiots, or had been imbecile from puberty), five cases complicated with epilepsy, and five which entered the institution with the paralysis peculiar to the insane, it leaves 436 cases, properly subject to treatment, and the cures are in the proportion of 49 in every hundred. The per centage of cures in cases of less than a year's duration, taking the whole twenty-two years, is 58·23. Within the last six years, it has been 66. Nearly all of this class, discharged as "much improved," were almost well; but either pecuniary considerations, or the anxiety of their friends, occasioned their removal as soon as the disease was so far overcome as to render their perfect restoration probable; and in many instances information was afterwards received of their perfect recovery.

Of the seventy deaths, six occurred within a week of the time of their admission; nine within two weeks; seven within three weeks; and three within four weeks; these were mostly cases of acute inflammation of the brain, or its meninges, many of them being brought to the Asylum after all hope of relieving them at home was abandoned. Ten died between a month and a year's residence, and the remainder varying from one year to twenty.

Of the eighty-one patients re-admitted, there were discharged

Restored - - - - - -	36
Much Improved - - - - -	4
Improved - - - - - -	6
Stationary - - - - - -	7
Died - - - - -	17
Remaining in the House - - - -	11

Twenty-two returned a third time; of whom there were discharged—Restored, 14; improved, 3; stationary, 3; died, 3; and one remains in the house. The other readmissions were of three individuals who, being liable to periodical insanity, have been accustomed to resort to the Asylum at the commencement of an attack, and to remain there until again restored to the use of their reason.

American Journ. of Med. Science. May, 1839.

SURGERY.

Cases of Sensitive Tumours of the Female Urethra, with Remarks.

By ALEXANDER E. HOSACK, M.D.

IN May, 1835, I was consulted by a servant woman, in a family where I was in attendance, for a complaint which she said had caused her considerable distress, and, as she expressed herself, it appeared as if something had dropped into the passage immediately after making water, causing her great pain at the moment, and which frequently bled, particularly upon being touched by her linen. Upon the slightest exertion she was seized with bearing-down pains to such a degree as to compel her to take to her bed. These difficulties, she said, had been gradually increasing upon her for two or three years, and being unmarried, she was from delicacy induced to conceal her sufferings until no longer able to bear them.

From this statement I was induced to make an examination, which clearly explained the cause of all her trouble. I discovered two or three little tumours immediately within the meatus urinarius, to which they were attached by a narrow neck. They were of a florid red colour, and appeared to be covered by the delicate lining membrane of the urethra. They were exquisitely sensitive and bled upon the slightest touch. In form they resembled a split pea, varying from that in size to a small kidney bean, and placed upright, in such a manner, as to break the flow of urine. The patient did not however complain of the pain upon urinating as her greatest distress, for it was not to be compared to that caused by exertion, or from contact of the dress, which was frequently excruciating.

By raising these tumours with a probe, I discovered their attachment to be limited to the margin of the urethra, and suggested to her the propriety of having them removed, which I assured her could be readily done, and with comparatively little pain. Having obtained her consent, I snipped them off with scissors: the hemorrhage was not excessive. In a few days the part was healed, and she appeared to be completely rid of the evil, until about six weeks after, when the sensitiveness and other symptoms returned. In the course of three months I was again requested to relieve her if possible by a further operation. Upon examination, I found the margin of the urethra fringed with the same highly-organised structure. It appeared as if the lining membrane had been prolapsed and was turgid with blood; or in other words, had shot out like a fungus. Under these circumstances I determined to remove the diseased structure by excising the meatus urinarius, and this was accordingly done. The wound in due time was healed, leaving the parts apparently sound, with the exception of a few spots of discoloration in the folds of the nymphæ, which I afterwards destroyed by caustic. The extremity of the urethra remaining somewhat harder than might have been expected in sound parts, I expressed doubt whether it might not be the incipient stage of scirrhus. The disease, however, in the course of a few months returned with all the distressing symptoms as before enumerated. The patient being again willing to submit to any operation that I might advise, I determined to remove the urethra to an extent that would hold out a better prospect of success. My friend, Dr. Wilkes, with whom I consulted, confirmed this opinion, and assisted me in the operation. The patient being placed upon the bed in a recumbent position, with the legs flexed upon the body, I began with measuring the length of the urethra, by introducing the female catheter, and marking it the instant the urine began to flow; this precaution I considered necessary, from the fact that the length of the urethra, in females, is very variable; at the same time, I was unwilling to encroach too much upon the bladder, which might endanger consequences more distressing to the individual than the existing disease. The preliminaries being attended to, I seized the fungous excrescence with the *pince* of *Museux*, and drawing it out, I circumscribed the urethra with a knife, carried on the dissection until I had detached about three quarters of an inch in extent, as I supposed. I then examined the urethra at the upper extremity of the wound, and finding it perfectly natural and free from all hardness, I separated it at that point. The hemorrhage for the moment was very great; but by pressure, constantly kept up with a compressed sponge, it was arrested, or so much restrained, as to do away with all anxiety on that account. The patient having made water a short time previous to the operation, I did not consider it necessary to leave a catheter in the bladder, which I afterwards regretted, as I was obliged to draw off the urine the following morning, but not without considerable difficulty, as may be imagined. I determined, however, for the future, to leave the catheter in the bladder, or at least until the urine should flow at its side; which took place on the sixth day, when I removed the instrument. Since which time she has enjoyed full control over that organ, and voids urine with comparative ease.

It is now six months and no return of the disease. No bougie was introduced to keep open the mouth of the urethra, as might *a priori* have been considered necessary. Indeed, I purposely avoided using it, lest the irritation might predispose the parts to a return of the disease. Upon examining the part removed, I found the urethra to be very much thickened and hardened at its extremity, but this circumstance not being observed in other instances of this disease as related by different authors, I must conclude that it had no agency in the growth of these tumours, but was probably the result of irritation.

I first met with this disease in the practice of my friend, Dr. Mott, who, several years ago, was consulted by a gentleman on account of his daughter, who laboured under this distressing complaint. The case was one of great interest, both from the circumstance of the patient being at the delicate age of eighteen, and on the eve of marriage. She had suffered from this disease for two years and upwards, and, considering it an insurmountable objection to marrying, had frequently deferred the nuptial ceremonies, at the same time not willing to break off her engagement; and, unable any longer to conceal her actual situation, she disclosed the true cause to her father, the only surviving parent, who immediately came to New-York, and placed her under the care of Dr. Mott.

In this case, Dr. Mott, after carefully examining the disease, determined upon removing the *meatus urinarius*, to the margin of which two or three small flattened and vascular tumours were attached. They were of the size of small beans, highly florid, and exquisitely sensitive. The wound healed kindly after the operation; the result was perfectly successful, when she returned home to her friends and afterwards married.

REMARKS. Although this disease is one of comparatively rare occurrence, much has been written upon it; still, elaborate works on surgery and midwifery have not, with but one or two exceptions, in any way noticed its existence. I confess it appeared to me to be quite a novelty; and as regards the excising of part of the female urethra for its removal, or for any other object, I do not recollect ever to have heard of a single instance; nor have I yet been able to discover that it has been done to any extent, beyond the mere margin of the external orifice of the urethra. This disease is first spoken of by Morgagni, who, under the head of excrescences and other diseases of the female urethra, remarks: "examining the body of an old woman, about the beginning of 1751, I met with a small triangular excrescence within the external orifice of the urethra, but it was not prominent;" and, in another part of the same chapter, he goes on to state, "that there is a red and fungous excrescence, which is of the size of a bean, sometimes to be observed attached to the orifice of the urethra." This disease is also described by an Englishman by the name of Hughes, of Stroudwater, in Gloucestershire, in 1769. In a case described by that gentleman, he speaks of it as of "a red colour, and of a softish, spongy texture, with an irregular, jagged surface; was sore when touched; and a bloody serum oozed from it." The patient was eleven years of age, and of a very thin habit of body; it had existed for three years. In this case, Mr. H. removed the *meatus urinarius*, which completely included the disease. The patient suffered for some time from retention of urine; only, however, during the healing process. Five years had elapsed since the operation, and the patient continued perfectly well.

"On examining the fungus, after the operation, it appeared about the size of the nipple of an adult; its anterior part being expanded, formed, as it were, a little cup, with its border indented like a cock's comb, having a hole in its bottom which was the orifice of the urethra, which ran through the body of the fungus; the internal membrane of the urethra was continued to the edge of the indented border, which was of a deeper red colour and softer texture than the other part of it." In volume xiii. page 784 of the Lancet, Mr. Wardrop has published four cases of this disease. The first was in a young girl thirteen years of age; the second in a lady of thirty years of age; the age of the third is not given; the fourth was upwards of sixty years old. In all of these cases Mr. Wardrop speaks of the exquisite sensitiveness as the most prominent symptom. In one of the cases above alluded to, the disease returned after marriage, when the patient again applied to Mr. W. for relief. He states "that the tumour had now assumed the appearance of a bright scarlet fungus, encircling the meatus, and was attended with such exquisite tenderness as to prevent sexual intercourse. The orifice, including the disease was removed, and it did not return."—Boyer makes mention of a fungus occurring in the female urethra. It is however noticed in a more particular manner by Bromfield, and according to Mr. Hughes, accounts are also given of it by Sharp, Warner, and Jenner. Mr. Wardrop also refers to Chaussier and Dubois, and states that it is particularly noticed by Madame Lachapelle as well as by Rosenmuller, Vogel, Kaldibrand, Drochaska, and some other German pathologists. In many of the instances referred to by the older writers, the symptoms were at first mistaken for those of stone in the bladder, and in the case just related by Mr. Hughes, the disease was mistaken by those who were first consulted for prolapsus of the uterus, and actually treated as such, nor was the error discovered until the patient was unable any longer to bear the pain consequent upon the pressure applied to that viscus.

By reference to the foregoing cases we arrive at the following conclusions:

1st. That the disease is characterized by peculiar symptoms.

2d. That it is not confined to any age.

3d. That it is unaccompanied with discharge, unless the parts be chafed or abraded.

4th. That, in order to prevent a return of the disease, it is better to remove, at once, the external orifice of the urethra, including the tumours.

5th. That it is a complaint of slow growth, and does not attain to any great size; for, in no instance yet recorded, so far as I am enabled to learn, has it been found larger than a small cherry.

Note. The only inconvenience to be expected from removing the meatus urinarius, will be that of retention of urine. This is not, however, a necessary consequence; for among all the authorities quoted, but two cases are mentioned in which it occurred; one related by Mr. Hughes, and the other by myself. In both instances they were spontaneously relieved, on the fifth or sixth day.

[The only remark we would make on the above interesting communication, is to express a doubt whether the less formidable operation of the destruction of the parts by some form of caustic should not have had a trial.]

The New York Journal of Medicine. July, 1839.

*** This is the first number of a new Quarterly Journal, published in New York, by our own highly respectable publisher, Mr. George Adlard. The present specimen augurs well for the future importance of the work, to which we wish every success. The plan of the journal is good; its contents highly creditable to the contributors; and the *getting-up* equal to that of our best English periodicals. It is about one fourth or one fifth less in size than our own Journal, and sells for about one sixth less price. We hope often to profit by its original articles.

Views and Treatment of an important Injury of the Wrist.

By J. Rhea Barton, M.D.

The accidents which are to be the principal subject of my remarks, usually pass either for sprains or dislocations of the wrist. Under one of these denominations are these cases to be detected, which, though partaking somewhat of the character of sprains or dislocations, are distinguishable from either of them respectively. They may be recognized by their being accompanied by more distortion of the hand and arm than any which can arise from simple sprains of the wrist, and yet less than that which must necessarily take place when there exists a complete luxation of the carpus. The profile of the limb under this injury is a peculiar one, distinguishing it on the one hand from the sprained wrist, and on the other from luxation.

In simple sprains of the wrist, though accompanied by extreme swelling, the limb will still be found to retain a characteristic outline of its natural contour. It is not marked by any abrupt and solid eminences, the swelling is rather uniform, diffuse, and puffy, the hand continues on the same line with that of the forearm, &c. In complete dislocations, the nature of the injury must always be very palpable from the great bulging of the overlapped bones, and from the shortening of the limb, &c. Between these two injuries, there is too great a dissimilarity to admit of an excuse for the surgeon who mistakes the one for the other: but he may confound with these, and it is a common fault to do so, *a sub-luxation of the wrist, consequent to a fracture through the articular surface of the carpal extremity of the radius;* although to this accident belong appearances exclusively its own. It is to this peculiar injury that I wish to draw attention.

It is one of the most common injuries to which the upper extremities are subjected; and every practitioner of moderate experience will, I am sure, be able to call to his recollection the appearance which the limb presents under such circumstances, as well as the embarrassment which he has experienced in his attempts to obviate eventual deformity, to preserve the functions of the fingers, and to restore the motions of the wrist and forearm.

The similarity of manner in which this accident generally occurs is striking. It is almost always found to have taken place in consequence of the individual having thrown out his hand to rescue himself from falling, or to ward off injuries threatening a more important part of the body. In the act of falling, for example, the hand is thus instinctively thrown out, and the force of the fall is first met by the palm of the hand, which is violently bent backwards until the bones of the wrist are driven against the dorsal edge of the articulating surface of the radius, which, being unable to resist, it gives way. A fragment is thus broken off from the margin of the articular surface of this bone, and is carried up before the carpal bones, and rested upon the dorsal side of the radius; they having been forced from their position, either by the violence or by the contraction of the muscles alone. We have then an imperfect luxation of the wrist, depending on a fracture through the extremity of the radius. The deformity will be found to correspond with the state of the case. There is a tumour on the dorsal side of the arm formed by the bulging of the carpal bones and fragments; whilst below it, on the palmar side, the extremity of the radius projects. The degree of prominence of these parts, depends upon the size of the fragment and the violence of the injuring force. The ulna not being very intimately involved in the injury, retains its position, and serves as an abutment, against which the hand seems to rest; whilst the radius, as it has its edge broken off, allows the hand on that side to be drawn upward, and hence to render, on the under side, the styloid process of the ulna more conspicuous than natural. Crepitus cannot always be felt, sometimes in consequence of the smallness or crushed condition of the fragment; at other times, owing to the great swelling and tension; but in every such case, the distortions of the limb are to be seen, and may be removed by making firm extension and counter-extension from the hand and elbow, at the same time gently depressing the tumours already spoken of. By the employment of these means, all deformity, except that which evidently depends upon the more general swelling, may be satisfactorily removed; but the moment the extension and counter-extension are relaxed, the combined action of the flexors and extensors of the fingers, as well as those of the wrist, force the deformity to reappear as conspicuously as before: and as often as the effort is renewed and discontinued, will the deformity appear and disappear. In this respect does this species of injury in an especial manner differ from a complete simple luxation of the wrist, which, when once reduced, must continue so after the reducing force has been withdrawn. There is no spontaneous reluxation after the simple complete dislocation has been removed; whereas, in this case it immediately succeeds the withdrawal of the force. This accident must not be confounded with those which are also of frequent occurrence, namely, fracture of the radius, or of the radius and ulna just above, and not involving the joint. It will be found, on referring to the writings of Boyer, Desault, Sir Astley Cooper, Dupuytren, and many others, that this frequently happens, and that the fracture often reaches to within a few lines of the extremity of the bone; and that these cases are very frequently mistaken for dislocations, though they are in reality fractures exterior to and disconnected with the joint; the deceptive deformity being occasioned by the displacement of the broken ends of the bone caused by the action of the muscles and the weight of the hand.

The fragment may be, and usually is, quite small, and is broken from the end of the radius on the dorsal side, and through the cartilaginous face of it, and necessarily into the joint. The pronator quadratus is not involved in the fracture. The radius and ulna are not materially disturbed in their relations to each other The only important change, which takes place in consequence of this fracture is, that the concave surface at the extremity of the radius, which receives and articulates with the first three carpal bones, is converted, as it were, into an oblique surface by the loss of a portion of its marginal ridge; commonly by the separation of an entire piece; sometimes by the crushing of its substance. The moment the cartilaginous extremity of the radius is deprived of its concave form, the united force of the carpal and digital flexors and extensors is exerted to create a complete luxation; but as the ligaments are only stretched, or but partially torn, this cannot take place. The carpal bones, therefore, only emerge collectively from their natural position, and carrying before them the broken piece, rest on the dorsal side of the radius, forming a tumour there; whilst the end of the radius itself occasions on the palmar side a prominence which is round and smooth, and differing in this from similar projections formed by the fractured ends of bones, the abruptness and harshness of which may sometimes be distinctly felt through the soft parts, and which are themselves, when pressed upon, acutely painful.

It follows of course in injuries of this kind, that unless some method of dressing be adopted whereby the retraction of the hand may be permanently counteracted, and the prominences repressed, the patient will recover with a crooked arm, and under a sacrifice of some of the functions of the hand. The customary modes of treating either sprains or dislocations of the wrist, or fracture of the forearm, are totally inadequate to the purpose, and should not be relied on as a treatment for these particular cases by any practitioner who has regard for the welfare of his patient, and for his own reputation. There is no professional point upon which I can more confidently express myself, than upon the errors committed in the treatment of these cases,—passing, as they commonly do, for sprains of the wrist, and hence treated as such. After an unvarying success in the management of this accident for many years in the Pennsylvania Hospital, in the Blockley Hospital, and in private practice, I can strongly recommend the following plan of treatment: Two thin, but firm splints of wood, are to be prepared, of sufficient length to extend from just below the condyles of the os humeri to the ends of the fingers, and of width enough to embrace the sides of the limb. These are to be lined on one of the sides with carded cotton, or something equally soft, and wrapped with a bandage. Two compresses, each about two inches square, and composed of strips of bandage, about one yard and a half long, evenly folded up, are also to be in readiness. The arm is then to be flexed at the elbow, and one assistant is to hold it firmly above the condyles, whilst another makes extension from the fingers. The surgeon now presses the prominent end of the radius on the inner side, and the bulging carpus and fragment on the outer side, into their respective places. The roller is then to be lightly pressed around the hand and arm, securing in its course up the limb one of the compresses precisely over the carpus and back of the hand, and the other with equal precision over the palmar side of the radius just above its carpal extremity. These compresses, when properly arranged, will be found *not opposite to each other, but the inner one commencing on a line opposite to that on which the outer one has terminated.* These being applied, the inner splint is next placed against the limb,—the assistant shifting his hand to admit of this being done, without his relaxing in the least degree the extension until the limb is bandaged to this splint, when it will be found that the extension is well maintained. The outer splint is now to be applied and secured to the arm by the return of the roller. The principal use of the latter splint is to act upon the outer compress, and by its general pressure to weaken for the time the force of the resisting muscles. By the employment of these simple means, the indications in the treatment of this accident will be found to be fully met. The arm may be carried in a sling, and the patient permitted to walk about, &c. In three or four days the limb should be undressed and inspected; and whilst held so that relaxation cannot take place, the wrist and fingers are to be bent enough to preserve the flexibility of the joints. The dressings are then to be reapplied. These operations are thenceforward, for four or five weeks, to be repeated every day, adding to them the motions of pronation and supination.

The practice of keeping a limb in splints, with the joints in an immoveable state for weeks, even when the fracture is remote from the articulation, cannot be too earnestly deprecated; and in cases where the injury to be repaired has involved a joint, such treatment is censurable to a high degree, as it is almost certain to destroy the mobility of it by promoting the adhesion of ligaments, the union of tendons with their thecæ, and by obliterating bursæ—evils never to be fully repaired. *Philadelphia Med. Examiner. Nov. 7,* 1838.

On the Use of Iodine in Herpes. By G. BILLINGSLEA, M.D.

FOR the last twelve years I have used the alcoholic tincture of iodine as a local application in those troublesome cases of herpes circinatus or ringworm so common in our southern country, with the happiest effect. Indeed I do not recollect any case in which it has been used without a radical cure. As I believe it is not generally known to our medical public, not having seen it recommended by any author in similar cases, you may confer a favour on some of the readers of your valuable periodical by inserting this notice.

American Med. Intelligencer. May, 1839.

MIDWIFERY.

On the Temperature of the Vagina and Os Uteri during Labour.
By Prof. DUNGLISON, of Philadelphia.

THE remark of Dr. Granville, that the temperature of the uterine system during parturition, sometimes rises as high as 120° of Fahrenheit's scale, has always struck us as needing further confirmation. We have often been impressed with the seemingly elevated temperature of the vagina under these circumstances, but have always suspected inaccuracy in the observations of Dr. Granville, not only because the temperature he indicates is so much higher than has ever been noticed in any condition of the system, or of any organ, but because the results of our own experiments have not shown that the temperature is *really* much elevated in the cases in question.

The following results of observations made at our request by Dr. Barnes, one of the Senior Resident Physicians of the Philadelphia Hospital, Blockley, so far as they go, confirm our own. They likewise exhibit the ratio of the pulsations of the maternal and the fœtal heart at the times of observation.

OBS. I.—Pulse, 84; fœtal heart, 130; temp. within labia, 100°; temp. at os uteri, 100°.

This is the *average* result of a series of thermometric observations, made during a space of twenty-five minutes; six hours after the commencement of true labour pains, and one hour previous to the delivery of the child. The patient was in labour with her first child, of an exanguious habit—with a hereditary predisposition to phthisis.

OBS. II.—Pulse, 72; fœtal heart, 120; temp. at labia, 100; temp. within os uteri, 102.

The result of a single observation made, in the case of T—— A——, twelve hours after the commencement of regular and severe, but not propulsive pains. The patient is stout and muscular, of short stature, and of intemperate habits. A few minutes after making the first observation, the pains ceased entirely, and did not recur until twenty-four hours after.

OBS. III.—Pulse, 73; fœtal heart, 128; temp. at labia, 105; temp. within os uteri, 106.

The *average* result of a series of observations made during a space of two hours, commenced fourteen hours after first labour pains, and terminating with the delivery of the placenta. The patient was of a full and corpulent habit, and in labour with her first child. *American Med. Intelligencer. Feb.* 1839.

MEDICAL STATISTICS.

Classification of Fracture Cases treated at the Native Hospital, Calcutta, from 1815 *to* 1837. *Communicated by* J. R. Martin, Esq., *Surgeon to the Institution.*

Fractures of the skull	70
Ditto nose	12
Ditto lower jaw	26
Ditto cervical vertebræ	2
Ditto clavicle	59
Ditto scapula	2
Ditto acromion process of the scapula	1
Ditto sternum	8
Ditto ribs	64
Ditto spine, with displacement	9
Ditto of the pelvis	2
Ditto humerus	235
Ditto of the condyles of the humerus	2
Ditto radius and ulna	340
Ditto into the elbow-joint	9
Ditto into the wrist	20
Ditto of the metacarpal bones	2
Ditto fingers	10
Ditto neck of the thigh-bone	14
Ditto trochanter major	1
Ditto thigh-bone	292
Ditto patella	22
Ditto tibia and fibula	343
Ditto into the ankle-joint	9
Ditto of the metatarsal bones	6
Ditto of the toes	12
Total	1572

Diseases of House-Patients treated at the Native Hospital, Calcutta, from Sept. 1, 1800-1, *to Aug.* 31, 1836-7.

Diseases of the digestive organs	273
Ditto respiratory ditto	97
Ditto circulation	2
Ditto brain and nervous system	344
Ditto generative and urinary organs	504
Febrile diseases	1547
Cholera	540
Dysentery	310
Rheumatism	533
Dropsical diseases	303
Cutaneous ditto	43
Various fractures and other injuries requiring operations	7112
Hydrocele cured by radical operation	162
Ulcers	1382
Miscellaneous cases, chiefly surgical	1372
Total	14,524

N.B. It is proper to mention, for the information of strangers to the nature of the institution, that it has but 50 beds, and was founded solely for the relief of "persons suffering from accidents," and that medical cases are admitted only when the condition of the wards admits of it. A dispensary is attached to the hospital, affording relief to an average of seventy-five thousand per annum.

Calcutta Quarterly Journal. January, 1838.

III. THE BRITISH JOURNALS.

(FOR THE QUARTER ENDING AUGUST 31, 1839.*)

ANATOMY AND PHYSIOLOGY.

On the Structure and Functions of the Spleen. By THOMAS G. HAKE, M.D. *Communicated by* FRANCIS KIERNAN, Esq., F.R.S.

THE author, passing in review the various opinions which have been advanced by anatomists respecting the intimate structure of the spleen, arrives at the conclusion that hitherto only vague and premature inductions have been made. It is generally admitted that the fibrous envelope of this organ is formed of the external fibres of the splenic vein; and that from the internal surface of this envelope fibrous prolongations are continued into the interior of its substance, giving support to a fine cellular membrane, which is continuous with their edges, and variously reflected so as to constitute cells. The parenchyma, or solid structure of the spleen, everywhere accompanies these membranous productions, and forms the exterior walls of the cells, being composed of branches of the splenic arteries, of the granular terminations of those arteries constituting the *splenic grains* of Malpighi, of *venules*, which ramify around the splenic grains, and of *cellules*, into which the venules open, and from which the splenic veins take their rise. The author concludes, as the result of his enquiries, that a dilatable cellular tissue exists, containing venous blood, between the granules within which the arteries terminate, and the venules on the outer side of the splenic grains: that the venous membrane, which is continued from the cells to the cellules, as well as to the venules, becoming more and more attenuated, but without changing its essential structure, gradually loses its tubular form, and resumes its primitive character of cellular tissue; and that the artery, in like manner, is limited in its distribution within the granules by a cellular structure, which becomes vicarious of it, and determines the function it has to perform.

The author, in conclusion, offers some observations on the probable functions of the spleen. He considers the opinion which supposes that organ to be distended, at particular times, with arterial blood, as being completely refuted by the evidence derived from the preceding account of its minute structure; and suggests the probability of the spleen being rather a diverticulum for venous blood.

The paper is accompanied by seven highly-finished drawings, illustrating the structures described.

Proceedings of the Royal Society. June 20, 1839. No. 39.

On the Difference of Colour in different parts of the Bodies of Animals. By JAMES ALDERSON, M.A. M.D., late Fellow of Pembroke College, Cambridge. *Communicated by* P. M. ROGET, M.D., Sec. R.S.

THE hypothesis advanced by the author in explanation of the well-known partial absence of the coloured pigment or *rete mucosum*, in different parts of the human body, and that of other animals, is that it is due to the union or adhesion of the epidermis and the true skin, so as to exclude the rete mucosum. He supports this hypothesis by the analogy of a cicatrix, which is the result of an organization of a certain portion of lymph, poured out from the cut surfaces of a wound, as part of the process of nutrition, or as the consequence of a small amount of inflammation, induced either from mechanical irritation, or other accidental circumstance. This hypothesis was suggested by the colourless appearance of the cicatrix from the section of the umbilical cord in the negro, and also of that seen by the author at the umbilicus of the bottle-nosed whale, the *Hyperoodon bidentatus*. *Ibid.*

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* Owing to the large amount of our other materials, we are forced greatly to contract this department of the Journal for the present quarter.—ED.

On the Blood-vessels of Tendinous Tissues. By James Paget, Demonstrator of Pathological Anatomy, and Curator of the Museum at St. Bartholomew's Hospital.

In this paper, which is very creditable to the author, Mr. Paget demonstrates the distribution of the larger vessels in tendons, which does not seem to have been previously effected; although, as Mr. P. observes, it does not appear why this has not been done, as he succeeded by using the common material for injection, size and vermilion. "The vessels in the substance of the tendon run in straight and parallel lines, from one end of the tendon to the other, between the fasciculi, rarely giving off branches and rarely anastomosing. Of the few branches which are given off, the greater number separate gradually and at a very acute angle from the trunk, and then pursue their course parallel to it; but occasionally a branch passes transversely across the intermediate tendinous fibres, from one vessel to another adjacent to it. The vessels of the substance communicate but rarely with those of the sheath of the tendon, and are derived from the vessels of the muscle or of the part in which the tendon expands to be inserted; so that I have sometimes succeeded in completely injecting both ends of a tendon, while the vessels of the middle portion remained empty. Each artery is accompanied by a single vein." *Lond. Med. Gazette. July*, 1839.

On the Effects of Lesion of the Trunk of the Ganglionic System of Nerves in the Neck upon the Eye-ball and its appendages. By John Reid, M.D., Fellow of the Royal College of Physicians, Edinburgh.

This is a brief paper, containing the details of several experiments on living animals, recently instituted with the view of determining a doubtful point, noticed in a previous publication. We have not room for the experiments, but the results obtained are stated in the preliminary observations, which we shall extract.

"In a former communication in the Edinburgh Journal (for January, 1838, p. 132,) I stated that I had frequently verified the observations of Petit and others, that when the vagus is injured in the neck in those animals in which the sympathetic is combined with the vagus, as in the dog,—that the conjunctiva becomes inflamed, the pupil contracts, and the eyelids are somewhat more closely approximated to each other. At that time I suggested that the contracted pupil and partially closed eyelids might probably depend upon the impatience of light which sometimes accompanies inflammation of the conjunctiva. I have since attended more carefully to this subject, and, during the last summer, satisfied myself that the contraction of the pupil, the projection of the cartilaginous membrane, or third eyelid, situated at the inner angle of the eye over the cornea, and the partial approximation of the eyelids to each other, take place immediately after the injury of the sympathetic, and before the inflammation of the conjunctiva presents itself, and that they continue after it has disappeared."

Edin. Med. and Surg. Journal. July, 1839.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Contributions to Intra-Uterine Pathology. Part II. *On the Inflammatory Origin of some varieties of Hernia and Malformation in the Fœtus.* By James Y. Simpson, M.D., Fellow of the Royal College of Physicians, Edinburgh, Lecturer on Midwifery, &c.

This is a most important article; and we regret that we are unable to give any portion of it in our pages. It is a continuation of a former paper noticed in our Thirteenth Number (p. 284); and its object is to trace "the *indirect* morbid effects which occasionally result from peritonitis in the fœtus," described in the former part. The author here considers, 1st, the pathological relations of fœtal peritonitis in the production of certain congenital forms of umbilical, diaphragmatic, and inguinal hernia; 2dly, endeavours to trace the primary origin of some varieties of malformation among the abdominal and pelvic viscera to the same proximate cause; 3d, notices the dependance of other species of malformation upon the effects of inflammatory action in the early embryo.

We recommend this paper to the particular attention of our readers.

Edin. Med. and Surg. Journal. July, 1839.

The Result of Experience in Sea-Scurvy, with Remarks on its Prevention and Treatment. By ANDREW HENDERSON, M.D. Surgeon, Royal Navy.

THIS paper contains some new matter, and a good deal of information that will be valuable to his junior brother officers in the navy, and to surgeons in ships of commerce. Dr. Henderson has performed seven voyages in charge of male convicts to Van Diemen's Land and New South Wales, with results as to health which testify strongly to the excellence of his arrangements. The author, like all well-informed surgeons and captains of the present day, trusts much more to good diet, good habits, and cleanliness, than to medicaments, for the prevention of scurvy. He denies all efficacy to lemon-juice, both as a preventive and means of cure. His remedy is *nitre*, which he gives to the amount of from two to four drachms in the course of the day, dissolved in six or eight ounces of water, in divided doses. "In this way, I have often with much satisfaction remarked a visible improvement in three days; but, in general, a much longer time is required to bring about a favorable change. I have frequently known patients very much averse to the medicine at first, saying that it caused vomiting, purging, and so forth; but never knew one attempt to refuse it after the second or third day, all coming to the hospital cheerfully at the appointed time to get their allowance, and often pushing one another aside, trying to get first in for the expected dose. The relief it affords to the sinking at the pit of the stomach, so very much referred to in *purpura*, is almost incredible. Indeed, had I not so often witnessed the remarkable change for the better in a short time, I could not credit the accounts given by the patients themselves, raised as it were from death's door to a state of ease and returning health." *Edin. Med. and Sur. Journal. July*, 1839.

Observations on the Treatment of Delirium Tremens, without Opium. By T. CAHILL, M.D.

ALTHOUGH confident, from a good deal of experience, of the infinite value of opium in many cases of delirium tremens, we have ourselves witnessed so much evil from its indiscriminate and exclusive use, that we gladly receive Dr. Cahill's criticisms on the practice. The original article contains the details of seven cases, which are intended to illustrate, and do illustrate, the proposition maintained by the author, "that opium is not beneficial in many cases; in others, that it is positively injurious; and that in all a cure can be effected without its assistance."

Dublin Journ. of Med. Science. July, 1839.

History of a singular Convulsive Disease, affecting Five Children in one family. By ANDREW DEWAR, Surgeon, Dunfermline.

THIS paper adds another authentic chapter to the many histories of strange anomalous convulsive diseases already on record; and strikingly illustrates, also, the important points of the propagation of such affections by *imitation*, and of their cure by the old Boerhaavian remedy, *terror*. The disease was most judiciously treated by the removal of the children from their home; and they were all cured by keeping them separate there; by threatening them with the cold affusion, searing irons, &c.; except the one originally affected, who required other means for her recovery. *Edin. Med. and Sur. Journal. July*, 1839.

On the Treatment of Strangulated Hernia, on the plan of Dr. O'Beirne.

Case i. By J. Sawkins, Esq. Surgeon, Towcester.
Case ii. By Fred. Ellis, Esq. of the Richmond Surgical Hospital, Dublin.

Both these cases proved successful under very unfavorable circumstances; and we entirely agree in the remark appended to the latter by Dr. O'Beirne, "that no medical man can, henceforth, be considered justified in proceeding to an operation for strangulated intestinal hernia, without having previously given a full and fair trial to the mode of treatment in question. If this principle were once acted upon, it is my conviction (says Dr. O'B.) that the operation would soon become comparatively rare, and many valuable lives be saved, which would otherwise be lost."

"Including this case (says Dr. O'B.) the practice has been employed, by myself and others, in twenty-one cases of strangulated intestinal hernia, all of which are now published. In fourteen of these the patients were quickly relieved from all suffering and danger, and, in the great majority of instances, after all other means have failed, and the operation had been decided upon. Of the seven cases in which the plan failed, the post-mortem examination in three demonstrated that they were not relievable by operation, or by any means short of divine; while, in two others, bands, adhesions, and other causes of failure were discovered during the performance of the operation; and in the two remaining there was the clearest evidence of the plan not having been fairly tried." *Lancet. July 6th and 27th,* 1839.

Case of Section of the Hamstring Tendons, for the Cure of contracted Knee-joint.

By Benj. Phillips, f.r.s., Surgeon to the Marylebone Infirmary.

This is an interesting case. The contraction which originated from rheumatism was very great, of some years' standing, and had resisted all the usual means. Mr. P. divided the tendons of the biceps, semitendinosus, and (by a second operation on the third day) semimembranosus. Its result was successful.

London Medical Gazette. July 20, 1839.

On the Treatment of Prolapsus Uteri, by means of the Actual Cautery and Caustics.

1. *On a New and Successful Method of Treating Prolapsus Uteri.* By Benjamin Phillips, f.r.s., Surgeon to the St. Marylebone Infirmary.
2. *Letter from* Dr. Evory Kennedy, Master of the Dublin Lying-in Hospital, *to* Sir Benjamin Brodie, Bart., *on the use of Caustics in Prolapsus of the Uterus.*

Both these communications deserve the attention of surgeons. The principle of the proposed treatment consists in producing a permanent contraction (not obliteration) of the vagina, by means of the cicatrices resulting from the partial destruction of the mucous membrane by lunar caustic, the fuming nitric acid (Phillips), or the actual cautery (Kennedy). This mode of treatment seems preferable to the plan by excision. For the details we refer to the original papers which will well repay perusal.

Gazette. June 29, 1839. *Lancet. June* 8, 1839.

MIDWIFERY.

Case of Restoration of Animation in a Stillborn Child. By H. Terry, Esq., Surgeon to the Northampton Infirmary.

This is a very interesting case, and adds to the credit already acquired by Mr. Terry for his long and successful prosecution of this important subject. We must find room for a brief extract. The infant was stillborn; and from fifteen to twenty minutes had elapsed ere Mr. Terry saw it. "The body of the child did not present the slighest appearance of life, and I thought the prospect of restoration was feeble in the extreme. It was right, however, to attempt it. Having set the funis at liberty, I hastily wiped the child's mouth, and at once began breathing into its lungs. I have various tubes for this purpose, but I never use any one of them: I find, or at least fancy, that nothing is so efficacious as my own mouth, and for this purpose I usually have with me a bit of leno or coarse muslin to place over the child's mouth to render the work a little less disagreeable. I continued breathing, with the child in this situation, for a minute or two, and then, observing the funis quite cold and collapsed, I hastily separated it, got the child into hot water, and gave myself a more convenient and agreeable situation for continuing the process. I am accustomed to keep the baby up to its chin in hot water, if the vessel will admit of it; if not, I cover the upper part of the body with flannels kept continually hot and and wet with the water. I support the head with my left hand, and compress the chest after each inflation of the lungs with my right hand, employing an attendant to hold the child's nose, if I find it necessary. In about ten minutes a very faint sob or slight attempt at inspiration took place, and shortly after this I found a feeble pulsation of the heart. The process was now persevered in with the pleasing assurance of success, and at the end of about twenty or five and twenty minutes there was sufficient respiration to admit of my service being discontinued." *Medical Gazette. July* 27, 1839.

Researches in Operative Midwifery. No. 2. *Version or Turning.* By FLEETWOOD CHURCHILL, M.D., Physician to the Western Lying-in Hospital and Dispensary, Dublin.

THIS, like the preceding productions of the same author, is a most elaborate production, giving a complete history, statistical and practical, of the important operation of "Turning." The following is the actual and relative frequency of the operation in the practice of the three principal European nations. "The records of English practice yield 36,569 cases, and 135 cases of version, or 1 in 267½. French practice 50,024 cases, and 514 cases of version, or 1 in 97¼. And German practice 21,415 cases, and 347 cases of version, or 1 in 61¼. The whole number of cases is 107, 978, and of version 996, or 1 in $111\frac{753}{996}$." We recommend this paper to the attention of all accoucheurs. *Dublin Journ. of Med. Science. July,* 1839.

MATERIA MEDICA, CHEMISTRY.

Economic Formula for Hydriodate of Potash. By WM. NICHOLS, M.R.C.S.

RUB together as much iodine and potass hydras (the potassa fusa of the former Pharmacopœia) as will render the mixture almost colourless, and add as much distilled water as will make, together, say, two fluid ounces.

The chemical equivalents of the iodine and potassa would of course be the proper proportions, provided they could be obtained perfectly pure, which, in commerce, I believe to be seldom the case. I therefore choose to get my solution prepared as above, of an amber colour, showing the iodine to be slightly in excess, and I afterwards add a few drops of the liq. potassæ, until the solution becomes perfectly colourless. By previously weighing the proportions of solid ingredients, the quantity of the salt in solution will be indicated; and as it is extremely soluble, it may be prepared so that each fluid drachm will contain a drachm of the hydriodate. *Lancet. Aug.* 25, 1839.

TOXICOLOGY.

Experiments with the Wourali Poison. With Remarks by Dr. CLANNY.

THIS poison is chiefly manufactured by the Accaaway Indians, who inhabit British Guiana, between the rapids of the Demerara river and the high mountains of the interior. The materials of which the poison is composed are unknown, and it is not unlikely but the secret will die with its possessors. So many reports had been circulated relative to the nature and properties of this poison, that in 1813, Mr. Waterton, a gentleman of independent property, who had visited Demerara purely through love of science, undertook, at his own expense, to travel 800 miles into the interior, for the purpose of ascertaining the precise facts. The privations and toil he underwent are detailed in his "Wanderings in South America," a work replete with interesting narratives and not a little humour. He obtained a considerable quantity of the poison, and on his return to England, experiments were tried on various animals: among others was a female ass, about three years old, upon whom the experiment we shall subsequently record was practised, and the creature recovered so as to be able to stand up in four hours. The creature was, at the request of the Duke of Northumberland, sent to Walton Hall (Mr. Waterton's residence), but it was some time before the constitution recovered the shock, though she ultimately became strong and healthy, and lived to the age of twenty-seven years, dying on the 15th of February last, through sheer decay of nature. Thus the point being fully proved that restoration to life could be effected after the animal functions had been entirely suspended by the deadly nature of the poison, it was conjectured that the wourali might be applied in certain cases of disease, so that first with the total suspension of animation, and next the succeeding restoration, a new existence might be imparted.

"Viewing the disease in horses as the result of irritation of the nervous system, Mr. Sewell conjectured that if a horse in tetanus were destroyed by poison, which acts by suppressing nervous power, and life were then to be restored by artificial respiration, the nervous system, on reanimation taking place, might possibly be free of the original morbid irritation. Reasoning thus, Mr. Sewell tried the following singular practice: A horse, suffering from a severe attack of the tetanus and locked jaw, the mouth being too firmly closed to admit the introduction of either food or medicine, was inoculated on the fleshy part of the shoulder with an arrow point coated with the wourali poison; in ten minutes apparent death was produced. Artificial respiration was immediately commenced and kept up about four hours, when reanimation took place, the animal rose up apparently perfectly recovered, and eagerly partook of hay and corn. He unluckily was too abundantly supplied with food during the night. The consequence was over distention of the stomach, of which the animal died the following day, without, however, having the slightest recurrence of tetanic symptoms."*

Other experiments have, we believe, been tried on animals at various times, with similar results, but no attempt that we know of has as yet been made on the human frame. In what way the poison acts has not been accurately discovered. A recent case of Hydrophobia at Nottingham induced a desire to ascertain whether it might not be successfully applied in hydrophobia. A request was sent to Mr. Waterton, at Walton Hall, to aid with his usual kind and generous feeling; that gentleman promptly attended, but unhappily poor Phelps expired before his arrival. Ever willing, however, to promote the advancement of science, Mr. Waterton engaged to exhibit the experiment upon animals, before the surgical and medical profession; and on Monday last, being the day appointed, the schoolroom, at the entrance to the Chapel Yard, in St. James's street, was selected for the occasion.

Exp. i. A brindled dog of good size was brought in for the purpose of testing the effects of the poison. An incision was made in the animal's left side, and an arrow-head, covered with the wourali, introduced; but, for a quarter of an hour, it manifested no symptoms of distress; at the end of that time, however, the action of the heart became irregular and tumultuous, and the pulse rose to 130, irregular and intermitting. At the expiration of 34 minutes, the dog showed indications of distress by twitchings and feebleness of the legs. At 36 minutes, the poison was operating powerfully; the convulsions were constant; the pupils of the eye greatly dilated; and the animal, unable to stand, fell down. The arrow-head was withdrawn. At 40 minutes, the struggling increased, and the pulse was very irregular, the respiration getting more difficult. At 42 minutes, the bladder was acted upon. At the end of 52 minutes, respiration had entirely ceased: there was a feeble action of the heart, the limbs supple. Five minutes more were allowed to elapse, and the body was opened. The heart was full of fluid blood, and, on being pricked with the point of a scalpel, gave not the slightest indication of irritability.

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* Mayo's Outlines of Physiology.

Exp. ii. A female ass, eight years old, was next tried. The animal was in rather poor condition, but, previous to operating, the action of the heart was regular, the pulse at 62.

At 9h. 6m. an incision was made in the neck of the ass, near the right shoulder, and an arrow-head, strongly poisoned, introduced into the wound. At 9h. 12m. the pulse was at 60, and continued uninterruptedly the same for five or six minutes longer, when it began to increase. At 9h. 20m. the pulse was irregular and had risen to 72, the respirations at 14 in a minute: at this time the pupils of the eye were becoming dilated. At 9h. 32m. the pulse was still at 72, the animal manifesting symptoms of uneasiness. At 9h. 36m. the pulse had risen to 84, and the creature repeatedly lifted its fore legs, which trembled, and the next minute it fell down, being exactly 31 minutes from the time of introducing the poison. There was a slight struggling, but the muscular power soon ceased. The pulsation was imperceptible, except a slight fluttering at the heart, and animation appeared to be extinct. The animal was then raised upon a table. An opening was immediately made in the trachea, and at 9h. 41m. artificial respiration was commenced.

The apparatus for producing respiration is a double pair of bellows, keeping up a constant current of air. To this a leathern pipe was made to fix over the nozzle; the other end of this pipe is attached to a tube that is introduced, by a bend, into the trachea, but easily removed in a moment, and a circular pad upon the tube, near the bend, is drawn pretty tight over the wound, so as to prevent an escape of air. Five operators are required: one to keep the bellows going, another to shift the part of the pipe on the tube, two to the body of the animal, and a fifth to the nostrils. The wind is forced by the bellows through the leather pipe and tin tube into the lungs; the moveable part of the leather pipe is then withdrawn from the tin tube, and the two individuals, appointed for the purpose, press upon the animal, so as to force the wind out again through the tube, the nostrils being stopped. The repetition of this process must be equal in time to the usual respirations of human beings in a state of health, or from sixteen to eighteen in a minute, so that some idea may be formed of the labour required.

At 9h. 44m. the pulsations became once more distinct, as high as 72, and continued varying, for seven hours, from 56 to sometimes 100, but most frequently at between 70 and 80. No other symptoms of life were at all displayed. Natural respiration was wholly suspended during the entire time, but about four o'clock there was evidently a slight muscular motion. Enemas of warm water and turpentine had been administered, and the bowels were freely acted upon. Dr. Davidson had also inserted on the right shoulder about 4 grains of strychnine: and turpentine had been used, as well as ammonia applied to the nostrils, but the room was so crowded by visitors that it was next to impossible accurately to take notes. About this time (four o'clock) the creature displayed symptoms, by the agitation of the nostrils, of returning natural respiration. At 4h. 38m. its natural respiration was free, and the artificial respiration was discontinued. Soon after the animal was laid on some straw before a large fire, where it continued in a very doubtful condition through the night. Every attention was paid to it. At 8h. (in the evening) it drank some water, and horns of gruel were occasionally given. It sometimes struggled a little with its legs, as if desirous of rising, but could not well raise its head. The pulse was feeble, and varying from 86 to 104. Towards the morning it breathed with great labour and difficulty, and apprehensions were entertained that it must sink; but between five and six o'clock the tube was removed from the trachea, and some mucus and blood was voided at the incision. Some lint was applied to the wound, and at seven o'clock it was sewed up.

At eight o'clock it was raised on its belly and seemed pleased with the change of position. A warm bran mash was offered in a bucket, of which it ate with much ease. Its breathing became more free, and about nine o'clock it was raised on its legs, but they were unable to sustain it, particularly the near foreleg, which was contracted and paralyzed. It was taken out into the yard for a couple of hours, but the cold was too intense, and it was again brought before the fire. At noon its pulsation was at 80, and it ate freely of mash and hay, and repeated horns of gruel were given to it. The creature continued improving through the day, but retarded the progress of recovery by ineffectual struggles to rise.

Exp. III. During the afternoon of Monday, a second dog was tried and expired in about half an hour. It was intended to try artificial respiration, and the trachea was opened, but the bellows were inadequate to the operation, and the attempt was abandoned.

Exp. IV. At eight o'clock (Tuesday), a male ass, about five years of age, was brought into the room; it appeared very vigorous, but with ventral hernia on the right side. It stood perfectly quiet, and the action of the heart was at 36. Mr. Attenburrow, the surgeon, cut into the trachea at 8h. 4m., and an incision was made on the fore part of the right shoulder, into which, at 8h. 6m. Mr. Waterton inserted an arrow-head with the wourali on it, the quantity being not quite one fourth of that used upon the ass yesterday. The creature struggled a little whilst the operation was going on, but as soon as it was over remained perfectly quiet.

At 8h. 16m. the pulse was at 66; and about four minutes afterward, the pupil of the eye become gradually dilated, though the animal stood perfectly patient, manifesting no symptoms of uneasiness.

At 8h. 30m. the pulse was at 62, and at that moment the ass operated on yesterday commenced with vigour to eat a mash; and some oats being offered to the one operating upon, it ate them very readily and continued to do so.

At 8h. 38m. spasmodic twitchings were evident in the abdomen, the animal still eating and apparently not much distressed. The next minute the legs trembled, and at 8h. 40m. the creature fell on its belly, but tried to rise again.

In two minutes more the twitching increased, with occasional struggles, but not very violent, and the animal lay down upon its left side, where it was suffered to remain, a bed of straw having been prepared for the purpose in the middle of the room. The tin tube was introduced into the trachea; the insertion caused the animal to kick a little, but it still continued chewing the oats.

At 8h. 44m. the action of the heart was regular, but the respiration convulsive and difficult, with repeated gaspings. It struggled occasionally, and at 8h. 49m. the pulse was 54, with spasm and difficult respiration, but the pupil of the eye not so much dilated.

At 8h. 54m. the arrow-head was withdrawn from the incision, and no poison remained upon it. The pulse continued the same, and the respirations were 44 in a minute; the vision pretty perfect.

At 8h. 57m. there was a gurgling in the throat and the respiration less frequent. Two minutes afterwards the animal struggled hard, kicked out with its hind legs, and at nine o'clock there was a tremor of the muscles, the respiration extremely difficult; the vision remained tolerably good.

At 9h. 4m. the action of the heart was strong but irregular, and the next minute the labour for breath was extremely difficult, the gaspings frequent, accompanied by spasm.

At 9h. 7m. respiration ceased, the vision entirely gone, the action of the heart was energetic, the extremities warm, throbbing of the carotid artery strong. The apparatus was put in motion, and artificial respiration commenced at an average of 18 a minute.

At 9h. 20m. the action of the heart was very feeble, and in other parts the pulsation was imperceptible. The artificial respiration was kept up on an average of 16 or 18; there was a spasmodic twitching of the eyebrows, and an occasional slight muscular motion of the tongue.

At 9h. 30m. the extremities began to get cold, but there was no rigidity in the limbs.

At 9h. 45m. the heart's action was quick but very feeble.

At 9h. 55m. the extremities were colder, but a warmth was still preserved in the region of the heart, and behind the ear, near the root.

At 10h. 1m. the animal made spontaneous efforts to breathe; the heart's action was energetic but not powerful, but no other pulsation perceptible.

At 10h. 10m. the lips were slightly agitated, the motion of the tongue was more active, and temperature of body improved.

At 10h. 25m. the nostrils gave indications of restored respiration, and the artificial respiration was kept up at 16 per minute. No pulsation perceptible except in the action of the heart.

At 10h. 27m. natural respiration recommenced.

At 10h. 36m. the ears were slightly agitated, and symptoms of returning vision.

At 10h. 40m. the respiration much hurried and irregular.

At 10h. 41m. the artificial respiration was discontinued.

At 10h. 44m. the natural respirations were 38.

At 10h. 50m. the natural respirations were 56, the breath warm, slight bleeding from the lower nostril, pulse very irregular, spasm on hind legs.

At 11 o'clock the natural respirations were 58.

At 11h. 5m. pulsations of the heart quick and laborious. The door of the room was closed with violence, and the noise produced a startling and quick motion of the whole body.

At 11h. 10m. the respirations 50, the ears cold.

At 11h. 17m. the pulsation at the heart was 60 and very feeble, barely perceptible in the arteries, and not possible to count.

At 11h. 22m. the pulsation of the heart was at 80, respiration improving, and muscular power increasing.

At 11h. 25m. some water was dropped upon his mouth, which he licked in with his tongue and swallowed, but the effort produced struggles.

At 11h. 30m. the pulse was at 52.

At 11h. 32m. the animal struck out with his legs rather violently.

At 11h. 25m. the respiration was 38.

At 11h. 40m. the respiration was 56; the mouth was moistened with water, producing strong muscular agitation.

At 11h. 55m. the pulse at 65 and feeble.

At 12 o'clock the respiration became more feeble, the animal heat was diminished, and the pulsation at 60; the eye not so susceptible of light.

At 12h. 5m. some gruel was poured upon the mouth, which produced violent muscular action; some hay was offered, which it smelt and attempted to eat.

At 12h. 13m. more hay was presented, which, in his attempts to eat, caused a spasmodic action of the respiratory organs.

At 12h. 25m. the pulse was at 65.

At 12h. 36m. the pulse was at 55, feeble; and the animal, attempting to cough, forced a quantity of mucus through the tin tube which remained in the trachea. It was evident that, by continued coughing, something was irritating the animal's throat, below where the incision in the trachea was made; and at 12h. 50m. a considerable quantity of mucus, tinged with blood, was forced through the tube; this continued at intervals for some time, the ass ejecting the mucus with difficulty.

At 11h. 40m. the pulsation was 60; and a piece of straw having got into the tube irritated the animal, who, in trying to get rid of it, struck the tube against the ground; the suddenness of the pain caused the creature to rise up upon his legs, and in a few minutes he walked about with a tolerable degree of strength. The tube was removed from the trachea, and some water being given it in a bucket it drank freely; the mucus continued to annoy it for some time longer, but at last it discharged the principal portion, breathed more readily, and manifested much greater ease.

At 2h. 35m. the pulse was 64, and the bowels and bladder acted upon pretty freely.

At 4 o'clock it began to eat some warm mash, and afterwards some hay; the wound in the trachea closed, so that no air was admitted, and the animal went on gradually gaining strength and becoming lively.

At five o'clock a person mounted his back, and he bore him without the least difficulty across the room. Since then the ass manifests every symptom of liveliness, strength, and even playfulness, betraying no signs of having been operated on.

Our reporter visited the patients yesterday (Thursday) afternoon, and found the one last operated upon trotting about the room as lively as possible. Indeed, the attendant informed him that, but for the coldness of the weather, he would have been turned out to grass that morning. The other (the medical gentlemen who examined them in the early part of the day said) was considerably better, though not yet able to stand for a long time at once, owing to the paralysis with which the near foreleg is affected; but the most sanguine hopes are entertained of its ultimate recovery.

The ass first operated upon died on Sunday morning last, about six o'clock. A *post-mortem* examination was consequently appointed to take place on Monday afternoon, at two o'clock. The operation was performed at the Medical School, by Mr. Sibson, of the General Hospital, in the presence of several of the faculty. On opening the animal, four or five ounces of reddish fluid were found in the pericardium. There were also four or five ounces in the cavity of the pleura. The lungs were in a firm state. The internal lining of the heart was very red—the surface quite pale—very little blood in the interior—some gas in the blood-vessels on its surface. There was no positive inflammation going on in the lungs; the lung most engorged was that on the side on which it had lain, and in that there was no more engorgement than what was apparently the effect of its lying on that side. There were no signs of vascularity or inflammation in the peritoneum. In fact, inflammation was not observed in any part, if we except a little in the immediate neighbourhood of the wound made in the trachea, for the introduction of the pipe, and that apparently only sufficient to have closed the wound, had the animal lived, and the parts where the wourali and the strychnine had been introduced, and those only superficial. The brain was found perfectly healthy, as well as the spinal marrow. The bowels were very full of food and wind, but exhibited no external inflammatory signs whatever. In short, there were no morbid appearances in the body to account for its death. The animal died of complete debility. It never overcame the shock it received from being in the unnatural state in which it was for seven hours. It was the opinion of several of the medical gentlemen, that it had never been so well since it was led out into the air the day after the operation was performed. The hair had come off in several parts of the body, which was caused by some turpentine having been spilt upon the table, and having got through the hair upon the skin. These were all the appearances noticed.

Nottingham Journal. April 12th and 19th, 1839.

Remarks by Dr. Clanny. Since the publication of the well-conducted and valuable experiments upon decapitated and living animals, elucidatory of the phenomena of respiration, performed by Sir Benjamin Brodie and Mr. Broughton, I have not been so much interested upon perusal of any others as those performed at Nottingham with the Wourali poison.

Since my discovery of the gases circulating in our arterial and venal blood, we have been enabled to explain the phenomena of the action of the Wourali poison in such cases. We find that, in the first experiment, artificial respiration was not employed, and that the deleterious effects of the poison were allowed to take their course upon the sensorium commune; hence the absorption of atmospherical air into the extreme branches of the pulmonary veins was prevented.

In the second experiment, the animal's life was saved by artificial respiration, and thus a certain portion of atmospherical air was absorbed into the systemic system.

The third corroborates the previous experiment, and requires no comment.

From the above-named discovery, made in the year 1834, we can also readily understand the manner in which the fœtus in utero receives a suitable quantity of oxygen from the parent. *Lancet. May* 18, 1839.

PART FOURTH.

Medical Intelligence.

PROVINCIAL MEDICAL ASSOCIATION.

The Seventh Anniversary Meeting of this Society was held at Liverpool, on Wednesday and Thursday, the 24th and 25th July; and exceeded every preceding one, both in the number of members present and the interest excited by the various proceedings that took place. We regret that we are compelled, by want of room, to give so imperfect a report of this important meeting.

In the forenoon of Wednesday the members of the Council and the various Committees met to transact business at the Medical Institution. At three o'clock the General Meeting took place in the Theatre of the same institution. Dr. Barlow, the late president, having resigned the chair to Dr. Jeffreys, the proper business of the Association began. The Secretary first read the Report of the Council, which was adopted, and ordered to be printed.

Dr. Barlow then read the Report of the Committee appointed to watch over the interests of the profession, which, as it is a document of importance, we shall give in full, together with the Petition to Parliament, unanimously adopted, after considerable discussion and some modification of the original terms.

At eight o'clock in the same evening the members again assembled, when Mr. James, of Exeter, read a report on the Progress of Surgery since 1836, which occupied two hours in the delivery. It was listened to throughout with profound attention, and elicited frequent marks of approbation. It was clearly and elegantly written, and was of an exceedingly interesting nature to surgical students. We shall have another opportunity of noticing this important document.

Mr. Turner, of Manchester, then brought forward a motion, in consequence of the recent law of the Royal College of Surgeons of London respecting hospital attendance in the large provincial towns, by which regulation it would seem that the advantages of these institutions as schools of practical surgery are not appreciated as they ought to be by the Council of the College. Mr. Turner maintained that it was not just in them to place a barrier to the success of provincial schools, by demanding a longer attendance upon provincial hospitals than upon those in the metropolis, as qualifying for their diploma; and concluded by moving that a memorial be presented to the College of Surgeons, embodying the views of the Association. After considerable discussion the motion was agreed to.

On Thursday the members breakfasted together in the great room in the Adelphi; and many of them afterwards visited the Botanical Garden, accompanied by Dr. Dickinson, the professor of Botany.

At twelve o'clock the members again assembled at the Medical Institution, when several interesting practical communications were made, and a still greater number postponed, for want of time.

Dr. Symonds, of Bristol, then read the retrospective address, on Medicine, which occupied upwards of two hours in the delivery. The learned author was much applauded during its progress, and also at the conclusion. It certainly is not inferior to any of its predecessors; and we regret that our limited space will not allow us to insert it, containing as it does a very admirable detail of all the events of importance to the profession which have occurred during the past year, and a well-digested sketch of all recent researches in medicine and the collateral sciences. The whole production was alike distinguished by sound philosophical principles, enlarged views of science, and a refined taste. When printed, it will be read with interest and advantage by the whole profession.

At the conclusion of the address, Dr. Gibson, Professor of Surgery in the University of Philadelphia (who was present) was, on the motion of Dr. Forbes, unanimously elected an honorary and corresponding member.

Dr. Conolly, of Cheltenham, then read the report of the Benevolent Fund Committee, which, although it was of a satisfactory nature, and pointed out many cases in which deserved relief had been afforded through its means, yet hardly gave such a record of subscriptions and expenditure as might be expected from so large an Association. The amount raised last year was only 50*l.*; 30*l.* of which were contributed by ladies. The report contained a well-grounded appeal to the humanity of the profession.

Dr. Cowan, of Reading, then read his report on Quackery, which we shall give at length, if our space permits.

Dr. Baron, of Cheltenham, then brought forward the report on Vaccination, which occupied three hours in delivery. It was such as was to be expected from the distinguished author of the Life of Jenner. As it was fraught with matter of the utmost importance, we trust no time will be lost in laying it before the public. We shall insert the petition to Parliament, with which it concluded, and which was unanimously adopted by the meeting.

After the conclusion of the business, the members and their friends, between three and four hundred, dined together at the Town hall; and the evening concluded with a *conversazione*, in the splendid drawing-room of the same mansion. On this occasion Mr. Ceeley, of Aylesbury, exhibited drawings of the interesting results of the experiments by which he has succeeded in inoculating the cow with small-pox.

It was settled that the next meeting of the Association should be held at Southampton; and Dr. Steed, of that town, was elected President for the ensuing year. Dr. Forbes was appointed to deliver the Anniversary Address,* and Mr. Dodd, of Chichester, to draw up the Surgical Report.

The following are the only documents laid before or emanating from this meeting, which we can find room for.

I. *Address of* Dr. Barlow, *on resigning the Chair.*

Gentlemen: In retiring from the office which, through your kind appointment, I have held for the past year, I shall delay the proceedings of the day for only a very few moments; for there is so much necessary business to engage your attention, that I should be inexcusable if I were needlessly to trespass on time required for other purposes, and which can be so much better employed. Each year brings some increase of the business to be done at our anniversary meetings, and in the prospectus of this year's proceedings you may perceive that more hours are allotted for conducting them than on any former occasion. In our earlier meetings the whole business was transacted with ease in a single day; now two days scarcely suffice. So fully, also, do the provisions made by our constitution, with the arrangements of the Council and Secretaries, embrace all that we can beneficially attend to, that any lengthened address from me, on the present occasion, would be as superfluous as it would be ill-timed. Nevertheless, gentlemen, I cannot take my leave of you without a few parting words. And first, I must beg to return you my sincere thanks for the high honour conferred on me in being placed, even for the brief period of a year, at the head of this numerous and most important Association. To be so distinguished is an honour of which the most ambitious might be proud. If "*laudari a laudato viro*" be deemed the highest praise that can be conferred,—to hold, even for a season, the rank of "*primus inter pares*" is a distinction still higher; the one emanating from an individual judgment, the other being awarded by the collective voice of a whole community. That I have been deemed worthy of holding this rank is the highest reward of professional labours that could have been bestowed on me; for of professional merit the profession alone can adequately judge. Their approval it is which gives real eminence; and to merit their favorable opinions should be the high aspiration of all whose ambition is pure and rightly directed. For the favour conferred on me in having been appointed your President, and for the personal kindness which I have invariably experienced in my intercourse with the members of the Association ever since it was first established, I beg leave to return to each and all my sincere and heartfelt thanks. I must detain you yet, gentlemen, for a single moment, while I offer my cordial congratulations on the rapidity with which our Association advances, and the elevation which it has attained. There must be some intrinsic worth in an institution which can unite so many intelligent and reflecting minds,—which can congregate annually such numbers as attend our anniversaries, withdrawing them from arduous duties which we all know are of a nature not to brook interruption of any kind,—which can inspire so much zeal and excite so much endeavour as our brief career throughout displays. Seven years ago we commenced with about fifty members: we now number in ranks nearly twelve hundred. It is impossible but that a body so numerous, so intelligent, and so united, must henceforward exert a powerful influence over all that relates to the science or practice of physic. It will, I trust, ever be our care to ensure that this influence be directed to praiseworthy ends; and that whatever measures we may find it necessary to pursue for attaining and establishing our own just rank in the political system, shall have the well-being of the community for their ultimate aim. In retiring from the chair, gentlemen, it is with peculiar satisfaction that I resign it to one so qualified to do honour to it as your respected President elect, Dr. Jeffreys. It is not in Liverpool that I need descant on his merits, his unceasing endeavours to promote the diffusion of knowledge being here, at least, well known; but they are not unobserved, even in places far distant; and the public voice, which rarely fails to award the meed of just praise to talents and energies devoted with sincerity and zeal to the public good, will assuredly enrol in the splendid list of public characters eminent for literature and science, of which Liverpool may proudly boast, the name of my valued friend and respected successor in this chair, Dr. Thomas Jeffreys. To him I now relinquish it, with my best wishes for his welfare and happiness, and for the increasing and enduring prosperity of the Association over which he is about to preside.

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* This will now be delivered by Dr. Scott, of Liverpool; Dr. Forbes having given it up.

II. *Address of* DR. JEFFREYS, *on taking the Chair.*

Gentlemen: Whatever may have been the motives which gave rise to the first idea of inducing men of kindred feelings and pursuits to promote and establish meetings for the purpose of imparting to each other views and opinions conducive to their happiness and best interests as a body, there can be but one opinion as to the effect of the Association whose Seventh Anniversary we are met to celebrate. Its beneficial results reflect much lustre on those under whose direction it was originally formed. Perhaps none exceed in zeal and energy the class of individuals I now address. Amidst personal privations from limited pecuniary resources, and difficulties arising from prejudices, often sufficient to weaken the exertions of the most ardent mind, we find the medical man ever ready cheerfully to promote every good work, encountering danger and obstacles of such magnitude, that the attempt to surmount them would, from dull understandings or hearts dead to the purest and most ennobling of human affections, incur only the reproach of rashness or infatuation. The Provincial Medical and Surgical Association has been one of the very first, by personal intercourse, by the exertions of its members, and by its publications annually issuing from the press, to grapple with these difficulties, and in a manner which has "grown with its growth and strengthened with its strength," so as to justify no cold and qualified encomium, but one far surpassing the measure of my humble abilities to bestow. This truth may be judged of by the alacrity with which the members have responded to the summons of this the seventh anniversary meeting; and there cannot be a better criterion of the interest which the resources of our annual reunion can call forth, than the numerous assemblage I see around me. It is difficult to say whether the gratification is most enjoyed by the original promoters of the Association, or by those who have more recently joined it; the latter, however, equally share the fruits of our labours. It is scarcely to be expected that the seeds of science and literature can be so abundantly disseminated in this emporium of commercial greatness, as in collegiate cities; but I am inclined to think that when the spark of ignition is once fairly kindled, it will burst forth into a brilliant and lasting flame. The example of six successive years, the manner in which the Association has been received, and the complete gratification visiting members have experienced, if I may be allowed to judge from my own feelings, ought to, and I am confident will, fill the hearts and minds of my medical brethren here with satisfaction; and we must be proud that Liverpool should be selected before other places conspicuous for the illumination of the understanding, for the spread of those truths which advance the progress of knowledge and promote the welfare of society. I well know that hints have been thrown out, and apprehensions entertained, that the Association coming among us would only discover the poverty of our resources and our inferiority, when compared with the advanced state of science and progress of improvement in other places; but I say, if this be really the case, which I hope I may be allowed to doubt, the step we are now taking is the way to render the reverse the truth. It is true we cannot grant classical honours, nor display a long list of names illustrious in the annals of letters, such as are to be found in our academical retreats; but we can evince an honest and well-directed zeal; and so long as we can remember a Roscoe, a Currie, a Brandreth, an Allanson, and a Park, now all resting in the silent tomb, it would be unpardonable in us not to feel proud of their talents and virtues, which, in spite of many disadvantages, enabled them to fill so large a space in the eye of their contemporaries; and the memory of which will, I trust, lead the rising energies of our youth to emulate their worth, and to improve science as they did. It is thus that Liverpool will take an honorable station among the places visited by the Association, and her sons claim an equal share in the friendship of others, where we may in future be minded to meet. The visit of the British Association to this town was a novelty to which it will ever be pleasing to revert, not only as being the means of introducing us to the acquaintance of many persons of the first scientific and literary eminence, but of permitting us to witness their admiration of the many interesting objects our northern metropolis presented to their notice. Should such be the result of this meeting also, how grateful will it be to myself personally, and to the gentlemen associated with me in the present arrangements! From the first dawn of my professional career it was my firm belief and conviction that the mainspring of all professional attainments rests solely with the individual himself, and that no position can be honorably and satisfactorily retained without feeling the conscientious and powerful stimulus of a deep and awful responsibility. On this solid foundation must the noble edifice of science be raised. To no station or pursuit in life does this maxim more strongly apply than to the medical profession; for our conduct should ever be such as to defy the most searching scrutiny, and court the most public investigation. An equally if not more difficult task to perform is it, to disperse those delusions of empiricism, by which so many, not only of the lower but of the higher classes of the community, are led astray. But whoever acknowledges the foregoing principle to influence his conduct, must further confess a principle, which, blended with mental cultivation, is so efficacious to the formation of the medical character, and is only to be derived from that Supreme Being, who hath alike formed the minutest creature, and regulates the mind of him who sounds all the depths and heights of science. Surely if He watches with parental care the lives of all his creatures, he will protect and guide the men who go forth among their fellows to execute the purposes of his benevolence, in dispensing the blessings of health and soothing the severities of pain. "Let him therefore who reads and observes, never forget that in all this, as in everything else, there is nothing casual, nothing purposeless, nothing undesigned, and that good ends have been intended, as good purposes have been effected; and that all creation presents to him who will examine it, the incontrovertible proofs of a Great Artist, intending, designing; perfect in wisdom, absolute in power." When I look back to the very able manner in which the deservedly distinguished men who have preceded me in this chair have displayed the worth and usefulness of our Association, I am relieved from all copiousness of illustration and cogency of enforcement on these subjects. My learned and zealous friend and predecessor, Dr. Barlow, has most justly said, that "the main objects for which we are associated, as stated in our fundamental constitution, are the advancement of medical science, and the maintenance of the honour and respectability of the profession." These objects have never been neglected; and each succeeding year has afforded additional proof of the soundness of the principles on which they were based. With harmony and unity of feeling and purpose, let the good of mankind stimulate all our endeavours. We shall thus have our reward in the approbation of all such fellow-labourers who are wise, and great, and good; and this after many of us shall have terminated our earthly career.

I should not stand acquitted to myself were I not to seize this public opportunity of expressing my approbation of the able and courteous manner in which my predecessor filled his office at our last meeting at Bath, calculated indeed to inspire no common feeling of respect and gratitude; and I am sure that you will one and all pardon me if I indulge in expressing the deep debt of gratitude we owe to our talented, zealous, and indefatigable Secretary, Dr. Hastings. Nothing I can say will add to his justly-established reputation, yet I cannot deny myself the gratification of giving utterance to the sentiments which I entertain in common with the members of the Association. The times in which we live are peculiar, and consequently influence every class of men—the profession to which we belong as well as others. It is not my business to descant upon the novelties and improvements of the day, that being the province of our learned and accomplished associate, Dr. Symonds, of Bristol, who will deliver the retrospective address; yet I cannot forbear quoting the remark of one of our most eminent honorary members (Sir Astley Cooper,) whose anxiety to be with us on the present occasion was expressed to me in very warm terms, and who lately observed to me, that "although science has advanced, men have retrograded. Where (said he) is our Newton, our Herschel, our Hunter, our Davy, our Priestley, our Black, or our Scheele? This (he adds) is the age of application, not of discovery." Let us then profit by that great and honest man's remark, on a subject so consonant to his favorite habits of study; and make use of the means which Providence has placed in our hands, by practically and more extensively applying the resources which a body of 1100 or 1200 individuals can command, and which has not inaptly been denominated "The Parliament of the Profession." Much has already been done, but much, even at this time, remains untouched; and this I am confident can only be effected by the concentrated talent, zeal, and vigorous industry, which I believe belongs to this enlightened and powerful body.

[After some further interesting observations on the progress of medicine, &c., including an affectionate biographical notice of the late Dr. Rutter, of Liverpool, Dr. Jeffreys concluded:]

I have now to bring this brief and I fear imperfect part of my duty to a conclusion, anxious to avoid prolixity and infringing upon the more important business of the meeting. The increased number of interesting subjects for discussion, the extent and demands of our connexions, both foreign and domestic, with the weight of matter for deliberation, will occupy so much time, that two days will prove scarcely sufficient. No one, I apprehend, can meditate dispassionately upon the objects of this Association without being alive to the power which it has of contributing to individual pleasure, professional advantage, and universal good: there may be "a faulty spoke in the wheel of every great machine," which will afford matter for comment to those who are prone to hypercriticism; but such minor points sink into insignificance, considered with the more extended views and prospects of beneficial results; nay, it is better that in our efforts we should disregard these minor imperfections, than that the mainspring of our intentions should be disturbed, and our views not fully and happily confirmed. I have now to request you will accept the acknowledgments of the local members for the honour you confer upon the town by your presence. No entreaty should ever have induced me to undertake the responsibility which awaits the individual who addresses you from this chair, had it not been from a thorough conviction of the advantages to be derived from the efforts of the Association, not only to the profession to which I have always been zealously devoted, but also to any community which shall possess the advantage of its presence.

I beg you will accept my expressions of gratitude for the attention with which you have favoured me upon this commencement of my official duties. I am fully sensible that neither my pretensions nor my claims have entitled me to this honour. Permit me to return my most heartfelt thanks to my medical brethren of the town, who are fellow-members, to the general Council, and to the members in ordinary, for the distinguished position in which they have placed me, rendering this the very proudest day of my professional life; and when the hour of separation arrives, when each of you must return to your professional duties at home, in the same proportion that I estimate the honour done to Liverpool and myself by this visit, will be the sincere regret with which I shall have to say—*valde vale!*

III. *Report of the Committee appointed to watch over the Interests of the Profession.*

At the anniversary meeting of the Association, held in Cheltenham, in 1837, a Committee was appointed "to watch over the interests of the profession at large," the duty specially assigned to its members being "to suggest to the Council from time to time such measures as may appear to them necessary to meet circumstances as they arise."

The terms of the appointment show that no direct obligation was imposed on this Committee to report its proceedings to the general meeting, its express duty being to offer suggestions to the Council. Nevertheless, as such appointment generally implies an obligation to report, your Committee deem it a proper respect on their part to submit to the Asociation the views which they take of the important trust confided to them. In the first place, being deputed "to watch over the interests of the profession at large," they considered that their attention was to be directed to the fundamental interests which involve the well-being of all ranks of the profession rather than to any of a subordinate nature, or to causes of complaint affecting particular departments only; and they adopted this view the more readily from the conviction which they entertain, that until the whole profession be organized on sound principles, and founded on a stable basis, no department of it can be well or effectively governed. While your Committee, however, have cordially concurred in the expediency of their own appointment, and have earnestly desired to fulfil to the best of their ability the trust reposed in them, the circumstances under which they entered on their duties have hitherto restrained all active discharge of them, the suitableness of any interference on their part being dependent on measures beyond their control, and the issue of which it was incumbent on them to await.

It must be in the recollection of all the members that the claims of the profession for an investigation of its manifold evils and imperfections were recognized by Parliament in 1834, and that on the 11th of March in that year, a Committee of the House of Commons was appointed "to enquire into and consider the laws, regulations, and usages regarding the education and practice of the various branches of the medical profession in the United Kingdom." This Committee commenced its labours on the 13th of March, and pursued its scrutiny minutely and diligently for several months. On the 13th of August a resolution of the House of Commons ordered "That the Committee have power to report the minutes of the evidence taken before them, together with their observations thereon." In compliance with this order, three successive volumes of the minutes of the evidence were laid before the House and printed, containing respectively the evidence relating to the London College of Physicians, the London College of Surgeons, and the Apothecaries' Company. Further evidences to at least an equal extent were obtained from the provincial medical faculty of England, from the Universities, and from the respective medical professions both of Scotland and Ireland. But these, though anxiously desired and expected by the profession, and by all the friends of rational reform, have never yet appeared. Why their publication has been so long delayed your Committee have been unable to ascertain. It was a probable cause of delay that the fire which consumed the Houses of Parliament in October, 1834, might have so damaged the records of the Committee of Medical Enquiry as to create a difficulty in continuing to publish uninterruptedly the further minutes. But this cause has never, so far as your Committee are aware, been pleaded; and they are, consequently, still ignorant why the remaining evidences have been withheld.

Your Committee, at the time of their appointment, confidently expected, not only that the remaining evidences of the Parliamentary Committee would speedily appear, but that, agreeably to the resolution of the House of Commons, this Committee would lay before the House a Report of their observations on the minutes; and your Committee were at the time further led to believe that on the basis of this Report, bills would be framed and introduced into Parliament for giving to the collective profession that political organization which both its own interest and those of the community at large imperatively demand. Awaiting this Report, and the announcement of the legislative measures which there was every reason to believe would be founded on it, your Committee felt that until the Parliamentary Committee should comply with the order of the House of Commons, by producing the whole of the evidences taken before them, and furnishing this Report, there was no likelihood of your Committee being able to promote the interest of the profession by any suggestions which they could offer to the Council, on which account they suffered the general meeting of last year to pass over without submitting to the Association any report of their views or intentions. Nearly five years having elapsed however since the Parliamentary Committee closed its investigations, your Committee have at length relinquished all expectation that either the unpublished minutes or a Report will be forthcoming, and they consequently deemed it their duty to institute enquiries as to whether any other course of proceeding than that which they had so long contemplated as the most appropriate and most feasible could be resorted to for accomplishing and expediting the reforms of the profession so greatly needed; and they have great satisfaction in announcing that a way has opened for effecting the desired ends far more favorable than they could have expected or hoped.

There are two modes by which, speculatively, an effective reform of the profession might be attempted; either by first organizing the profession so as to determine specially its classes and gradations, and then, in conformity with the system thus established, devising suitable qualifications for whatever departments the collective profession should comprise, leaving the special qualifications of each branch to be regulated by the government under which it should be more immediately placed; or by beginning with qualification, and by founding on it the gradations which should be recognized in a well-regulated profession. The first mode would have been that pursued, if the Parliamentary Committee had completed their enquiry so far as to furnish a Report, and had then introduced bills into Parliament for effecting the reforms which their Report should recommend. Attentive and mature consideration, however, has satisfied your Committee that the second mode is far preferable, attaining its ends with much more ease and far greater effect: and your Committee rejoice to add that the opportunity of promptly following out this second course is now happily furnished in a way which your Committee could not have anticipated, and through an instrumentality the creation of which they cannot but deem a most auspicious coincidence.

The foundation of the University of London must be well known to all the members. It being the province of this University to grant degrees in the faculties of law and of medicine, as well as in arts, one of its earliest measures was to determine the curricula of studies which should qualify candidates for obtaining the respective degrees; and in doing so, the Senate of the University proceeded in the most direct and judicious way, appointing a Committee of each faculty to devise the most suitable course of studies for each. The Committee of the faculty of medicine, after diligent and laborious consideration, sketched a Report which, in the first instance, they circulated widely among teachers and practitioners, in order to elicit the various opinions and criticisms which the document was calculated to call forth.

Having thus collected numerous opinions and comments, the Committee next circulated these, with a view to their being still more maturely considered. No more suitable mode could well be devised for bringing every topic involved under general and searching scrutiny. Having digested the several replies, the Committee next proceeded to modify their original sketch; but ere they submitted to the Senate their amended Report, this was once more laid before the several medical schools of England, Scotland, and Ireland. The Senate then proceeded to revise, with great care, the Report of the Medical Committees; and the "regulations" of the Senate, founded on this Report, are now in the possession of every medical school in the kingdom.

Your Committee must here solicit the attention of the members to some considerations connected with these regulations, which a cursory inspection of them might fail to suggest. To a casual observer, or on a transient examination, the "Regulations" of the University of London might appear, in the course of studies prescribed, to serve no other end than to create a new species of medical practitioners, and thus merely to introduce one more variety into a body in which discrepancy and inequality of qualification have long constituted a prominent evil. Were no other end contemplated, the course of studies thus enjoined, however perfect or superior to existing usages, would be utterly valueless, adding to the prevailing confusion, instead of superseding or diminishing it. But the views of the framers of the Medical Report were far from being so limited or misdirected. Impressed with the great and manifold evils which result from the present complicated and incongruous state of the profession, it appears to have been their design so to frame their Report that it should serve as a foundation for the legislative enactments necessary for establishing uniformity of qualification throughout the kingdom. In providing an improved qualification for the individual practitioner, they prepare the way for the still higher object of ensuring perfect competency in the entire body, and in every department; and, with a singular felicity, the scheme of reform typified in the "regulations" of the University, not only provides full security for every member introduced into the profession being competently qualified, but it consolidates the whole profession, restoring its natural unity, without disarranging those distinctions in practice which time and the natural tendencies of the profession have insensibly created. In this view the Committee of the Medical Faculty, deputed by the Senate of the University of London, have been acting, not merely as projectors of a curriculum of studies, but as profound and effective reformists of the whole profession. According to their plan, the Bachelors of Medicine would be the general practitioners of the kingdom, qualified by their education to practise every branch of the art. The Bachelors, who should look only to obtaining a legal qualification to practise with public attestation of their competency, would be content so to remain; while the more ambitious would advance to the degree of Doctor. The signal excellencies of this scheme become manifested in proportion as it is scrutinized and followed out into its practical operations. In the first place there would be the great advantage of every member entering the profession being fully attested, both as to his general and professional acquirements; and thus the best security would be afforded for the profession continuing to maintain the eminence, literary, scientific, and moral, so long accorded to it. Secondly, to every member the way would be open for reaching the highest distinction, according as talents or inclination might prompt. The more advanced distinction being given not in lieu of the primary, but in addition to it, each class would understand its own position, and no unseemly rivalry could be engendered. If a feeling of honest pride or other motive should at any time incline a Bachelor to become a Doctor, it would be always in his power, by a slight effort, so to do. Thus the system would realize what has ever proved the most powerful incentive to human endeavour, a career opened to talent, and every individual in the profession would have free scope to elevate himself to any height which his talents and industry could enable him to attain. A further advantage of such a system is that all members so entering the profession, being on their introduction fully and equally proved as to their competency, all pretext for subjecting them to further ordeals would be removed, and individuals would be free to practise wherever their own interests or inclinations might determine. Such improvements in the profession would realize all that the most sanguine could desire.

IV. *Report of the Committee appointed to consider the Nature, Extent, and Evils of Quackery.*

The great extent and importance of the subject, the difficulty of acquiring extensive and correct statistical information in regard to it, and the present unsettled state of the internal arrangements of the medical profession itself, combined with the natural and almost necessary differences of opinion as to the precise measures which it might be desirable to propose—these and other reasons, unnecessary to allude to, have prevented your Committee from coming forward and presenting you with a formal Report.

They are induced, however, to hope that you will not consider that they have wholly neglected the duties intrusted to their care, and that in the statement which accompanied the account of the proceedings of the last Anniversary, (put forth, it is true, upon individual responsibility, but with the sanction and assistance of the members of the committee generally,) you will admit that some additional information has been obtained.

The *machinery of Quackery* was there partially exposed; and the reasons why it should be legally suppressed, with the popular and not unfrequent professional objections to all legislative interference, were freely canvassed, and, we trust, satisfactorily replied to.

The *interests of Government* in empiricism were also, for the first time, accurately investigated; and it was shown, from unquestionable authority, that the revenue from quackery is less than 50,000*l.* a year. This fact is not without importance, since one of the most tenable and practical objections urged by the opponents to all active measures, was the great amount of the government profits. The true statement of the case is, however, now before you; and it is evident, whatever other obstacles may exist, that the one now alluded to can no longer be regarded as either insuperable or formidable.

The actual state of existing anti-empirical enactments has also been examined, and, while it cannot but be admitted that much of the present respectability and standing of the medical profession is to be attributed to the legal penalties and restrictions which have from time to time been enforced against ignorant and unqualified practitioners, (the number of whom, great as it still is, has in consequence been materially diminished,) it is at the same time equally clear that no efforts have yet been made to prevent or even to curtail the irresponsible and indiscriminate circulation of *medicines;* but that, by the stamp and patent regulations, this glaring abuse is legalized and encouraged; and, from the unequalled facilities which now exist for advertising, an injury upon the public health is inflicted, far greater than could ever be the case from the strictly personal, though unqualified practice of physic.

Procuring the abolition of the stamped and patent medicines, at least in their present irrestricted form, should therefore be the great and leading indication of our efforts at the present moment; and your Reporter is convinced that the greatest if not the only serious obstacles to its accomplishment are the inertness and apathy of the medical profession.

That medical men are alone competent to expose the evils of empiricism, and that, as legalized guardians of the public health, they are, *ex officio,* as well as morally, bound to do their utmost for its suppression, are axioms so palpably correct, that they need no arguments for their support; and, while we should be justified in coming forward and demanding redress, upon the grounds that our privileges and constitution are grossly infringed, we are still more strongly called upon to exert ourselves for the benefit and protection of those, the conservation of whose health is the very object of our corporate existence.

It is of little importance to determine whether, as a profession, we should gain or lose, in a pecuniary sense, by the suppression of quackery, since the public are naturally and justly but little interested in the amount of our private emoluments; but knowing, as we do, the injurious effects of empiricism upon the national health, it *is* important and unquestionably our duty that we should, irrespective of all personal interests, stand boldly forward and denounce it, while we exert our influence, both in public and private, to check and resist its encroachments.

Our best claims upon public confidence and respect should rest, not upon a timid and cringing servility to popular prejudices, or upon a morbid, selfish sensibility, which dreads collision with existing interests, hostile as we believe these to be to the general welfare, but upon a firm and disinterested insisting upon what we know to be essential to the public good—upon a temperate but decided assertion of our medical authority, and upon a uniform refusal to sacrifice our professional character to public ignorance and caprice.

The anomalous condition in which we are now placed, the indignities to which, as a profession, we have been exposed by the public authorities, the cheap estimation in which we are held, and the open and successful competition which impudence and avarice have so long been able to maintain against us, are mainly to be attributed to the want of a more honorable and independent spirit among ourselves, to the not attaching sufficient weight and importance to the office we hold, to the frequent absence of unity and sincere cooperation in our ranks, and to the too eager pursuit by many of individual benefit, at the expense of the body to which they belong. Until we are true to ourselves, we may in vain expect the full harvest of public confidence and esteem; and this precept remains still unfulfilled, so long as we silently submit to the gross encroachments of modern quackery, and while we have no means of expelling from our ranks all those who sacrifice the best interests of their profession at the shrine of a selfish and ignominious cupidity.

Medical reform can only be securely effected by the exertions of medical men; and it is idle to expect assistance from without, unless we show a willingness to purify and a power to defend ourselves. Instead of vaguely descanting upon our grievances, let us actively unite for their removal, and rest assured there is no reasonable limit to the good which might then be accomplished. The present Association furnishes us with an engine of no ordinary magnitude and influence; and, while we hope that the great and leading questions which more directly affect the public welfare and our corporate purification, will ever engage its primary and earnest attention, we trust that it will also constitute a permanent *court of honour*, excluding from amongst us all whose conduct is at variance with the higher moral interests of a profession, whose dignity and usefulness we are pledged by every honorable feeling to uphold.

The question of medical quackery is at this moment engaging the serious attention of the French government, and very stringent regulations may be shortly expected. The British Medical Association is also occupied in collecting information upon the subject, in conjunction with a plan of general medical reform. Let us not, then, withhold our cooperation, but let each member feel himself personally interested in the struggle, and pledged to furnish his mite of information and suggestions to those who may be more particularly intrusted with the labour of revision and arrangement.

It is only by a careful digest of a large aggregate of facts and opinions that satisfactory results can be obtained, and there is no subject more deserving of the combined talent and energies of the Provincial Medical and Surgical Association, and in the right settling of which so many important interests are involved, than the one to which we are now directing your attention.

For the reasons already alluded to at the commencement of this report, your committee have refrained from submitting, for your approval, any definite measures for the suppression of empiricism, being anxious, if possible, first to secure the united cooperation of the members, before differences of opinion are excited by the discussion of secondary details. They may, however, be permitted to remark, that quackery in its actual and grosser manifestations is capable of being as effectively interfered with as abuses of many other descriptions; and, though the hope of its *utter* extinction may justly be regarded as chimerical, no reasonable doubt can be entertained by those who have considered the facts of the case, that its present daring aspect and alarming prevalence, may be most materially modified and controlled by well-directed legislative prohibitions.

To accomplish so desirable a result, it is only necessary for the medical profession of Great Britain zealously and cordially to unite, and, by doing so they could not fail to secure the attention and support of Government, as well as to enlist in their favour the common sense and correct feeling of every conscientious and enlightened individual.

We would also beg leave to observe, that reiterated experience too plainly proves that all other means of a less direct or more prospective character must ever prove comparatively inert and unavailing; but if aided by efficient legislative protection, they would necessarily tend to disabuse the public mind of those prejudices and misconceptions on which the quack so successfully trades.

Among the more important of these accessory measures may be enumerated popular lectures on empiricism, by which some are convinced and others shamed out of their delusions; also the circulation of tracts, unfolding the iniquities of the system, and demonstrating its irrationality; and lastly, that more limited but perhaps more effectual exposure of its dangers and inconsistencies, which our daily intercourse with its victims permits, but which we are too apt to neglect, either from morbid sensitiveness, moral cowardice, or the shortsighted promptings of self-interest.

In conclusion, let us resolve no longer to tolerate quacks among ourselves; but by a uniform and unflinching attachment to truth—by unwearied efforts for the acquisition of knowledge and its enlightened application for the removal and alleviation of human suffering—by a constant willingness to acknowledge and eradicate abuses, wherever they may exist,—let our conduct and bearing, individually and collectively, be such as at all times to entitle us to the sympathies and support of every upright and intelligent mind.

V. *Petition to Parliament for Reform of the Profession.*

That this petition emanates from a body of medical practitioners, comprising nearly twelve hundred members, and entitled, the Provincial Medical and Surgical Association.

That the members, being derived from every county in England, and the Association open to all regular medical practitioners who desire admission, it may be considered as representing the provincial medical faculty of this portion of the United Kingdom.

That your petitioners naturally feel a deep interest in the welfare and respectability of the profession to which they belong, and have long deplored the manifold evils to which it has been subjected by the want of a suitable political organization, such as should both ensure the professional competency of all who enter it, and give full legal protection to all who bring to it the ordained qualifications.

That your petitioners hailed with peculiar satisfaction and gratitude the disposition manifested by Parliament, in 1834, to take this important subject into consideration, and rejoiced in the appointment by the House of Commons of a Committee to enquire into the general state of the profession, assured that due enquiry alone was needed to demonstrate the necessity of legislative intervention.

That your petitioners marked, with high approval, the diligent scrutiny so ably conducted by this committee, and have anxiously awaited the publication of the minutes of the evidence taken before it. But five years having elapsed since the committee completed its enquiries, without the whole minutes of evidence being yet published, or any report of the committee presented, your petitioners feel that they should fail in the duty which they owe to their profession if they longer delayed appealing to the legislature in its behalf.

It was contemplated by your petitioners to solicit the House of Commons for enforcement of their resolution of the 13th of August, 1834, which called on the Committee of Medical Enquiry for the Minutes and Report. But circumstances have occurred which render your petitioners less anxious respecting the proceedings of this committee, it appearing to your petitioners that the objects sought by that enquiry may be attained through other more direct and effectual means.

Your petitioners beg leave to represent that the main requisite and only stable foundation for any sound system of medical polity is to establish an adequate and uniform education for the whole profession, so that all who enter it shall pass through the same course of preliminary and medical instruction, be tested by the same examinations, and, when approved, entitled to the same privileges. The natural unity of the profession imperatively demands this consolidation, there being no more preposterous or mischievous anomaly than that presented by the existing state of the medical institutions of this kingdom, where practitioners of physic issue from no less than sixteen separate sources, differing from each other in the course of education enjoined, the qualifications required, the examinations by which the qualifications are tested, and the privileges conferred.

Finally, your petitioners, confident that the time is arrived when the intervention of Parliament is imperatively called for to give to the medical profession a sound legal constitution, and deeply impressed with the conviction that an adequate and uniform education, with community of rights and privileges, constitutes the only sure basis on which to found such constitution, humbly beg that the necessary legislative enactments may be passed for establishing in each of the three divisions of the kingdom one superintending body, founded on the same principles and governed by similar regulations, through whose examination and by whose licence alone, shall admission to the profession be in future attained.

VI. *Petition to Parliament respecting Vaccination.*

That this Association consists of nearly twelve hundred members, including a great proportion of the physicians and surgeons of eminence, practising in the various cities and towns of England, most of whom, during the whole of their professional lives, have bestowed much of their attention in studying the qualities of human smallpox and cow-smallpox.

That at their last anniversary meeting, held in July, 1838, at Bath, a section was appointed to enquire specifically into the present state of vaccination.

That the section so appointed hath recently presented its Report to the meeting held at Liverpool, containing information of an highly important character, and well deserving the attention of your Honorable House.

That your petitioners have peculiar satisfaction in stating that the result of their enquiries has thrown much light on the nature of *variolæ vaccinæ*, and that Dr. Jenner's opinion, that it was an affection of a true variolous character, has been demonstrated by historical evidence, as well as by direct experiment, human smallpox having been recently communicated to the cow by inoculation, and the result having been the production of a *variolo-vaccine* lymph, possessing all the properties of the original vaccine disease.

That this direct confirmation of a great doctrine adds infinite value to the original discovery, by explaining alike the nature and the degree of protection that may be derived from perfect vaccination, which is, in short, to use the language of the discoverer himself, to impregnate the constitution of man with smallpox in its mildest, instead of its pestilential and fatal form.

That the diffusion of this truth may be made subservient to the best purposes, and, with the aid and countenance of your Honorable House, be rendered highly instrumental to the preservation of human life.

That your petitioners have learned, by the concurrent testimony of a very large portion of their members, that cow-smallpox, if duly and carefully communicated, has an enduring influence in protecting the constitution; that while they admit that this protection is not in all cases complete, they have unquestionable proof of its being capable, if generally and properly employed, of mitigating, controlling, and, they might almost say, of extinguishing smallpox in any district.

That they have further learned that, while vaccination has been imperfectly and insufficiently employed, in many places, smallpox has been and continues to be diffused in a manner highly detrimental to the health and safety of the community.

That, before suggesting any measure for the more efficient diffusion of vaccination, they would specially implore your Honorable House to take measures for regulating the practice of smallpox inoculation; and they are induced to urge this prayer of the petition with greater earnestness, because they have ascertained that such practice has been abandoned by almost every respectable medical man in the kingdom, from a disinterested conviction that it is uncalled for and dangerous, and ought to be universally superseded by vaccine inoculation.

That the abandonment of smallpox inoculation by medical gentlemen has led ignorant and illiterate persons to take up the practice, whereby the disease has been widely disseminated throughout the country, and that many deaths have been the consequence.

That your petitioners humbly beg leave to represent to your Honorable House the extreme necessity of restraining such proceedings; they do not ask for a positive enactment to forbid smallpox inoculation entirely, though they think there are sufficient grounds for so doing; but they pray that it may be enacted that a practice, always dangerous and very frequently fatal to the subject of it, as well as to the community at large, shall not be permitted to be followed by any one who has not been duly qualified to exercise the profession of medicine or surgery; that such restriction, in their opinion, would be wise and salutary at all times, but is more especially demanded now that a mild and efficient substitute may be brought within the reach of every member of the community. To accomplish this latter object your petitioners humbly submit that certain regulations ought to be adopted. They have ascertained that during recent epidemics, a very large proportion of the lower classes, both in towns and among the rural population, have been found totally unprotected; that in one city in the south of England, five hundred persons perished from this cause in the course of last year; and that in another place, on the breaking out of smallpox, the whole of the children under nine or ten years of age were exposed to its ravages, which were only checked by prompt and extensive vaccination.

That these and many similar facts have convinced them that at this time there is no sufficient provision for the vaccination of the poor in this kingdom; that the practice, as offered to the poor at our public institutions in towns, as well as by private individuals, is by no means adequate to the wants of our greatly increasing population.

That it appears to your petitioners to be the duty of the State to remedy this great evil, by appointing regularly-educated vaccinators, with suitable salaries, in districts sufficiently numerous to embrace the whole of the poor population of the country, and who shall offer gratuitous vaccination at stated periods to all within their bounds, keeping accurate registers of their proceedings, and communicating regularly with the national vaccine establishment.

That under such a system, duly organized and vigilantly executed, your petitioners have the strongest reason for believing that smallpox would be effectually restrained, and soon become almost unknown.

VII. *Important Experiments in Vaccination.*

Performed by Robert Ceely, Esq. of Aylesbury.

Mr. Ceely exhibited several original and beautifully executed coloured drawings, illustrative of the following subjects:

1. Two successful inoculations, in different stages, of young stirks, with small-pox matter.
2. Numerous vesicles, in different stages, in several human subjects, chiefly children, produced by the lymph obtained from these inoculations.

These drawings were intended to illustrate the experiment alluded to in the admirable report of the vaccination section, read at the meeting on the same day by Dr. Baron; and in which it was declared that there could now be no doubt as to the correctness of the doctrine first announced, and always maintained by Dr. Jenner, that human and vaccine variolæ are identical, the latter being merely a mild modification of the former.

After minutely describing these interesting and well-executed drawings, Mr. Ceely remarked that he had several times inoculated cows, in various ways, with smallpox virus, in different stages, either without success, or with the production of *tubercles* or pimples, without lymph—that these two successful inoculations were performed in the beginning and middle of February last, at a time when small-pox was prevailing in Aylesbury—and when he had invested three other cows with blankets from the bed of a smallpox patient, and employed a man, recently convalescent from smallpox, frequently to walk among and handle all the animals under experiment.

These experiments, he stated, were witnessed by five medical men, a veterinary surgeon and his assistant, all of Aylesbury, who were perfectly satisfied with the manner in which they were conducted, and highly gratified with the unexpected results

The two animals, on which the above experiments succeeded, were not more than twelve months old. The parts selected for inoculation were the sides of the labia pudendi, and adjacent vicinity.

In the first animal, fourteen points (the teeth of a new comb), well charged with fresh virus of 7th or 8th day of variola discreta, were inserted into seven punctures in the left side of the labium pudendi, from which only one vesicle was produced; the other punctures proving tubercular or papular. Until the *tenth* day, there was some doubt as to the probable character of that which ultimately became perfect: but on *this* day, clear pellucid lymph was taken on points, and immediately inserted into the arms of several children. The animal, on the *ninth* day after variolation, and *before* a perfectly vesicular appearance was observed, was *vaccinated* with recent dry lymph from a child in eleven punctures on the right side of the labium pudendi. On the *fifth* day after vaccination, (and thirteen of variolation,) when *every vaccine* puncture had succeeded, lymph was taken from two or three of the vaccine vesicles, and also inserted into the arms of some children.

The *variolous* vesicle, and the *vaccine vesicles* ran their course together; the former attaining its acmé on the fifteenth day; the latter, theirs, of course, on the seventh day, and were smaller. The lymph from the variolous vesicle, and that from the vaccine vesicles, produced in the human subject the true vaccine; the former, for a short time, appearing less active than the latter; but finally, losing all traces of disparity.

In the second experiment, recent liquid smallpox virus, (seventh or eighth day variola discreta,) in *capillary tubes* was used. The punctures were first deluged, each with the contents of one or two tubes; and new points of the same materials as used in the other experiment, well charged from the same patient, on the same day, were cut off short, and left in the wounds till the next day. Eight punctures were made—four above, and four upon the back of the left labium pudendi. All had risen on the fifth day; but only the four upon the *back* of the labium advanced, and produced pellucid lymph; the others near the ischium and perineum, for a time tubercular or papular, gradually subsided. Lymph was taken from one of the four vesicles on the sixth, eighth, ninth, and tenth days; the disease declining on the eleventh day. This lymph, too, was inserted into the arms of children, and produced, especially that of the eighth day, vaccine vesicles of a remarkably fine character. Many hundred children and some adults, at Aylesbury, Cheltenham, and the Smallpox and Vaccination Hospitals, London, have been vaccinated from this source. The efficiency of the lymph has been fully tested by infection and inoculation, both of which it has resisted. Both *stirks* have resisted careful and repeated *reinoculation* and *revaccination*.

3. Experiments in *retro-vaccination*, or vaccination of the cow from the human subject, and of the effects of the resulting lymph again transferred to the human subject.

Mr. Ceely remarked, that although the most successful vaccinations of the cow are effected by the reiterated manipulations of the milkers—yet the young stirk can be vaccinated with comparative facility with lymph recently obtained from the milch cow. It is, however, not so easy to *retro-vaccinate* from the human subject, as he had learned from many experiments. When successful, in general, the disease, thus induced in the young animal, arrives at its acmé between the tenth and eleventh days—the normal course of the natural disease in the milch cow. The *renewed* lymph thus produced seems to have undergone a change—for if taken from the seventh to the tenth day, and returned to the human subject, it rarely produces a vesicle at the acmé till the eleventh, twelfth, or thirteenth days —and then often smaller than ordinary, but in about two or three removes from the cow it recovers its previous activity. As in man, so in the cow, revaccination is unsuccessful at an early period.

4. The artificial vaccine vesicle in dogs, and an occasional attendant vesicular eruption. The dog, it is well known, is very susceptible of vaccination; the vesicle appears to run a similar course as that in the cow and in man; but is manifestly smaller, yielding much less lymph. This animal, too, is liable to a consequent vesicular pemphigoid eruption, resembling, in miniature, what is occasionally noticed in infants about the tenth or fourteenth days of vaccination.

5. The natural (and genuine) variolæ vaccinæ on the teats and udder of a white milch cow, exhibiting vesicles of different sizes in different stages,—entire, incrusted, or broken.

6. The *White Pock.*—This, though in general a mild and unimportant disease, has, to an inexperienced observed, many features in common with the true cow-pock, especially towards its close and period of incrustation; but Mr. Ceely showed, that by a little attention, particularly in the early stage, they might be readily distinguished. It was only in the stage of incrustation that an error might be committed. He remarked that it might be said that the *white pock* and the true *variolæ vaccinæ* bore the same relation to each other in elementary character, progress, maturation, and decline, as varicella does to human variolæ.

7. The *Yellow Pock.*—A pustular disease, resembling, at first, ecthyma vulgare, and ultimately assuming an impetiginoid character.

8. The *Blue Pock.*—A vesicular disease, often putting on the appearance of impetigo.

These varieties of the spurious disease depicted on the teats and udders of the milch cow, frequently occur, at all seasons, from common causes, especially in heifers on their first milking, are often intercurrent, and were taken from subjects inspected in the same dairies where the true cowpock coexisted. The latter would appear to be not an unfrequent visitor of the numerous dairies in the rich vale of Aylesbury, during the autumn, winter, and spring; and Mr. Ceely seems to have made diligent use of the opportunities thus afforded him of observing this and other eruptive diseases of cows. It is evident he has been at great pains, and considerable expense in collecting and illustrating the many facts we have here merely glanced at, and we are glad to hear that it will not be long before they are made public in such minute detail as their novelty and interest demand.*

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The Association held its meeting this year at Birmingham, in the week beginning August 26. The proceedings in the medical section were, as usual, both important and interesting, and we regret that our limits will only allow us to give a mere catalogue of the papers redd.

Section E. Medical Science.

President—Dr. Yelloly. *Vice Presidents*—Dr. Johnstone, Dr. Roget, Dr. Macartney. *Secretaries*—Dr. G. O. Rees, Mr. F. Ryland. *Committee*—Drs. Blakiston, Booth, G. Bird; Mr. W. S. Cox; Drs. Evans, Foville, B. Fletcher, Hastings, T. Hodgkin; Mr. J. Hodgson; Drs. J. Johnstone, Pritchard; Mr. J. Russell; Drs. R. S. Sargent, Vose; Mr. R. Wood; Dr. Wale.

List of Papers read.

1. Abstracts of a remarkable case of Rupture of the Duodenum, and of some other interesting cases. By Sir David Dickson, M.D.
2. A notice of the methods that have been used for the removal of Capsular Cataract, where the opaque capsule remains after absorption of the Lens. By Mr. Middlemore.
3. Case in which the operation for Artificial Pupil was performed with success. By Mr. Middlemore.
4. On the means of repressing Hemorrhage from Arteries. By Professor Macartney.
5. On the Sounds produced in Respiration and on the Voice. By Dr. Blakiston.
6. Observations on Poisoning by the Vapours of Burning Charcoal. By Dr. Golding Bird.
7. On the Rules for finding with exactness the position of the principal arteries and nerves, from their relations to the external forms of the body. By Prof. Macartney.
8. On the Cause of the increase of Small-pox, and of the origin of Variola Vaccina. By Dr. Inglis.
9. On the New Vaccine Virus of 1838. By Mr. Estlin.
10. On Alkaline Indigestion. By R. D. Thomson, M.D.
11. On the Red Appearance of the Internal Coat of Arteries. By Mr. Hodgson.
12. On the Respiration of Deteriorated Atmospheres. By Mr. C. T. Coathupe.
13. Report of Ten Cases of Calculus treated by Lithotrity. By Mr. Costello.
14. On the Microscopic Structure of the Teeth. By Mr. Nasmyth.
15. On the Structure of the Epithelium. By Mr. Nasmyth.

The following grants were made to the section:

Experiments on sounds of the heart	£25
Physiology of the lungs and bronchiæ	25
Construction of medico-acoustic instruments . . .	25
Enquiry into the connexion between the veins and absorbents .	25
Experiments on acid poisons	25
Total grants to Section E......	£ 125

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* Dublin Medical Press. Aug. 1839.

BOOKS RECEIVED FOR REVIEW.

ENGLISH.

1. Tables, showing the Temperature of Cove, for the year 1838, and contrasting it with Torquay, &c. By D. H. Scott, M.D., &c. Folio, pp. 4.

2. Selections in Pathology and Surgery; or, an Exposition of the Nature and Treatment of Local Disease; exhibiting new pathological views, and pointing out an important practical improvement. Illustrated by Cases. By John Davies, Surgeon to the Hertford Infirmary.—London, 1839. 8vo, pp. 128. 6s.

3. Practical Remarks on the Use of Iodine, locally applied in various Surgical Diseases and External Injuries. Illustrated by cases. By John Davies, Surgeon to the Hertford Infirmary.—London, 1839. 8vo, pp. 62. 3s.

4. A Treatise on the Diseases of the Heart and Great Vessels, and on the Affections which may be mistaken for them, &c. By J. Hope, M.D. F.R.S. 3d Edition, corrected and greatly enlarged. —London, 1839. 8vo, pp. 639. 18s.

5. The Medical Portrait Gallery. Parts XVII, XVIII, XIX, containing Memoirs and Portraits of Jas. Ware, F.R.S., Sir Chas. Bell, F.R.S., Hippocrates, Dr. Bostock, Dr. Roget, and Mr. Stafford.—Lond. 1839. 3s. each No.

6. The Student's Guide to the Hospitals and Medical Institutions of Paris. To which is added, an outline of the Edinburgh and German Universities. By John Wiblin, M.R.C.S.— London, 1839. 12mo, pp. 90. 3s.

7. Human Physiology, for the use of Elementary Schools. By Charles A. Lee, M.D., late Professor of Mat. Med. in the University of New York. Second Edit.— New York, 1839. 8vo, pp. 336.

8. The Collected Works of Sir Humphry Davy, Bart., L.L.D., F.R.S., &c. Edited by his brother, John Davy, M.D. F.R.S. Vol. I. Memoirs of his Life.—London, 1839. 8vo, pp. 475. 10s. 6d.

9. On the Enlisting, Discharging, and Pensioning of Soldiers; with Official Documents on those branches of Duty. By H. Marshall, F.R.S.E., Deputy Inspector-General of Hospitals. Second Edition.— Edinburgh, 1839, 8vo, pp. 259. 7s. 6d.

10. The Surgical Anatomy of the Groin, the Femoral and Popliteal Regions. By Thomas Morton.—London, 1839. 8vo, pp. 207. With Plates. 9s. plain, 13s. col.

11. The Cyclopædia of Anatomy. Ed. by R. B. Todd, M.D., F.R.S. Part XVII. (Hibernation Insect.) July, 1839.

12. First Annual Report of the Registrar-General of Births, Deaths, and Marriages in England.—London, 1839. 8vo, pp. 168.

13. The Surgeon's Vade Mecum; a Handbook of the Principles and Practice of Surgery. Illustrated with numerous wood engravings. By Robt. Druitt, M.R.C.S. —Lond. 1839. Sm. 8vo, pp. 429. 8s. 6d.

14. A Practical Treatise on the Human Teeth; showing the causes of their Destruction and the means of their Preservation. By William Robertson. With Plates. Second Edition.—London, 1839. 8vo, pp. 205. 7s. 6d.

15. An Inquiry into the Morbid Effects of Deficiency of Food, chiefly with reference to their occurrence amongst the destitute poor; also, Practical Observations on the Treatment of such cases. By R. B. Howard, M.D., Physician to the Ardwick and Ancoat Dispensaries, Manchester.—London, 1839. 8vo, pp. 77.

16. Stammering practically Considered; with the Treatment in detail. By T. Bartlett, Assist. Surg. King's Own Light Infantry.—Lond. 1839. 12mo, pp. 83. 5s. 6d.

17. The Animal's Friend; or, the Progress of Humanity. No. VII. 8vo, pp. 72.—London, 1839.

18. Essays, Anatomical, Zoological, and Miscellaneous. Reprinted. By Arthur Jacob, M.D. F.R.S.A., Professor of Anatomy and Surgery in the Royal Coll. of Surgeons in Ireland.—Dubl. (no date). 8vo, pp. 126.

19. The Hunterian Oration for 1839. By Edward Stanley, Esq. F.R.S.—London, 1839. 8vo, pp. 41.

20. Nineteenth Annual Report of the Dundee Lunatic Asylum. By P. Nimmo, M.D., and A. Mackintosh, Surgeon.— Dundee, 1839. 8vo, pp. 42.

21. Inaugural Dissertation on the Physiological Inferences to be deduced from the Structure of the Nervous System in the Invertebrated Classes of Animals. (Prize Thesis.) By W. B. Carpenter.— Edinburgh, 1839, 8vo, pp. 83. 1s. 6d.

22. Affinities of Plants; with some Observations on Progressive Development. By Thomas Baskerville, Surgeon.—Lond. 1839. Small 8vo, pp. 114.

23. Retrospect of the Progress of Surgical Literature, for the year 1838-9. By Messrs. Newnham, Wickham, and Salter. Read before the Southern Branch of the Provincial Medical Association.—London, 1839. 8vo, pp. 47. 2s.

24. Observations on the Disorder of the General Health of Females, termed Chlorosis; showing the true cause to be entirely independent of Peculiarities of Sex. By Samuel Fox, Surgeon. — London, 1839. 8vo, pp. 30. 6s.

25. Cure of Club-foot, Bent-knee, Wry-neck, Spinal and other Deformities. With Remarks on the late progress of Art, and on the necessity of a Public Institution for the relief of the Poor labouring under Deformities. By Gustav Krauss, M.D.—London, 1839. 8vo.

26. Outlines of a Course of Lectures on Chemistry, delivered before the British Residents at St. Petersburg, in 1837. By George Lefevre, M.D., &c. (Not published.)—Petersburg, 1838. 8vo, pp. 152.

27. A Lecture, introductory to the Course of Instruction in the Medical Institution of Yale College, delivered Nov. 2, 1838. By Jonathan Knight, M.D., Professor of Surgery.—New Haven, 1838. 8vo, pp. 27.

28. The Annual Address to the Candidates for Degrees and Licences in Yale College, Feb. 26, 1839. By Thos. Miner, M.D.—New Haven, 1839. 8vo, pp. 20.

29. Medical Lexicon; a new Dictionary of Medical Science, containing a concise account of the various Subjects and Terms, &c. Second Edition, with numerous modifications and Additions. By Robley Dunglison, M.D. M.A.P.S., &c.—Philadelphia, 1839. Roy. 8vo, pp. 821.

30. Practical Surgery. By Robert Liston, Surgeon. With Notes and additional Illustrations. By G. W. Norris, M.D., one of the Surgeons of the Pennsylvania Hospital.—Phil., 1838. 8vo, pp. 274.

31. Medical and Physiological Problems; being chiefly researches for Correct Principles of Treatment in disputed points of practice. By W. and D. Griffin, M.D.—Limerick, 1839. 8vo, pp. 114.

32. Narrative of the Discoveries of Sir Charles Bell in the Nervous System. By Alex. Shaw, Assist. Surg. to the Middlesex Hospital.—Lond. 1839. 8vo, pp. 252.

33. An Essay on the Functions of Life, comprising demonstrations of a Supreme Inceptive Function in the Organism of Man, New Views, &c. By W. Batten, Esq., Surgeon.—Lond. 1839. 8vo, pp. 36.

34. Documents and Dates of Modern Discoveries in the Nervous System.—London, 1839. 8vo, 5s.

35. A System of Practical Surgery, with numerous Explanatory Plates; the Drawings after nature. By John Lizars, Professor of Surgery to the Royal College of Surgeons, Edinburgh. Part II.—Edinburgh, 1839. 8vo, pp. 333. 24 Plates.

FOREIGN.

1. Das schräg verengte Becken nebst einem Anhange über die wichtigsten Fehler des weiblichen Beckens überhaupt. Von Dr. F. C. Naegele, Professor der Medicin, &c. zu Heidelberg. Mit 16 Tafeln. Mainz, 1839. Folio, pp. 118.

2. Traité des Maladies des Reins, et des Altérations de la Secrétion Urinaire, &c. Par P. Rayer, M.D., &c. Tome I. avec six Planches.—Paris, 1839. 8vo. pp. 638.

3. Die Ermordung der Anna Catharina Höge nach actenmässigen Berichten. Von P. Schmidt, M.D.—Hamb. 1837. 8vo. pp. 212.

4. Die Geburtshülfliche Auscultation. Von Dr. H. F. Naegele.—Mainz, 1838. 8vo. pp. 140.

5. Dissertatio inauguralis de Nervorum regeneratione. Auctore C. O. Steinbrück. (Accedunt duæ Tabulæ.)—Berolini, 1838. 4to. pp. 80.

6. Naschrift op het werkje, over de overeenkomst en het verschil tusschen de Jicht en de Scrophulosis, vooral met betrekking tot de Longtering; door A. A. Sebastian, M.D., &c.—Groningen, 1838. 8vo, pp. 96.

7. De Craniis Esthonum Commentatio Anthropologica. Auctore Alex. Hueck, M.D.—Dorpati Livonorum, 1838. Folio, pp. 16.

8. De Fungo Genu necnon de Tuberculis in hoc Morbo inventis, Dissertatio inaug. quam conscripsit F. J. Lederle.—Petropoli, 1838. 8vo, pp. 81.

9. Viaggio Medico in Germania nella state del 1837. Par B. Bertini, Consigliere del Collegio di Medicina, &c.—Torino, 1838. 8vo, pp. 174.

10. Statistica Nosologica dal 1821 al 1833, e Rendiconto Medico per il 1834, del Venerando Spedale Maggiore, &c. Per Bernardino Bertini, M.D., &c.—Torino, 1835. 8vo, pp. 92.

11. Archivio di Medicina pratica universale compilato dal Dottore A. B. M. Schina, Professore, &c. Sezione Anatomico-fisiologica del Sistema Vasale. Vol. I. II.—Torino, 1836. 8vo, pp. 240, 333.

12. Della Medicina Idiopatica in Germania. Par B. Bertini, M.D.—Torino, 1838. 8vo, pp. 23.

13. Etudes Cliniques sur divers Points de l'Histoire des Fièvres bilieuses et typhoides. Par le Docteur H. C. Lombard, Médecin de l'Hôpital de Genève.—Paris, 1839. 8vo, pp. 40.

14. Lettre sur la Vaccine et les secondes Vaccinations. Par le Dr. H. C. Lombard.—Genève, 1839. 8vo, pp. 50.

15. Memorie della Società Medico-chirurgica di Bologna. Vol. I. Fasc. III. IV.—Bologna, 1837, 1838.

16. Bericht über die Einrichtung und die Ergebnisse der chirurgischen-ophthalmologischen Clinik zu Freiburg während der leztverflossenen neun Jahre, unter der Leitung des verstorbenen Geheimen Hofrathes Dr. Beck. Herausgegeben von Professor Dr. Schwörer.—Freiburg, 1838. 4to, pp. 84.

17. Mémoire sur l'Hémorrhagie des Méninges. Par E. Boudet.—Paris, 1839. 8vo, pp. 64.

INDEX TO VOL. VIII.

OF THE

BRITISH AND FOREIGN MEDICAL REVIEW.

PAGE

- Abdomen, auscultatory phenomena in 461
- Abortion, with retained ovum . 44
- Abscess, ileo-cœcal . . . 279
- Absorption 311
- Ague, Dr. Kremer's theory of . 67
- Air-douche in diseases of the ear . 94
- Alcohol in the brain . . . 539
- Albers, Dr. on fatty disease of the liver 554
- Albumen, its relations to urine . 142
- Albuminuria, M. Colon on . 121
  - causes of . . 137
  - uncertain diagnosis of 249
- Alderson, Dr. on colour . . 593
- Alison, Professor, his physiology 241
- Alum in diseases of the heart . 555
- Amputation, a new mode of . 205
  - spontaneous, case of 567
- Anatomy, Wistar's treatise on . 244
  - Mr. Wilson's practical . 240
- Aneurism, treatises on . . 413
  - thoracic and abdominal 464
- Animals, on cruelty to . . 536
- Antimony, action of . . . 251
  - sulphuret of, effects of . 556
- Anus, artificial 256
- Aphonia, cured by ammonia . 252
- Apoplexy, cutaneous . . 552
- Arnold, Professors, their physiology 476
- Arsenic, minimum fatal dose of . 568
  - new process for detecting 373
- Arteries, surgical anatomy of . 538
  - on the torsion of . . 559
  - structure and physiology of 171
- Arteritis, Dr. Bonetti on .. 556
- Asphyxia in infants . . 474
- Asylum for the insane at Frankford 583
- Atavism 487
- Attention, effects of, on the body . 492
- Auscultation, obstetric, Naegele on 365
- Barlow, Dr. his address . . 604
- Barlow, Dr. on injury of the wrist 588
- Beaumont, Mr. on polypi . 237
- Beck, Dr. his medical jurisprudence 246
- Blasius, Dr. on amputating . 205
  - on aneurism . 413
- Bleeding in brain affections . 489
- Blood-vessels in smallpox . 247
- Blood, M. Magendie on . . 341
  - experiments on . 270, 345
  - state of, after death . 272
  - its vital properties . 177
  - viscosity of . . 323
  - effects of medicine on . 355

PAGE

- Blood, state of in plague . 551
- Blood-fever . . . 441
- Blushing, physiology of . . 245
- Bona, in Africa, fevers at . . 57
- Bones, the episternal . . 248
- Bone formed from periosteum . 285
- Books received for review 307, 617
- Botany, Mr. Lindley's school . 538
- Bouvier, M. on clubfoot . . 384
- Bougies, ivory . . . 258
- Bowring, Dr. on plague and quarantine 233
- Brain, ulceration of . . 275
- Brain of the negro, Tiedemann on 374
  - weight of . . 375, 377
  - alcohol in . . 539
  - on bleeding in affections of the 489
- Bramley, Mr. on bronchocele . 103
- Breschet on the ear . . 68
- British Association for Science . 616
- Broken head, virtues of a . 557
- Bronchocele, its geological relations 106-9
  - its treatment . 119
  - treatises on . . 103
- Buffy coat, nature of . . 347
- Burgess, Dr. on blushing . 245
- Buzorini, Dr. on typhus . 428
- Cahill, Dr. on delirium tremens . 595
- Calculus, pulmonary, large . 279
- Calcutta, medical statistics of . 592
- Calor mordax, its cause . . 445
- Capillaries, physiology of . 175
- Carpenter, Dr. on the nervous system 506
- Ceely, Mr. on vaccination . 614
- Cerebellum, on the functions of . 576
- Children, five at a birth . 564
- Chlorosis, Mr. Fox on . . 542
  - with menorrhagia . 250
  - Blaud's pills in . 252
- Christison, Dr. on disease of kidneys 121
- Churchill, Dr. on version . 597
- Chyle, its progress into blood . 479
- Circocele, operation for . . 558
- Circulation, pulmonary . . 329
- Claubry, M. on typhus fever . 428
- Clanny, Dr. on Wourali poison . 597
- Club foot, treatises on . . 384
  - causes of . . 388
  - operation for . . 391
- Cod-liver oil in scrofula . . 554
- Colchicum, on the use of . . 499
- Colon, on the pathology of . 501
- Colour of animals, certain causes of 593
- Colchicum, external use of . 280

PAGE
Coudret, M. on animal electricity 406
Congestion of blood, nature of . 182
Consumption in West Indies . 219
Contagion, mode of propagating 315
Convulsions, singular case of . 595
Cooper, Sir A. anecdote of . 535
Copaiba, rheumatism from . 280
Cowpox, identity of, with smallpox 614
Craig, Mr. on protracted labours . 467
Cupro-ammoniacal liquor . 353
Cutaneous diseases, Dr. Willis on . . . 239-541
Davidson, Mr. on the perinæum . 284
Davies, Mr. on iodine . . 512
Davy, Sir H. his Life . . 545
  Dr. on the blood and respiration 269
    his life of Sir H. Davy . 545
De Candolle, M. his organography 243
Deleau, M. on the ear . . 68
Delirium tremens, on opium in . 593
Determination of blood . . 181
Dewar, M. on convulsive diseases 595
Dickson, Dr. his unity of disease 531
Dierbach, Dr. on materia medica . 353
Digestion, experiments on . 478
Diseases, on the connexion of . 501
Druitt, Mr. his vade mecum . 536
Dunglison, Dr. his medical library 539
  on the temp. of the uterus 591
  his medical lexicon . 548
  on the fœtal heart . 581
Dunn, Mr. on dislocation of femur 286
Dysmenorrhœa, Dr. Meigs on . 40
Ear, treatises on . . 68
Education, medical . . 287
Elasticity . . . 326
Electricity, animal . . 406
  its relation to fever . 445
Electromotor, a new medical agent 409
Ellis, Mr. on hernia . . 596
Endosmosis and exosmosis . 317
Exploration, uterine, importance of 370
Eustachian tube, anatomy of . 75
  catheterism of . 93
Evidence, medical, nature of . 483
Evolution, spontaneous, case of . 567
Excito-motory system . . 508
Exhalation . . . 317
Eye, anatomy of . . . 71
  physiology of . . 82
  diseases of . . . 89
Eye, development of hair in . 256
  Morgan on diseases of . 329
Face-presentation, causes of . 258
Family diseases not hereditary . 287
Farcy in the human subject . 552
Femur, congenital dislocation of . 559
Fever, typhus, treatises on . 428
  use of wine in . . 281
  intermittent, ligature in . 251
    narcotine in 263
  irritative, from hot liquids . 277
  yellow, its fatality . 214

PAGE
Finger, permanent flexure of . 401
Flood, Dr. on the arteries . . 538
Fœtus, on the pulsations in . 581
  origin of hernia in . 594
Food, Dr. Holland, on deficiency of 524
  deficiency of, symptoms of . 525
    treatment of . 526
Fourchette, laceration of . . 38
Fox, Mr. on chlorosis . . 542
Fractures, statistics of . . 592
Galvanic midwifery forceps . 666
Gangrene, from tight bandaging . 558
Gases permeate tissues . . 322
Gastrodynia, treatment of . . 358
Gonorrhœa, secondary symptoms of . . . 426
Gout and scrofula, relation of . 242
  nature of . 497
Groin, surgical anatomy of . . 542
Guerin, on section of spinal muscles 557
Hair developed in the eye . . 256
Halliday, on West India diseases . 211
Hanging, a new sign of . . 572
Heart, state of, in fever . . 281
  wound of . . . 255
  Dr. Hope on diseases of . 448
  natural sounds of . . 448
  on alum in diseases of . 555
Heart, fœtal, on the pulsations of . 581
Henderson, Mr. on sea-scurvy . 595
Hepatitis, mercurial inunction in . 490
Hernia, on trusses in . . 561
Hereditary disease . . . 484
Hermaphrodism, M. St. Hilaire on 1
Hernia, Dr. O'Beirne on . . 596
Hilaire, St. M. on monstrosities . 1
Hemorrhage, uterine, treatment . 475
Holland, Dr. his medical notes . 481
  Dr. B. on food . 524
Homœopathic doctrines . . 427
Hope, Dr. on diseases of the heart . 448
Hope, Dr. on the spleen . . 598
Hosack, Dr. on tumours of the urethra . . . 585
Hunter, John, on inflammation . 169
  animal magnetism 493
Hydriodate of potash, preparation of 597
Hydrocephalus, acute, mercury . 426
  on compression in . 560
Hydrocyanic acid in gastrodynia . 358
Hydrostatics, vital . . 528
Imbibition, physiological . . 311
Inflammation, Hunter's theory of . 185
  Macartney's ditto 184, 186
  Dr. Carswell's ditto . 189
  Mr. Davies on . 573
  anatomical characters of 193
  treatment of . . 198
  treatises on . . 169
  Magendie on . 339
Influenza, an epidemic . . 502
Inglis, Dr. on bronchocele . 103
Iris, on the physiology of . . 580

PAGE
Insanity, American treatment of 583
Insect life, as a cause of disease 506
Intermittence, nature of 505
Intermittent inflammations 421
fever, treatises on 56
Introsusception, mercurius vivus in 422
Iodine, Mr. Davies on the use of, 512-18
diseases in which it is useful 521
its use in herpes 591
Iron, sesquioxyde, preparation of 360
antidote of arsenic 573
Irrigation in inflammation 200
Jackson, Dr. on typhus fever 428
Jahn, Dr. on practical medicine 419
Jaw, lower, peculiar disease of 423
Jeffreys, Dr. his address 605
Jones, Mr. on the diseases of women 529
Jurisprudence, medical, elements of 246
Kennedy, Dr. on prolapsus uteri 596
Kidneys, treatises on diseases of 121
table of diseases of 125
Knee-joint, contracted, operation for 400
Köchlin, Dr. on metallic medicines 353
Labours, protracted, treatise on 467
treatment of 471
Laycock, Mr. on colchicum 280
Lee, Dr. his physiology for schools 533
Lens, on the reproduction of 247
Leucorrhœa, Dr. Meigs on 41
Life, physical phenomena of 309
Liquor amnii, morbid accumulation of 564
Lime, on the use of 359
Lincke, Dr. on the ear 68
Lindley, Mr. his school botany 538
Little, Dr. on club-foot 384
Liver, on fatty disease of 554
Lombard, Dr. on typhus 428
Longevity in Prussia 575
Löwenhardt, L. on practical medicine 419
Macaulay, on cruelty to animals 536
M'Clelland, Mr. on bronchocele 103
Maddock, Dr. on copaibal rheumatism 280
Magendie, M. on life 309
Magnetism, effects of 481
animal 494
Maillot, M. on intermittent fevers 56
Malformation, case of 566
Malgaigne, M. on hernia 561
Marshall, Mr. on soldiers 537
Martin, M. on pregnancy 37, 50
Materia medica, treatises on 353
Maunsell, Dr. on political medicine 547
May, Dr. on ulceration of the brain 275
Membrana tympani, anatomy of 73
Meigs, Dr. his treatise on midwifery 37
Menstruation 39
theory of 530
Mercurial inunctions, large, use of 419
Metals, their medical use 353
Metallic tinkling 451
Middle ear, diseases of 99
Midwifery, treatises on 37, 50, 467

PAGE
Mitscherlich, Dr. on materia medica 353
Mollusca, on the nervous system of 507
Monsters, different kinds of 29
Monstrosities, St. Hilaire on 1
classification of 4, 18
general laws of 4, 7, 8, 11
Montault, M. on typhus 428
Moon, its influence on disease 481
Morphia, bimeconate of 275
Morgan, Mr. on diseases of the eye 229
Mortality in Prussia 578
Morton, on the perineum 244
on the groin 542
Naegele on obstetric auscultation 365
Narcotine in intermittent fever 263
Nasmyth on the teeth 159
Negro, comparative size of brain of 374
Nephritis, varieties of 150
diagnosis of 152
treatment of 156
Nerves, functions of the eighth pair 264
Nerves, on the lesion of ganglionic 594
Nervous system, anatomy of 226
Nervous system of the invertebrata 506
Newnham, Mr. his surgical report 546
Nicholls, Mr. on hydriodate of potass 597
Notes and reflections, Dr. Holland's 482
Nutrition, physiology of 549
O'Beirne, Dr. on hernia 598
Old age, medical treatment of 503
Ophthalmia, conium in 256
Opium, its use in fever 439
Organography, vegetable 243
O'Shaughnessy, Dr. on narcotine 263
Otitis 91
Otto, Dr. on cutaneous apoplexy 552
Paget, Mr. on blood-vessels in tendons 594
Palsy, treatment of 490
Pathology, Dr. Arnold's 476
Patient, when he may judge for himself 495
Pelvis, diameter of 37
Percy, Dr. on alcohol in the brain 539
Pericarditis 463
Pereira, Mr. his Materia Medica 353
Perineum, surgical anatomy of 244
laceration of 284
Periosteum, forms bone 245
Perspiration, effects of 491
Permeability of tissues, physiological 322
Phillips, Mr. on prolapsus uteri 596
on division of tendons ib.
Pilcher, Mr. on the ear 68
Phthisis, given to animals 249
Physiology, Dr. Arnold's 476
Dr. Alison's 241
Physiology for schools 533
Pirondi, Dr. his travels in England 534
Placenta, natural position of 371
Plague, observations on 233
morbid anatomy of 550
Poisons, mode of operation of 314
Poisoning by sulpho-cyanic acid 258

PAGE

- Political medicine, Dr. Maunsell on 547
- Polypi, ligature of . . 237
- Porosity, physiological . . 311
- Pregnancy, diseases complicating . 50
- Prolapsus uteri, new treatment of 596
- Provincial Med. Association . 303, 603
- Puerperal fever . . 49
- Pulse, variability of in the infant . 367
  - in diseases of the heart 456-466
- Purgatives, on the abuse of . 496
- Quackery, report on . . 610
- Quarantines, Dr. Bowring on . 233
- Rash, the typhoid . . 432-436
- Rayer, M. on the kidneys . 121
- Reflections and Notes, Dr. Holland's 482
- Reform, medical . . . 287
  - report on . . 612
  - petition for . . 610
- Registration of births and deaths . 532
- Reid, Dr. on the eighth pair of nerves 264
  - on the ganglionic nerves 594
- Revaccination in the Prussian army 261
  - Mr. Newnham on . 546
- Roupell, Dr. on typhus . . 428
- Rumsey, Dr. on irritative fever . 277
- Sawkins, Mr. on hernia . . 596
- Scrofula and gout, relation of . 242
- Scurvy, Mr. Henderson on . 595
- Sebastian, Dr. on gout and scrofula 242
- Simon, Dr. on nutrition . . 549
- Simpson, Dr. on fœtal pathology . 594
- Smallpox, anatomy of . . 247
  - identity of with cowpox 614
- Snake, living swallowed . . 257
- Societies, medical, English . 535
- Soldiers, on enlisting . . 537
- Solon, M. on albuminuria . 121
- Somervail, Dr. on tobacco . 582
- Speculum, Mr. Jones on the . 529
- Spinal marrow, respiratory tract of 480
- Spinal muscles, section of . . 557
- Spine, tenderness of, in ague . 67
  - supposed diseases of . 500
- Spleen, rupture of . . 248
  - structure and functions of . 593
- Squire, Mr. on morphia . . 275
- Stillborn children, restoration of 596
- Stokes, Dr. on wine in fever . 281
- Stromeyer, Dr. on orthopœdia . 384
- Sudorific medicines . . 490

PAGE

- Surgical literature, report of . 546
- Swan, Mr. on the nervous system 226
- Syme, Mr. on the formation of bone 285
- Teeth, treatises on the structure of 158
  - development of . . 165
- Tendons, on the division of . 384
- Tendon achilles, section of the . 385
- Tendons, on the circulation in . 594
  - hamstring section of . 596
- Teratology, St. Hilaire on . 1
- Terry, Mr. on suspended animation 596
- Tobacco, on the external use of . 582
- Todd, Dr. on the ear . . 68
- Tracheotomy, statistics of . 558
- Trismus in Iceland . . 486
- Troops, British, on the health of . 211
- Tulloch, Major, on the army . ib.
- Twins united, case of . . 568
- Tympanum, anatomy of . . 76
- Typhus fever, treatises on . . 428
  - contagiousness of . 430
  - treatment of 438-441
- Typhoseptoses, a family of diseases 442
- Unity of disease . . 531
- University of London, regulations of 287
  - examiners in 304
- Urethra, female, on tumours in . 585
- Urine, chemistry and pathology of 126
  - critical . . . 157
- Vaccination, experiments in . 614
  - petition respecting . 613
- Vade mecum, Mr. Druitt's . 536
- Vagina, muscularity of . . 39
- Vagina and uterus, temperature of 591
- Valves, diseased, without murmur . 279
- Valves, diagnosis of . . 452
- Varicocele, case of . . 253
- Veins, wounds in . . 460
- Veratrine, researches on . 353
  - effects of . . 363
- Version, obstetrical, statistics of . 597
- Wardrop, Dr. on aneurism . 413
- Waterton, Mr. on Wourali poison 597
- Willis, Dr. on cutaneous diseases 239-541
- Wilson, Mr. his surgical anatomy . 240
- Women, on the diseases of . 529
- Wormald, his Anatomical Sketches . 542
- Wourali poison, experiments with . 597
- Wrist, peculiar injury of . . 588
- Wry-neck, case of . . 402

END OF VOL. VIII.

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THE BRITISH AND FOREIGN MEDICAL REVIEW.

NO. XVI—OCTOBER, 1839.

PART FIRST.—Analytical and Critical Reviews.

1. MAGENDIE'S Lectures on the Physical Phenomena of Life . . . [illegible]
2. MITSCHERLICH, PEREIRA, DIERBACH, [illegible], [illegible] on Materia Medica [illegible]
3. NAEGELE on Obstetric Auscultation . . . 367
4. TIEDEMANN on the Brain of the Negro and European . . . 371
5. STROMEYER, ZEIS, BOUVIER, LITTLE on Club-foot, and the Section of Contracted Muscles and Tendons . . . [illegible]
6. [illegible] on Animal Electricity . . . 400
7. WARDROP and BLASIUS on Aneurism . . . 413
8. LÖWENHARDT and JAHN'S Practical Essays . . . 419
9. [illegible] & MONTAULT, JACKSON, ROUPELL, &c. on Typhus Fever . . . 428
10. HOPE on Diseases of the Heart . . . 448
11. CRAIG on Protracted Labour, Uterine Hemorrhage, &c. . . . 467
12. F. and W. ARNOLD'S Physiology and Pathology . . . 476
13. H. HOLLAND'S Medical Notes and Reflections . . . 492
14. CARPENTER, on the Physiology of the Nervous System . . . 500
15. DAVIES'S Selections in Pathology and Surgery . . . 512
16. B. HOLLAND on the Effects of Deficiency of Food . . . 524

PART SECOND.—Bibliographical Notices.

1. JONES on Diseases of Women . . . 528
2. DICKSON on the Unity of Disease . . . [illegible]
3. The Registrar General's First Annual Report . . . [illegible]
4. LEE'S Human Physiology . . . [illegible]
5. [illegible]'S Six Months' Residence in England . . . [illegible]
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11. DUNGLISON'S American Medical Library and Intelligencer . . . [illegible]
12. PERCY'S Experiments with Alcohol . . . ib.
13. WILLIS'S Illustrations of Cutaneous Disease . . . 541
14. WORMALD and M'WHINNIE'S Anatomical Sketches—MORTON'S Surgical Anatomy of the Groin . . . 542
15. FOX on Chlorosis . . . ib.
16. DAVY'S Memoirs of Sir Humphry Davy . . . [illegible]
17. NEWNHAM, WICKHAM, and SALTER on Surgical Literature . . . 546
18. [illegible]'S Political Medicine . . . 547
19. DUNGLISON'S Medical Lexicon . . . 548

PART THIRD.—Selections from the British, American, Colonial, and Foreign Journals.

ANATOMY AND PHYSIOLOGY, 10 Articles; PATHOLOGY, PRACTICAL MEDICINE AND THERAPEUTICS, 17 Articles; SURGERY, 11 Articles; MIDWIFERY, 10 Articles; MEDICAL JURISPRUDENCE AND TOXICOLOGY, 4 Articles; MEDICAL STATISTICS, 2 Articles; MATERIA MEDICA, 1 Article; TOXICOLOGY, 1 Article.

PART FOURTH.—Medical Intelligence.

Provincial Medical Association . . . 603
British Association for the Advancement of Science . . . 616
BOOKS RECEIVED FOR REVIEW . . . 617
INDEX, TITLE, &c.

No. XVII. WILL BE PUBLISHED ON THE 1st OF JANUARY, 1840.

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